Bourdieu the Ethnographer

The Ethnographic Grounding of Habitus and Voice¹

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Abstract. A remarkable feature of the way in which Bourdieu’s work has been adopted in studies of language in society is the emphasis on Bourdieu as a macro-sociologist providing insights into the larger processes of structuration in tightly integrated First-World societies. Yet, Bourdieu himself consistently emphasized the ethnographic epistemological foundations of his work, and especially in the last years of his life abundantly acknowledged influences from ethnography. This paper delves into Bourdieu’s views on ethnography-as-epistemology, arguing that one of Bourdieu’s central concepts, habitus, should be seen as inextricably linked to situated ethnographic inquiry. Taking habitus as an ethnographic concept, we may find better ways of investigating problems of voice – the conditions for speaking in society. This point is illustrated with examples from the Belgian asylum procedure, where habituated conversational practices by the interviewer simultaneously appear to contain proleptic moves that ‘prepare’ the story of the applicant for the next step in the asylum procedure. We see in this form of simultaneity the on-the-spot, layered deployment of macro-social (institutional) conventions through conversational, co-operative practices.

The concerned was interrogated on November 23, 1993 at the [High Commissariat for Refugees and Stateless Persons] in the presence of

¹ I thank Moira Inghilleri for gently twisting my arm to write this article, thus allowing me to revise and expand the short essay on ‘Bourdieu the ethnographer’ which I had written as the introductory paper for a panel on ‘Bourdieu and Ethnography’ (International Pragmatics Conference, Toronto, July 2003). Marco Jacquemet, Jane Hill, Chris Stroud, Adrian Blackledge, Stef Slembrouck, Jim Collins and Moira herself all commented perceptively on that essay. I wish to specially thank katrijn Maryns, with whom I have worked for several years on these issues, and whose wonderful dissertation (Maryns 2004) provided me with further inspiration for this article. Just as this paper went into production, a special issue of Ethnography came out devoted to Bourdieu and fieldwork. Although I could not completely incorporate that material in this paper, I recommend Loic Wacquant’s introductory chapter (Wacquant 2005): a fascinating and insightful reflection on Bourdieu’s basis in fieldwork, largely corroborating the analysis presented here.
[name], his attorney.

He claimed to be a ‘political informant’ of the MPLA. On October 18, 1992 however, he passed on information to UNITA. At the UNITA office, however, he met with Major [name], who works for the MPLA. Two days later, Major [name] had the concerned arrested. Fearing that the concerned would give the Major away at the trial, [name of the Major] helped the concerned to escape. The concerned fled to [locality] where a priest arranged for his departure from Angola. The concerned came, together with his wife [name and register number] and three children, through Zaïre and by plane, to Belgium. They arrived on May 19, 1993.

It has to be noted that the concerned remains very vague at certain points. Thus he is unable to provide details about the precise content of his job as ‘political informant’. Furthermore the account of his escape lacks credibility. Thus it is unlikely that the concerned could steal military clothes and weapons without being noticed and that he could consequently climb over the prison wall.

It is also unlikely that the concerned and his wife could pass the passport control at Zaventem [i.e., Brussels Airport] bearing a passport lacking their names and their pictures. Furthermore, the itinerary of the concerned is impossible to verify due to a lack of travel documents (the concerned sent back the passports).

The statements of the concerned contain contradictions when compared to his wife’s account. Thus he declares that the passports which they received from the priest [name] were already completely in order at the time they left Angola. His wife claims that they still had to apply for visa in Zaïre.

This is the text of an official letter sent by the Belgian asylum authorities to a refugee from Angola (the original text was in Dutch bureaucratese; the version above is my own translation). Such letters are part of the asylum application process, which in itself usually involves three stages: (1) an initial interview on the causes and reasons for seeking asylum and on the ‘facts’ of the case, conducted by the Immigration Office (Dienst Vreemdelingenzaken); (2) in case of a negative initial verdict, a second ‘deeper’ interview conducted by the High Commissariat for Refugees and Stateless Persons (Hoog Commissariaat voor Vluchtelingen en Statenlozen); and (3) in case of a second negative verdict, a hearing before the Board of Appeal of the Supreme Court (Raad van State) (Maryns 2004). This particular letter comes from the second stage of the process. The High Commissariat informs the man about the rejection of his asylum application and provides reasons for this rejection.

As a text-artefact, the letter represents an advanced stage in a long cycle
of entextualizations of ‘the story of the applicant’ – a text trajectory which contains numerous interventions in the story by various actors: summaries, translations, reformulations, interpretations, evaluations (see Silverstein and Urban 1996; also Sarangi and Slembrouck 1996). The text trajectories are, as a rule, multilingual – applicants from other countries tell their stories to Belgian asylum officials who are normally native speakers of either Dutch or French, sometimes in uneasy varieties of French or English, sometimes in what is claimed (or believed) to be their native language with the assistance of an interpreter – causing, as can be imagined, nightmarish communicative events. Rejection of applications is the rule as well. Belgium has the highest percentage of rejected asylum applications in the whole of the EU, and an overwhelming number of these rejections are based on assessments of the ‘quality’ of the applicant’s story. They are, in other words, based on elements gathered during the text trajectory sketched above (Maryns 2004, Blommaert 2001a, 2001b, 2005, Inghilleri 2003).

Who speaks here? Whose voice do we hear? Surely there is a stentorian bureaucratic voice; but that voice has completely absorbed and appropriated the voice of the applicant. In fact, most of what we read in the text is a summarized version of the applicant’s story, framed in a metapragmatic evaluative grid that casts doubt on or disqualifies episodes, fragments or the whole of the story. That is, it casts doubt on or disqualifies utterances made by the applicant at one point. What we read in the text is the end product of a sequence of entextualizations of an orally produced, performed narrative, which we know was produced in a particular language, with a particular rhythm, prosody and intonation, generically formatted into different episodes, plots, side-plots and sub-plots, accompanied by gesture and facial expression, and in response to prompts given by an interlocutor (whose utterances are also orally produced and performed in the sense given here). The interaction itself is a highly specific, genred event in which the conditions and expectations are not necessarily equally well understood by both parties (Maryns 2004).

The text does everything to elide these performance features (and does a remarkable job, to be sure); but it is good to remember that intertextuality here is not an abstract transfer of textual substance but a process that has to deal with performed, situated narrative. This, as mentioned above, raises issues of voice. The transformations of the story entail changes in the conditions for articulating subjectivity. We see in the transfer from performed narrative to bureaucratic text-artefact a transfer of one form of subjectivity into another: from a situated, conditioned and contextualized subjectivity articulated in performed narrative, to a decontextualized ‘pure’ subjectivity which ascribes the profoundly manipulated final and evaluated version of the story to the applicant himself, as in, “the concerned remains vague”, “the statements of the concerned contain contradictions”. This is subjectification in the sense of Foucault (2003); the subject as a product of power, as an
effect of evaluative statements by others on one’s (auto-)biography. The statements of the applicant were made under strict conditions of power, during a genred event known as the application interview. Their subsequent entextualizations were also produced within a particular regime of power in which ‘the story of the applicant’ was scanned for contradictions, unlikely episodes, incoherent bits or passages lacking detail and precision. But when this work is over, the story is re-attributed to the applicant: it has become ‘his own’ story again (see also Barsky 1994, Briggs 1997). So, who speaks? According to the bureaucratic voice, it is the applicant who has spoken, but not well enough.

In what follows, I intend to elaborate on the tension between subjectivity and situatedness – a tension which is crucial to any understanding of voice. Voice here refers to the capacity to make oneself understood as a situated subject. It is a matter of ‘brought abouts’ and ‘brought alongs’, of perduring conditions for articulative performance as well as of on-the-spot deployment of resources and means. And I will argue that Bourdieu’s concept of habitus can be useful in interpreting the tension, provided we see habitus as ethnographically grounded, i.e., as allowing for the situated, performed subjectivities we address here. Recall that Bourdieu used habitus in a variety of (situated) formulations; the shorthand definition I shall use in the remainder of this paper is this (Bourdieu 1990a:54):

> the structures characterizing a determinate class of conditions of existence produce the structures of the habitus, which in their turn are the basis of the perception and appreciation of all subsequent experiences. The habitus, product of history, produces individual and collective practices – more history – in accordance with the schemes generated by history.

Note the emphasis on the historical embeddedness of habitus. Bourdieu does emphasize the dimension of durability in anything he says about habitus – habitus as a system of perduring conditions for thought and action, as a sediment of structure in our agency – but he does so within a *historical*, not a timeless frame. That means he does so within a frame that allows for considerable change, even within the same synchrony since different historically grounded forms of habitus may be involved in the same event. Habitus is durable, but not static. In order to understand this important nuance, we need to turn to Bourdieu-the-ethnographer. Bourdieu used an ethnographic epis-
temology in his work, and this is what should prevent us from seeing structure (sedimented in habitus) as static.

1. Bourdieu the ethnographer

In studies on language in society, Bourdieu is often seen as a macro-sociological theorist, whose main contributions lie in the field of theorizing social action and structuration, symbolic power relations and capital, and habitus. A glance at the politics of citation of his work in mainstream pragmatics and sociolinguistics testifies to this: Bourdieu is most often referred to when concepts such as habitus and field are brought into analyses, as well as when language is defined as a symbolic commodity with exchange value (the main source for such references would be his translated collection *Language and Symbolic Power*, Bourdieu 1990b). Furthermore, one would see references to his work confined to analyses of First-World societies, some analysts of other societies effectively contesting the validity of his theoretical apparatus for non-western societies and social processes (Haeri 1997, Stroud 2002). His oeuvre is often presented as closed, bounded, finished.3

Bourdieu’s work contains more that is of crucial importance for the development of a critical science of language in society, in particular when we focus on Bourdieu-the-ethnographer. There are, I believe, at least three motives for this: (1) Bourdieu’s own interest in ethnography; (2) Bourdieu’s own use of ethnography; and (3) what I would like to call the ethnographic invitation in Bourdieu’s work. Taken together, they offer us another Pierre Bourdieu and another oeuvre, probably much closer and much more directly useful to the study of language in society.

Bourdieu’s lively interest in ethnographic work is a matter of record. In fact, he was instrumental in introducing important work from the American ethnographic tradition to a Francophone and wider interdisciplinary audience. Erving Goffman was greatly admired by Bourdieu (Bourdieu wrote an obituary on Goffman), and as one of his last publishing projects he co-edited a collection of papers by Aaron Cicourel translated into French (Cicourel 2002). He was also a founding board member of the journal *Ethnography* (founded in 2000) and contributed important papers to its first few issues. He had a keen interest in and a profound understanding of conversation analysis, ethnomethodology, Batesonian anthropologies of communication, and Batesonian ethnography as exemplified in *Naven*

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(Bateson 1958). Some of his close collaborators (for instance Loïc Wacquant and Yves Winkin) can clearly be situated within that school of ethnography.

The second and third reasons deserve some more comment, for they are more fundamental. I will first discuss the role of ethnography in Bourdieu’s own work; after that I will turn to the ethnographic invitation in his work.

The backbone of Bourdieu’s own work is ethnography, and remarkably, he is consistently explicit about this. His use of ethnographic vignettes in his works (e.g. *The Logic of Practice* or *Homo Academicus*) is more than a stylistic feature of his writing, it is a crucial ingredient of his theorizing. Throughout his work, whether discussing Algerian workers, immigrants in the Parisian *banlieue*, French academics or the French middle class, there is a sense of lived and experienced reality. Bourdieu starts from a fully ethnographic attendance to microscopic detail of human activity and works his way up to generalizable patterns of behavior and social organization. A concept such as ‘sens pratique’ is richly ethnographically grounded, and so is habitus. And even the English version of *Distinction* (one of his most sociologically-oriented works) starts with the announcement that “it can be read as a sort of *ethnography of France*” (Bourdieu 1986: xi, emphasis added).

Let me elaborate on this, using two particularly illuminating texts: the paper on ‘Making the economic habitus’ (Bourdieu 2000), published in the first issue of *Ethnography*, and the preface to the 1990 Polity Press edition of *The Logic of Practice* (Bourdieu 1990a). The first one can be read as a declaration of faith in ethnography, where Bourdieu expands on the way in which he depended on ethnography for constructing theoretical concepts such as habitus. The second is a very informative intellectual autobiography.

In ‘Making the economic habitus’, Bourdieu revisits his 1960s fieldwork in Algeria. The sense of experienced reality is there in the first sentence: “I witnessed, in Algeria in the 1960s, what with hindsight appears to me to be a veritable social experiment” (Bourdieu 2000:18). A motif has been flagged: the layers of reinterpretations that tend to cover fieldwork impressions, in themselves valuable because of the deeply contextualized nature of perception and understanding in the field. Bourdieu now intends to go back to the unmitigated, raw, contextualized fieldwork observations and ‘peel off’ these layers (*ibid.*, emphasis in original):

Without repeating the details of already published analyses, and giving priority to unpublished information preserved in my fieldwork notebooks, I would like to outline briefly what appeared to me with total clarity in that quasi-laboratory situation, namely, the *mismatch* between economic dispositions fashioned in a precapitalist economy and the economic cosmos imported and imposed, oftentimes in the most brutal way, by colonization.
The purpose of the paper is theoretical, to expose the fallacies of rational actor models in economic theory and to pit it against the notion of economic habitus. Such a habitus, Bourdieu insists, is the product of particular historical conditions; the economic dispositions have a social and historical genesis. And (ibid.)

[i]t was no doubt because I found myself in a situation where I could directly observe the disarray or the distress of economic agents devoid of the dispositions tacitly demanded by an economic order that for us is entirely familiar – in which, being an embodied and therefore naturalized social structure, they appear as self-evident, necessary and universal – that I was able to conceive of the idea of statistically analysing the conditions of possibility of these historically constituted dispositions.

In this fragment we see a harmonious move from ethnography-as-direct-observation to statistical analysis. The move might puzzle hardcore ethnographers, so some clarification may be in order. I turn to the second text for that, the preface to *The Logic of Practice* (Bourdieu 1990a). Let us recall that the book not only develops the concepts of habitus and practice, but also makes crucial epistemological points about objectification and objectivity-in-subjectivity. The preface retrospectively couches this argument in an intellectual travelogue.

Bourdieu starts from an acute awareness of ‘framing’ in research. We all enter our research sites under particular sociohistorical conditions, and these have an effect on what we see, perceive and understand. Bourdieu was aware of this during his 1960s fieldwork in Algeria. The country had just passed through a traumatic war of liberation against French colonial rule, and the impact on his fieldwork was considerable, Bourdieu himself of course being French among the Algerians. There is a wonderful vignette he uses to illustrate this. He had recently looked at a photo taken in a house in Kabylia during fieldwork, and he was surprised to see how well lit the indoors image was, despite the fact that Bourdieu did not have a flash on his camera. The reason was that the roof of the house had been blown off by a French grenade during the war. For Bourdieu, this example demonstrated the way in which the conditions for seeing, observing and grasping the observed and experienced social reality were deeply influenced by historical processes determining (i.e., collapsing in) the fieldwork event. There was no way in which he, at that time and place, could not be influenced by the effects of the past war in Algeria. It affected his ‘field’, surely, but also his way of entering and cruising through that field – his identity as an ethnographer. This is where ethnography, in Bourdieu’s experience, became an epistemological issue.
In order to escape the biasing effect of these conditions, Bourdieu explored two sets of measures. First, he emphasized the importance of revisiting the same object over and over again – a point already illustrated in ‘Making the economic habitus’ – as well as of comparison (his work in Algeria was followed by ‘native ethnography’ in the Béarn and in the French academic and educational system) and expansion (including more materials than just those collected during fieldwork). Second, he turned to the kind of structuralism then advocated by Lévi-Strauss, in order to find a vantage point which enabled scientific objectivity. In doing this, he attempted like Lévi-Strauss to move from ethnography to ethnology – a search for transcontextual (or a-contextual) ‘driving principles’ in the social system observed, by focusing on correlations, contrasts and forms of systemic coherence.

Whereas the first set of measures was maintained throughout his oeuvre, the second set – the turn to structuralism – was abandoned. The main reason was again ethnographic experience. Instead of the strict, transparent and mechanical schemes of structuralism, Bourdieu had encountered paradoxes, contradictions and flexible potential in the field. Furthermore, he had experienced experience, so to speak: the fact that the distance advocated in ethnology is, in actual fieldwork conditions, overgrown with sharedness of meaning, joint understandings of ‘the logic of the game’, and so on. In other words, Bourdieu had ethnographically experienced the fact that the ethnological claim to objectivity-through-distance generated another, and a potentially more dangerous form of ethnocentrism than the one intrinsic to his own observer’s – but participating and co-constructing – role in ethnography. Bourdieu worries about the specific role of the observer, and this role is not substantially different whether one investigates Algeria, the Béarn or the Sorbonne. From that point onwards, ‘dispositions’ occur, and Bourdieu theorizes how he himself became part of the object – the objectification of subjectivity. This is also the point where he makes the shift from anthropology (or ‘ethnology’, see below) to sociology: a science in which precisely the objectification of subjectivity is central, and a science that can aspire to eventually develop a subject.

Important for our purposes here is that Bourdieu, whenever he reacts against anthropology, reacts against Lévi-Straussian ethnology and its “methodologically provoked anamnesis” (2000:24) which suggests closure and total strangeness – absence of shared understanding – between observer and observed. And he replaces it with a ‘sociology’ which aims at recovering, on the basis of an experienced logic of practice, the internal dynamics of

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4 This merging of subject and object is of course epitomized in Bourdieu’s work on academia, e.g., Bourdieu (1988); Bourdieu et al. (1994).
social systems. The formulations are remarkably similar to some current theorizing in and on ethnography, notably Hymes (1996) and Fabian (1991, 1995, 2001). To make this point clear: Bourdieu started from an inherited (Lévi-Straussian) methodology which separated ethnography from ethnology, the former being the descriptive, methodical apparatus preparing the ground for the latter, which was seen as rigorously scientific, theoretical and interpretive. Gradually, however, the distinction between ethnography and ethnology was shown to be flawed, since the ethnographic stage already invoked issues of interpretation and theory – in fact, became the ‘scientific’ stage *tout court*. It is the rejection of the Lévi-Straussian sequential-hierarchical distinction between ethnography and ethnology that creates Bourdieu’s sociology: a science that has abandoned ‘ethnological’ distancing as an epistemology and replaced it with an emphasis on the situated, experienced and practised (i.e., ‘ethnographic’) aspects of reality.

Let us now return to ‘Making the economic habitus’ and observe how Bourdieu, this so-called macro-sociologist, consistently emphasizes local, situated, ethnographic understanding in the development of the habitus concept. First, and related to his critique of Lévi-Strauss, he emphasizes the incoherence and lack of (structuralist) structure in the observed facts (2000:23; emphasis in original):

> I would need to evoke here the long series of often infinitesimal experiences which made me *feel* (*éprouver*) in sensible and concrete fashion the contingent and arbitrary character of these ordinary behaviours that we perform every day in the ordinary course of our economic activities and that we experience as the most natural things in the world…. 

In fact, this lived experience tends to obscure the “implicit philosophy of labour” (*ibid.*: 24) – the habitus (*ibid.*; emphasis in original):

> because nothing had prepared me to understand the economy, especially my own, as a *system of embodied beliefs*,

I had to learn, step by step, through ethnographic observation later corroborated by statistical analysis, the practical logic of the precapitalist economy, at the same time as I was trying as best as I could to figure out its grammar.

The ‘grammar’ here refers to the structural (but *sociologically* structural, in the sense outlined above) properties of the object. These formal properties remain important in his work. In Cicourel’s view, this qualifies Bourdieu as a ‘structuralist’, for he still reaches out towards “levels of theory and data which are removed from the moment-to-moment interactional implementation of
locally instantiated social organization” (Cicourel 1993:89). But more central to Bourdieu’s concerns, I believe, is the structuring in this “system of embodied beliefs”, a system which can only be recovered by accepting one’s own habitus as something that needs to be put up for questioning. And this, he consistently stresses, is a matter of ethnography, or to use his terms, of (reflexive) sociology.

For Bourdieu, ethnography is an epistemological issue. In its traditional, codified form it evokes frightening questions of power and unilateral interpretation; but when brought down to the level of practice (in Bourdieu’s sense) driven by habitus, it becomes a site for constructing subjective knowledge and questions about knowledge. These questions have validity for theory, for they involve questions of contextualized understanding versus universal or transcontextual patterns and models. It is by accepting ethnography as the epistemological point of departure for theoretical questions that Bourdieu can come up with theory. It is a mature position; he accepts ethnography in its fullest sense, including the inevitable quagmires of subjectivity, bias and ‘doing-as-if’ in the field, and in that sense prefigures what later came to be known as Critical Ethnography.

This brings me to my final point. Perhaps most importantly, Bourdieu’s work contains what could be called an ‘ethnographic invitation’, a call to empirically explore in micro-ethnography the structures suggested in his work, an appeal to continue thinking theoretically while we work ethnographically (see Cicourel 2003 for a discussion of this issue). There is no closure of the conceptual framework in his oeuvre; on the contrary, there is a clear suggestion that single cases, even if they don’t speak to the totality of the population or the system, can speak to theory. As he states, “I believe it is possible to enter into the singularity of an object without renouncing the ambitions of drawing out universal propositions” (Bourdieu 1986:xi).

Herein lies the true fertility of Bourdieu’s oeuvre. He not only demonstrates that ethnography need not be Kleinarbeit, but he also whips us into ethnographic inspection of what he offers in the form of theoretical models and claims – ethnography to him is the epistemological tool to arrive at theory. He does so by emphasizing over and over again the biographical, experiential ethnographic basis of his own theorizing.

2. Habitus and voice

Let us now return to our example of the asylum procedure. We see a systemic (i.e., not an incidental) mismatch between the stories produced by the applicants and the criteria for assessment used by the Belgian authorities, expressed, for instance, in the huge rejection quota. In terms of voice, it is clear that asylum seekers systemically fail to get their message across, to make themselves understood. I emphasize the systemic aspects of this phe-
nomenon for several reasons.

One reason is that we see rather obvious recurrent patterns in the text trajectories of applicants’ stories, patterns in which the authorities appear to use a particular rational, linear, detailed and ‘factually’ coherent narrative as a model for assessing the applicants’ performance. Such models are implicitly sketched in guidelines for interviewers (Maryns 2004:54-56). So the text trajectories are regulated, or to use a term more suggestive of power, they are regimented. The authorities use a particular regime of discourse in the transformation of performed narrative into ‘official’ text-artefacts. This has an effect on what we define as our object of inquiry and what kinds of phenomena we accept as evidence for our claims; this is therefore an epistemological argument. We are looking for patterns that may reveal features of the discursive regime.

Another, entailed reason is that in our analysis, we should not assume that we can focus on the details of single cases in order to solve this larger question of patterns in the text trajectories. In other words, a ‘pure’ discourse analysis in which we try to reduce the problem to ‘misunderstandings’ between applicant and interviewer will not do. Surely, situated misunderstandings (in the sense of, e.g., conversation analysis) occur; the reasons for such misunderstandings, however, are rarely purely situational and most often involve factors that surpass the single case. We are facing ‘frames within frames’, to use Goffman’s (1974) terminology, and the larger frames also matter. Such frames involve institutionality, sociolinguistics and other determining aspects of the events – aspects that define the limits of what can happen in such situated events, and that provide ‘formats’ for the single cases.

This is an analytical argument, and taken together with the epistemological one given above, we now arrive at the following formulation of the problem. What we need to investigate are the larger patterns of pre-structuring in asylum procedure cases, but we need to do this ethnographically, with an eye for the way in which larger patterns are deployed and played out during concrete steps in the procedure.

Habituated, ‘normal’ patterns of behaviour are the switchboard through which such larger patterns can be converted in practices that are being perceived as ‘good’, ‘regular’, ‘normative’, and so on. Institutional rules are converted into institutional routines, and such routines (habitually – i.e., related to the habitus) organize behaviour, experience and practices. As Inghilleri (2003) demonstrates, they also organize the discourse people use and thus provide the motives and the procedures for converting one discourse into another through interpreting, translation, summarizing, taking notes, or even conversational involvement. Let us examine this process in some detail.

Such discursive conversions, as said before, are the key to the procedure, and they happen instantaneously. The interviewers listen to the applicant’s
story and question him or her on aspects of it; the answers of the applicant are immediately converted into summaries, in Dutch or French, word-processed on a computer-generated standard form, often already incorporating legal-categorizing terminology. Also, interviewers engage in conversational interaction with the applicants by means of backchanneling cues, reformulations and clarifications, summaries, and so on.

The following example illustrates such synoptic reformulating statements (Maryns 2004:219). The interview was recorded in late 2002 in the interview rooms of the Immigration Office (Dienst Vreemdelingenzaken) in Brussels, and it involves a Flemish official interviewer (I) and a male asylum applicant from Cameroon (AS). The interview was conducted in English, and it may be useful to point out that neither of the interactants was a native speaker of that language. The applicant argues that during an incident with law enforcers, both his and his family’s identity documents were taken away. However, he fails to identify what particular documents he is talking about, and the interviewer provides a helpful gloss. This is a small negotiation – or conflict – over terminological appropriateness, and I will mark the relevant terms in this exchange in bold:

AS: … that my **documents** and inside the file that is so because my head was down but my eye ... I notify my (inaudible) and my different different **document** y-my-my **certificate** of my children...

I: **Identity documents** and (inaudible)

AS: yeah, yeah, yeah, my-my **identity documents**

The interviewer, thus, provides a helpful gloss, ending the applicant’s struggle to find the right term. But simultaneously, he provides a summary and a *register-specific gloss* for the applicant’s words (‘identity documents’), a regimented term that can be inserted in the official report of this interview.

The next example is a fragment of a Board of Appeal Hearing, involving a female judge (I) who conducts the interview with another refugee from Cameroon (AS). The court proceedings are in Dutch, and therefore an interpreter (T) is provided to translate the Dutch statements into English and vice versa. We see how an interviewer (I), through an interpreter (T) challenges the plausibility of the applicant’s (AS) claim that there are no telephones in the town where he lived (English glosses are given in italics):

AS: yeah there is no telephone in C.
I: no
T: Er is geen [there is no]
I: =geen telefoon in C . en het is onmogelijk om om het S SDF te contacteren … Ik bedoel we kunnen zo makkelijk SDF mensen contacteren email of fax … en en de SDF leden zelf kunnen dat
Here, the judge connects the statement of the applicant with general criteria of plausibility, derived from previously given information on the applicant’s involvement in an opposition movement. The applicant’s claim is ‘framed’ in terms of the plausibility criteria used in the assessment of applications in general, and this framing assumes the shape of conversational involvement – a challenge to the previous remark and a call for more elaboration and additional information from the applicant.

Such discursive conversions provide the routinized, habituated layer of ‘professional’ bureaucratic practices; they involve a reduction of a potentially infinite range of differences to a closed set of established categories, some of which may favour the applicant’s case while others may jeopardize it (Inghilleri 2003). Thus, such routinized practices are intrinsically evaluative and judgmental: they induce a particular kind of ‘understandability’ in relation to the statements of the applicant, a proleptic understandability in terms of criteria used in the next step of the procedure. They only occur during the moment of performance (the interview) because of the existence of ‘next steps’ in the procedure, the bureaucratic text-trajectory.

Two frames of historicity and views of factuality are combined in this move. As for historicity, the momentary frame of performance is maintained. As seen in the example, the interviewer often uses such converted regimented statements as conversationally relevant turns in the interview, signalling consent and understanding and often triggering affirmations from the applicant. But another historical frame is being introduced as well, that of the next steps in the procedure. This frame detaches what is being performed from its moment of occurrence as well as from the particular conditions-of-use of summarizing statements and requests for clarification during the interaction, and propels it into a different environment with different participants, codes, rules and norms. This is the moment where situated subjectivity becomes decontextualized, ‘pure’ (or purified) subjectivity – where the story no longer develops on the basis of situated understandings alone, but becomes a ‘case’ ascribed to a particular individual. And it becomes a ‘case’ through a form of simultaneity in which situated criteria for understanding are being used alongside established patterns of procedural understanding, and the latter criteria are, as a rule, only known to the interviewer.

This is where the second meeting of frames comes in. The established procedural criteria are presented and (habitually) perceived as ‘getting the
facts right’ in the ‘case’. In other words, they induce a register for talking about facts in the specific application case, and as with all registers, the act of enregistering marks a clear distinction between what belongs to the register and what does not (Silverstein 2003). This is a very concrete matter. The interviewer makes on-the-spot decisions about which parts of the performed narrative ‘belong’ to the range of factuality and which parts are redundant. Interpreting and translation are, of course, clear cases in point. But such decisions are often also interactionally flagged during the interviews, e.g., by means of the synoptic reformulations or the evaluative framings seen in the examples above. They thus take the shape of interactionally produced, supportive and collaborative expressions of conversational understanding. It is again a case of simultaneity where two acts of a fundamentally different order (here to be taken literally) are blended in one performed sequence, and where we see the process of extraction of the story to the next steps develop in the form of an interactional pragmatics.

What gets thrown out? Often, what disappears from the reformulations (and thus also from the interviewer’s report of the interview) are things that are hard to understand interactionally: situated misunderstandings, either the effect of differential linguistic competence, accent or differences in lexis due to particular varieties of the language used, or the effect of an overload of detail often confusing to the interviewer (see Blommaert 2001b for examples). That is, misunderstandings that belong to the moment of performance become absences in the next steps of the procedure; they are erased as potential ‘facts’ to be considered in this case. Names of people, objects and places, but also particular anecdotes and longish, detailed subnarratives on the situation in the home country are common victims of such erasures. For the applicant, these elements may contain crucial ‘facts’. Their interactional deployment, however, either allows or compels the interviewer to strike them from the record – that is, to strike them from ‘the story of the applicant’ that will be judged in the next steps. It is again a proleptic move, simultaneously deployed while satisfying interactional conventions or responding to the interactional dynamics in the interview.

The guidelines for interviewers emphasize that the interview should not be concluded unless the interviewer is satisfied that he or she has obtained all the facts necessary for processing the case. It should be clear, though, that the delineation and identification of facts relevant to the case is already a far-reaching intervention into the story of the applicant, and that it involves insertion of the story in a discursive regime over which the applicant has no control. The applicant has effectively lost his or her voice in the process. For our purpose here, the point is that the interviewer deploys institutional strategies in routinized performance. That is, the institutional habitus is manifest in ordinary, habituated, common-sense conversational practices of considerable versatility, which involve the applicant as well. Both collaborate
towards the performance of the narrative; but the interviewer simultaneously extracts the situated narrative to the level of ‘a case’, proleptically shaping the situated interaction in view of requirements of the next steps in the procedure. Production and evaluation proceed together, every productive conversational move being also, at once, an evaluative move which detaches the story from the situated subjectivity during the moment of performance and moves it to the level of ‘pure’ subjectivity, i.e. the subjectivity which is the product of power. There is no abstract institutional machinery involved here, no faceless technical apparatus testing and clinically observing. What we see is conversational involvement enacted in such a way that it enables institutional judgement to proceed afterwards on the basis of a particular, genred and regimented version of the story. The link between both worlds of practice is the professional habitus, the embodied ‘normalcy’ of institutional interactions, with its particular rhythm, pace, turn-taking patterns, tone and key (Inghilleri 2003, Slembrouck 2004). It is the particular form of habitus deployed here which accomplishes and regulates the on-the-spot meeting of two different sets of frames, the particular kinds of simultaneity discussed here. It accounts for this strange phenomenon in which someone may lose voice even while he or she is using it.

3. Conclusion

The examples discussed here are by no means exceptional, to be sure. The kind of heteroglossia described is probably just that, a macroscopic instantiation of Bakhtinian heteroglossia. The advantage offered by institutional encounters such as the asylum procedure is precisely that it offers us a macroscopic and almost grotesque vantage point. Problems of voice are problems of inequality, and as such they will occur in every environment where inequality is a feature of structure (Hymes 1996, Blommaert 2005). The point here, however, is that such structured forms of inequality in language require an analytical toolkit that addresses them as features of structure, not necessarily of intentional deployment in interaction. We need to be able to address the fact that heteroglossia often assumes rather predictable, familiar shapes – the fact that, as Bourdieu so brilliantly demonstrated, our thoughts and actions seem to drift into specific directions and not in others. And this is where habitus may be a useful concept. It offers us a perspective on the not necessarily innocent nature of routinized behaviour, on the fact that routines may be the points where patterns of inequality enter into our everyday behaviour, and that these patterns of inequality lead to patterning in our routines as well.5

5 It is interesting to see that Aaron Cicourel’s essays on medical reasoning, edited by Bourdieu and Winkin (Cicourel 2002), make exactly this point. Cicourel, like Bourdieu,
This is why I believe there is some virtue in reading Bourdieu from an ethnographic viewpoint. The case he argued was a case for situated deployment of structure in activities we perceive as self-evident and normal. It explains why we shift into particular forms of performance when discussing particular topics with particular people, as well as why we wear neckties to do so on certain occasions. Like every classic ethnographer, from Marcel Griaule to Evans-Pritchard, Victor Turner and Clifford Geertz, there is a danger that he should be read as a describer of abstract structure. It is more profitable, I believe, to read his works as examples of ethnography-as-epistemology, a way of conceiving knowledge as always requiring a foundation in lived and experienced reality and practice. Habitus is exactly the concept that should allow such a reading.

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References


emphasizes the ethnographic, experiential and even autobiographical grounding of his cognitive sociology, and he also sets out to identify the structures articulated in everyday professional routines in medical settings. Bourdieu and Winkin in their introduction to Cicourel (ibid.) also comment on style and approach in sociology. Cicourel, according to Bourdieu and Winkin, on the one hand represents the most rigorous, disciplined and aridly factual kind of sociology; yet in selecting the facts he uses, he strictly confines himself to data collected through ‘deep’ ethnographic experiences of his own. The description of Cicourel’s approach is obviously reflexive of Bourdieu himself.
Jan Blommaert


Sarangi, Srikant and Stef Slembrouck (1996) *Language, Bureaucracy, and...*


