

Practice-based video analysis: a position statement

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Practice-based video analysis stands for an ethnomethodological approach that makes explicit the analyst's practical experience in the technical practice that his or her video analysis bears upon. This paper offers a position statement advocating such a "bastard" approach (cf. Lynch 2015). First, the paper outlines the programmatic interest of the approach in the field of ethnography, ethnomethodology, and "multi-modal" video analysis in particular. Second, a tutorial example is offered to demonstrate how the approach, when developed in a stepwise manner, makes it possible to recover a technical practice in its constitutive accountability. Finally, initial attempts to introduce practice-based video analysis in Science and Technology Studies (STS) and Conversation Analysis (CA) are reflected upon in terms of the contrasting disciplinary politics that these attempts have disclosed and may disclose – be it at the pub, in peer review, or at conference presentations.

Introduction¹

How come a problem is posed in the first place, namely that our questions appear worthy of being asked? What is relevant for the solution of a problem? When does it appear 'sufficiently' solved, so that we interrupt further investigations? (Schütz 1982, *translated by the author*)

If I trust my unsettled experience of research practice in ethnography, ethnomethodology, and Science and Technology Studies (STS), then there

¹ The argument developed in this paper has been presented at various occasions, including a special ethnomethodology session on "respecification" at the Institut Marcel Mauss, EHESS, Paris, on 11 December 2015, the *International Institute of Ethnomethodology and Conversation Analysis* at the University of Southern Denmark, Kolding, on 6 August 2015, and the Annual meeting of the *American Sociological Association* in San Francisco on 18 August 2014. Special thanks are due to Andrew P. Carlin, Barbara Pentimalli and Andrea Spreafico for encouraging this written statement of the argument.

seem to be two basic options for deciding on how to begin with tackling the “problems of relevance” outlined in the epigraph. Basic as they seem, these options may be cast in terms of two mundane questions: “why must whatever I say be important?” *versus* “what do I have in front of my eyes?”. In the field of STS, the work of most authors, empirical or conceptual, seems to indulge in the first kind of opening move. “Relevance,” accordingly, is understood as an argumentative requirement and rhetorical performance of pertinence in terms of how science, technology, and medicine (to add in an increasingly studied domain) are to be questioned and analyzed, if not “reassembled” to face the public issues of the day, ranging from climate change and its global response (e.g. Latour 2004) to financial speculation and the cultural critique of its fateful abstractions (e.g. Knorr Cetina 2016). In turn, the second question – “what do I have in front of my eyes?” – defines the leading question for both empirical and conceptual inquiry in the field of ethnomethodology (e.g. Sharrock and Coulter 2001). “Relevance”, then, does not primarily allude to the worldly virtues of a research stance, but it points to the empirical question of how the manifest conduct of practical activities, as the key phenomenon under scrutiny, displays what is crucial to the participants involved, if only to sustain their conduct *in situ* – for example, that a task or topic will be addressed right away².

The two outlined positions may be mutually exclusive as opening moves for social inquiry (cf. Sacks 1984). Yet they presumably aren’t when arranged in temporal succession. In *Scientific Practice and Ordinary Action*, M. Lynch (1993) famously argued that ethnomethodological inquiry may start with taking up a grand topic from the history, sociology or philosophy of science, and then make explicit how that topic (the topic of “discovery,” for example) may be relevant to and part of mundane scientific practices. This two-step move, leading to a “respecification” of scholarly themes in practical terms (e.g. Garfinkel *et al.* 1981), has been extended to many domains other than science, including the humanities (cf. Button 1991) and social sciences (cf. Hester and Francis 2007). In *Respecifying Lab Ethnography*, I argued that two-step “respecification” may turn out to be a self-defeating enterprise, at least insofar as it tends to borrow its topical agenda, not from the practice encountered *in situ*, but instead from a scholarly literature accessed ‘offsite’ (cf. Sormani 2014a). A ‘second order’ respecification might be the odd result: general concepts are discussed in the light of a particular context (a locally unfolding practice), whilst the

² Indeed, «there is [...] no guarantee that the course of [a] conversation [or other kind of activity] will provide the occasion for any particular mentionable to ‘come up naturally’» (Schegloff and Sacks 1973: 302).

‘first order’ relevancies that went into the procedural establishment of that very context remain unknown (including the practice’s technical intricacies and own conceptual articulation). Practice-based video analysis, in turn, was introduced to recover those ‘first order’ relevancies, both technical and conceptual. A current domain of experimental physics, expressible in terms of one formal equation ($N \alpha dI_t / dV_t$), provided the empirical case in point³.

“Practice-based video analysis,” then, stands for an ethnomethodological approach that makes explicit the analyst’s practical experience in the technical practice that his or her video analysis bears upon (in contrast to transcript-based approaches, as we shall see). Instead of a two-step move, the approach involves a three-step procedure: first, the approach requires a video recording to be made of regular practitioners at work; second, the filmed practices and observed tasks are tentatively reenacted; third, the difficulties encountered in the reenactment exercise – technical, social, and/or moral – are to be treated as “tutorial problems” (Garfinkel 2002, chap. 4) that give distinctive insight into the particular exigencies and competent achievement of the examined practice (e.g. as achieved by regular practitioners in the initially filmed situation). The outlined “bastard” approach, which mixes practical experience and analytic interest, was developed in the vein of Garfinkel’s later program (cf. Lynch 2015) and, in particular, by taking its cue from his ‘hands on’ autodidactic and experimental approach (cf. Garfinkel 2002, chap. 9; Lynch *et al.* 1983). This paper takes the form of a *position statement* that articulates the broader interest of practice-based video analysis (including practice-embedded “point of view” shots) in and for the descriptive analysis of a wider range of social practices – at least for ethnographic, ethnomethodological, and “multi-modal” video analytic approaches focused on everyday relevancies *in situ*. First, the paper highlights the *programmatic interest* of practice-based video analysis for such approaches. Second, it examines a tutorial example and, on this basis, demonstrates how the three-step approach allows one to describe a situated practice in its *constitutive accountability*. Finally, it reflects upon the *disciplinary politics* that initial attempts to introduce practice-based video analysis in established fields, including Science and Technology Studies (STS) and Conversation Analysis (CA) have disclosed and may disclose – be it at the pub, in peer review, or at conference presentations⁴.

³ A. Schütz’ (1973) distinction between “first” and “second” order concepts, Garfinkel and Sacks’ (1970) and Wieder’s (1974) arguments on the internal relationship between and mutual elaboration of practical activities and discursive formulations, and Livingston’s (2008) analysis of “Lebenswelt pairs” provided key resources to make this case (see also Bjelić 1996).

⁴ The irony involved in formulating a position statement for an approach that addresses the basic question “what do I have in front of my eyes?” (rather than the sophisticated one “why must

Programmatic interest

The kinds of problems of relevance outlined by A. Schütz in the epigraph provide a longstanding concern for the ethnographic study of scientific practices. In the “laboratory studies” tradition, methodological discussions have revolved, notably, around the problem of technical access to scientific practices, the degree and type of access necessary for ethnographic findings to be made, and the research implications to be drawn for STS more broadly (for recent discussions, see Doing 2008; Merz 2006). The “problem of technical access” is largely equivalent to a particular problem of relevance: the empirical concern by the ethnographer to recover the practical priorities of scientists at work. Yet major changes in research policy, trading “trust in science” for more overtly politicized agenda-setting (cf. Audétat *et al.* 2015; Lynch 2014), have also promoted a different concern for “relevance” in STS, including ethnographic approaches, now expected to frame practice descriptions in terms of social and political, if not moral issues (cf. Hackett *et al.* 2008; Hess 2001). The question whether this framing exercise is not better described as a ‘bad deal’, where a received normative agenda substitutes for a more autonomous sociological approach (e.g. Watson 2000), does not have to be addressed in detail here. Suffice to note that the very question hints at the broader scope of the issue involved, regarding not only a sociological inquiry’s “choice of problems”, but also and more importantly its “formats of problematization” (cf. Quéré 2004: 93). In the remainder of this section, I shall briefly discuss how problems of relevance have figured in ethnography, ethnomethodology and – more recently – “multi-modal” video analysis. The *programmatic interest* of practice-based video analysis, when it comes to the problem of analyzing everyday concerns and practical priorities *in situ*, will then be made explicit against the background of that discussion⁵.

Ethnography, by and large, has been considered as much an observational as a textual enterprise – a delicate project, dangling between prose and participation. Accordingly, ethnographers’ methodological concerns are distributed across the range of tasks implied by that project: how to get (technical) access? What to do, observe or note? How to write it down? Hinting at the routine problems of fieldwork, any of the casually listed questions may become a topic in itself. Reconsider J. Van Maanen’s *Tales of the Field* (1988). Based on the author’s experience as a seasoned ethnographer, it addresses the last question in the list.

whatever I say be important?”) will be taken up in the final part of this paper. In the meantime, the paper addresses the concern raised in note 2 above.

⁵ That is not to say that the approach has no implications regarding the (ethno-)methodological engagement with normative agendas, such as “disciplinary politics” – a point taken up in the final section.

It does so by devising a typology of stylistic conventions in ethnographic reporting – distinguishing different types of “tales” – and interpreting that typology as the lived expression of any culture’s ambiguity (ibid.: 127). As a critical reviewer’s question suggests, the latter interpretation did not go unnoticed for long: «If culture is itself ambiguous, is it encountered as such by people leading lives within and according to a culture’s maxims? Can we get any closer description of these lives, of the actual, situated interactions the pursuit of which provides – for ‘natives’ as well as their ethnographers – whatever evidence there is of culture’s bearing on the lives of a people?» (Zimmerman 1990: 596). Zimmerman’s question, whatever the current interest of his review, encapsulates how the analytic problem of relevance (at least as far as practice description is concerned) may present itself as an ethnographer’s problem (whenever he or she becomes interested in participants’ everyday concerns and practical priorities)⁶.

In *ethnomethodology*, the field devoted to the descriptive study of “folk methods” (cf. Lynch 2001), problems of relevance have been defined and dealt with in at least two ways. “Why that now?” (Schegloff and Sacks 1973) has provided a (if not *the*) leading question for “Conversation Analysis” (CA), initially developed as a subfield of ethnomethodology and devoted to analyzing the mundane methods of the interactive production of intelligible talk. The “why that now” question invites conversation analysts to examine the retrospective and prospective orientation of any given turn at talk. In displaying an orientation to both previous and subsequent turns at talk, any turn at talk exhibits the practical priorities (in short, “relevance” or “relevancies”) in and as the unfolding conversation, including its successive topics and structural organization (cf. Schegloff and Sacks, *ibid.*). Whether, why, and how social identities and particular attributes of participants come to matter in conversation should be examined accordingly (cf. Schegloff 1991). A second way to analyze “relevance” in ethnomethodology goes back to its “studies of work” program (Garfinkel 1986), a program devoted to describing the practical methods and technical intelligibility of work, any kind of work (cf. Rouncefield e Tolmie 2011). The analytic issue at stake has become known as “Shils’ complaint” and been reported as follows: “By using Bales Interaction Process Analysis [BIPA] I’m sure we’ll learn what about a jury’s deliberations makes them a small group. But we want to know what about their deliberations makes them a jury” (cf. Garfinkel *et al.* 1981: 133; quoted in Lynch 1993: 6-7). BIPA is not CA. Yet Shils’ complaint has also been brought to bear on CA, especially

⁶ D.L. Wieder’s *Language and Social Reality* (1974) has arguably provided the most convincing answer to Zimmerman’s concern, insofar as Wieder’s study provides a detailed account of how a particular way of talking may constitute a particular culture as the “real thing” for its members.

when it is extended beyond its “natural habitat” (ordinary conversation) to address work practices *in situ*⁷.

A recent development in the hybrid field of ethnomethodology/CA is “*multi-modal*” *video analysis*. This development grew out of CA, insofar as it extends its methods – real-time recordings of unfolding interaction, detailed transcriptions, and turn-by-turn analysis – to encompass “embodied interaction” (Streeck *et al.* 2010). The use of video recordings in addition to audio materials makes it possible to describe locally unfolding interaction as a “multi-modal” phenomenon – that is, a phenomenon whose intelligible production involves not only turns at talk but also non-verbal constituents (gestures, glances, instruments and objects, etc.). This development of CA (cf. Deppermann 2013) has also aimed at recovering work practices *in situ* (e.g. Heath and Hindmarsh 2002; Goodwin 2000; Mondada 2005). The pursuit of this aim enriches CA’s interactive answer to any turn at talk’s relevance, insofar as it asks “why that [turn] now” from a “multi-modal” perspective (examining, for example, gestural and visual components in the turn’s construction). Yet it also begs the question of the *technical intelligibility* of any locally unfolding course of work practice. Indeed, a coordinated deployment of multiple resources (verbal, gestural, instrumental, etc.) may well be necessary, but still remain insufficient, for the successful accomplishment of a technical task in participants’ own terms (as an experimental task of a particular kind, for example). Whenever this difference between necessary and sufficient conditions in practice description is not mentioned or remains analytically inconsequential, new variants of Shils’ complaint become relevant⁸.

The unacknowledged difference, in turn, marks the *programmatic interest* of practice-based video analysis. The latter, when conducted step by step, allows one to recover the technical intelligibility of work practices *in situ* and, in doing so, to make explicit the coordinated deployment of multiple resources as a constitutive part of their successful accomplishment in participants’ terms and techniques (those particular to current domains of experimental physics, for example)⁹.

⁷ For this line of critique, see Bjelić and Lynch (1992), Lynch (1993; 2015), Sormani (2014a: introduction). The pioneering analysis of jury deliberations is to be found in Garfinkel (1967: chap. 4). It was in the context of this analysis that the term “ethnomethodology” was coined (cf. Garfinkel 1974).

⁸ For example, important strands of “multimodal” video analysis examine the “coordination” of multiple resources (conversational, gestural, instrumental, etc.), whilst leaving out the purposefully oriented character of the ensemble of required tasks (e.g. a “high school physics lab”) that somehow relies upon and must rely upon the described coordination (cf. Ford 1999).

⁹ In this context, Garfinkel (2002) alluded to any practical task’s “oriented” achievement (that is, an achievement oriented with respect to the manifest purpose that the task serves). A book

Constitutive accountability

The experiment on which we report was set up, not to figure out how Galileo's experiment [the inclined plane demonstration] did work but rather to discover what would make it *not* work, what contingencies would lose the phenomena. Because these would then be contingencies that Galileo would have to have taken into account. And indeed *when* you find out what they are, you *can* see that certain features of the design of his experiment are designed to take those contingencies into account (Garfinkel 2002: 264, note 2; *emphasis added*).

“Multi-modal” video analysis has characteristically described the coordinated deployment of multiple resources (verbal, gestural, instrumental, etc.) as a necessary condition for the practical accomplishment of technical tasks in wide range of domains (including archeology, physics, surgery, among others). For example, video-based descriptions have been offered of how “multiple resources” are used in archeological excavation (Goodwin 1994), high school physics labs (Ford 1999), and surgical training (Mondada 2007). Yet the description of how such resources are used *in* these contexts does not address how they are used *for* them (that is, for the successful achievement of a particular excavation task as archeologically relevant, for example). Just how, in other words, are technical tasks accountably achieved as such, as characteristic moves in and for the particular domains (e.g., archeological excavation) that they constitute and are expected to constitute? What is their *constitutive accountability*? Practice-based video analysis, the three-step endeavor described in the introduction, addresses and answers this empirical question of primordial EM/CA interest (cf. Macbeth 2014 and note 10 below). A tutorial example will now be offered to demonstrate how the approach, when developed in a stepwise manner, makes it possible to recover a technical practice in its constitutive accountability – in line with Garfinkel's ‘take’ on Galileo's experiment, quoted above¹⁰.

“A little dirt never hurt anyone” is the title of an award-winning paper in STS (Mody 2001). As I read out loud this title to a physicist whilst he was

that explores the heuristic and critical potential of practice-based video analysis beyond experimental physics is in the works (cf. Sormani *forthcoming*).

¹⁰ For a task, practice, or interaction to be achieved as such, it has to be *accountably* achieved. “Constitutive accountability” refers to this practice-immanent requirement and the mundane methods of its lived local satisfaction (for example, in terms of “technical intelligibility”). It defines ethnomethodology (EM)'s core phenomenon (cf. Garfinkel 1967; 2002).

cleaning his microscope, I did not have to wait for his rejoinder: "... but it surely hurts my microscope" (see also Sormani 2014a: 241). His instant rejoinder constituted an apt reminder of the practice-immanent requirements of microscopy in and for his field of experimental physics. Among these were both [microscope] and [sample cleaning]. How were these requirements met *in situ*? [[Q-tip drenching]] constitutes an instructive case in answer to this question and a first tutorial example of practice-based video analysis (double square brackets have been used to indicate that the mentioned task was devised as part of an overall [sample cleaning] procedure; on the analytic use of brackets, see Garfinkel and Sacks 1970; Lynch 1993: 289-290).

Consider, to begin with, the video recording of two regular practitioners at work. As the following excerpt suggests, both a trainee and an instructor were involved in [[Q-tip drenching]] at the lab bench:

Excerpt 1 [[Q-tip drenching]] at the lab bench

I: instructor, T: trainee

00 T I((attempts to drench Q-tip with alcohol))

01 I I((observes trainee's attempt))

02 #0

03 I >I advise you<=to keep lit upright and

04 I I((indicates how to hold the

05 I alcohol bottle))

06 I to |position yourself at this level here. >so that=

07 I I((indicates bench level as operating level))

08 I =if 'it comes<=it doesn't fall- (* °)

09 T I((kneels down in front of lab bench, keeps bottle

10 T upright, drenches Q-tip, 5s))

11 #1

12 T I((puts the alcohol bottle back on the lab bench))

13 T I'll try to have this dropped? °on°

14 T I((changes q-tip from

15 T his left to his right

16 T hand))

17 I that's it. (.) °>then you blow with.<°

18 T I((approaches the drenched Q-tip

19 T to the sample))

20 T °°>in depositing it, right?<°°

21 T I((puts alcohol on the sample surface, 6s, in making

22 T contact between them| first...

23 T |there you go.=



#0



#1

At least two observations can be made on the basis of the above excerpt (for a prior version of the following analysis, see Sormani 2014a, chap. 4).

First, it is under supervision that the trainee (T) attempts to achieve [[Q-tip drenching]] as the task at hand. “Under supervision” means under the gaze and with the help of an instructor (I). The latter observes the tentative achievement of the task by the former (lines 00-02, #0) and, on that basis, gives him advice on how to correct his technical gesture and bodily position (lines 03-08). By taking into account the instructor’s advice, the trainee succeeds in drenching the Q-tip (lines 09-11, #1). Second, the task at hand – [[Q-tip drenching]] – appears indeed as part of [sample cleaning], a procedure that includes both prior and subsequent tasks (such as assembling the sample, an alcohol bottle, and a Q-tip, and, once the Q-tip is drenched, depositing an alcohol drop on the sample surface, cf. lines 18-22). Taken together, these two observations make available [[Q-tip drenching]] as a progressively achieved, “multi-modal” phenomenon. Yet they beg the key question of just how the [[Q-tip drenching]] task was accountably achieved, as a constitutive part of the [sample cleaning] procedure, so that this achievement was successful? In particular, one may ask: why did it take five seconds (cf. line 10)? Was the Q-tip drenched first (say, after two seconds), then inspected (for the remaining three seconds)? If so, why? Just how was the Q-tip drenched? How much alcohol would be enough? What would the appropriately drenched Q-tip look like, so that the next task – [[alcohol drop depositing]] – could be attempted (cf. lines 18-22)¹¹?

This set of questions, as well as the analytic issue flagged in note 11, brings us to the second step of practice-based video analysis: the reenactment of the task at hand. As I attempted [[Q-tip drenching]], I filmed my attempt from within its tricky course. This “point of view” shot and the problems encountered in making it provided me with a “look again” procedure (Watson 1998: 206); they allowed me and now us (as we shall see) to (dis-)solve most of the raised questions, as well as to make explicit just how the trainee drenched the Q-tip in the first place so as to achieve the overall purpose of the task (i.e., its constitutive contribution to the cleaning procedure and envisaged physics experiment). Consider my account of the self-instructive exercise:

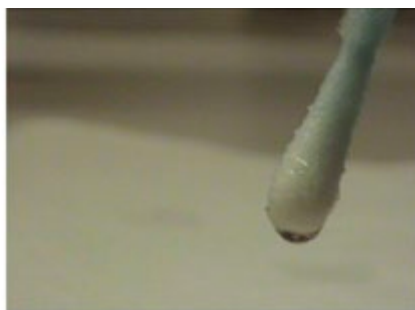
¹¹ In passing, it should be noted that I was at first unable to identify many of the technical tasks as constitutive moves of the locally unfolding experimental procedure. An initial transcription was deliberately made of practitioners filmed at work under the principled requirement of exhaustiveness. When reconsidering that initial transcription on the basis of my subsequent *practical* experience in microscopy, I noticed that the tacit selection principle for passage transcription had been “talk” (rather than “tasks”). Indeed, I had transcribed the talkative moments of the interaction between instructor and trainee, instead of their joint achievement of technical tasks – in short, I had missed the “praxioms” (Bjelić 2003) of the experimental practice under scrutiny.

Excerpt 2 [[Q-tip drenching]] as a tutorial problem

To drench the Q-tip, I press the alcohol bottle. Immediately, the liquid shoots up the bottleneck and pours over the targeted Q-tip, which is thus drenched. This immediate reaction comes as a “nasty surprise” to me, and I cannot avoid granting it with an expletive. The net result: A drenched paper tissue below the Q-tip (a), as well as an alcohol drop dangling from the latter, due to the surface tension of the liquid (b).



(a)



(b)

[[Q-tip drenching]], as engaged in and filmed “from within,” afforded me with a “tutorial problem” – that is, a problematic task whose attempted solution taught me, as it might have anyone else, the methods required to solve it (see Garfinkel 2002: 145; Rawls 2002: 28, 33-39). That is not to say that I managed to achieve the task. On the contrary, as I pressed the alcohol bottle too abruptly, I drenched not only the Q-tip but also the paper tissue underneath. Abrupt bottle pressing, then, appears as my lack of mastery of the method required to have the Q-tip drenched *appropriately* (lack of mastery which, in the above account, is formulated in terms of a “nasty surprise,” “granted with an expletive”)¹².

By contrast, the trainee’s achievement of the same task, [[Q-tip drenching]], now appears in its distinctive mastery, under the auspices of *utmost caution* – that is, on the basis of the third step of practice-based video analysis: the detailed reconsideration of the initial video clip (see Excerpt 1, line 10, #1). His caution, paradoxically, finds an almost invisible yet eventually observable expression: holding an almost empty plastic bottle in his right hand, he pressed it so smoothly (so as to have a minimal amount of alcohol moving up

¹² Note, incidentally, that and how the examined task affords us with its own means of inspection and analysis, demonstration and description: «the phenomena of everyday life, without exception, already possess whatever methods they require for their own [production,] observation, recognition, collection, and analysis» (Clayman and Maynard 1995: 25).

the bottleneck) that the pressing of the bottle remained almost invisible to the “outside” camera view (as afforded by the bystander or onlooker’s eye, that is). No distinctive, abrupt bottle pressing could be observed during the five seconds that he kept the bottle close to the Q-tip; nor did the trainee comment upon the unfolding task and its eventual achievement (as I did, granting my clumsy action with an expletive). In sum, the competent, mute performance of the tactile skill produced its own apparent and thus observable invisibility. The smooth pressing of the alcohol bottle, upon re-inspection, appears as a *steady* pressing to have the intended, minimal amount of alcohol move up the bottleneck to being released in a *controlled* manner. In other words, the trainee appears to have anticipated, if not pre-empted, the problem that might otherwise have occurred during the next task (that is, the “alcohol spillover” that would “dilute the silver glue” and thus “detach the sample,” which the instructor had warned him against). However, this apparent anticipation expresses nothing other than the trainee’s achievement of the current task, [[Q-tip drenching]], in an experimentally adequate way, and that is, to begin with, as part of the locally enacted [sample cleaning] procedure. Indeed, the envisaged physics experiment – a Niodime topography – would be impossible on a detached sample because this would be outside the microscopic device¹³.

Disciplinary politics

The politics of reading offer an anthropological puzzle. Knowledge of reading-really is cultural knowledge of how the community reads; the grounds of the politics of reading are deeply embedded in the practices of the critical community. What does that knowledge look like to the community, how is it articulated, and how is it tied to *disciplinary politics*? Such questions are central concerns of any anthropology, for they ask who is to speak authoritatively about the tribe – about its knowledge, its members’ practices, and the worldliness of its projects (Livingston 1995: 135; *emphasis added*).

When it comes to “disciplinary politics,” as characterized by E. Livingston in his study of literary criticism, one may speak of a ‘reflexive’ motive for techni-

¹³ For further analysis of this tutorial experiment and its additional tasks and preparatory procedures, the reader is invited to consult Sormani (2014a: chap. 4).

cal self-instruction and, by extension, for practice-based video analysis – in addition to its empirical and descriptive motive, as elaborated in the previous section. As a novice practitioner cum video analyst, one may indeed encounter “tutorial problems” (Garfinkel 2002: chap. 4). Yet those “tutorial problems” do not only disclose (and thus help one to make explicit) competent “members’ discipline-specific procedures” (ibid.: 145) as their intended *figure*, but they do also disclose the *ground* against which this figure becomes (or should become) recognizable, intelligible, accountable – in short: any craft’s circumstances. Craft and circumstance, like figure and ground, constitute each other; they may thus vary or be varied together, be fitted to, or severed from, each other (all of which, in turn, can be observed). As when some physicists commented upon the incongruity, for me, to use *their* training microscope: “what are you doing here!?”¹⁴

The practical engagement in technical tasks, then, teaches one this internal, yet variable relationship between craft and circumstance – in short, “disciplinary politics,” understood not only as a procedural requirement invoked for aiming at practical adequacy, but also in terms of the local circumstances in terms of which such adequacy may prove “fitting” or not – both of which, craft and circumstance, should be treated as both historically and practically contingent (so should the “framing” of sociological inquiry, as alluded to above). How have practitioners of STS and CA treated such contingency, both the contingency of their own modes of analysis and that of its possible alternative(s)? This final section of the paper shall take up the question with respect to the recent introduction of practice-based video analysis as a possible development of ethnomethodological inquiry. Its recent introduction (see Sormani 2014a; 2014b) was contingent in three respects: first, related to the difficulties of recovering a current domain of experimental physics in its own terms and techniques; second, given the manifestly pending answers to Shils’ complaint, both in the field of “multi-modal” video analysis and constructivist ethnography in STS (cf. Doing 2008), at least in and for current domains of experimental physics (see previous sections); finally, the qualifier “at least” hints at a third contingency: the idea to subject CA itself to a practice-based video analysis by using an analyzed transcript as a *script*, both to reenact and reexamine the initial analysis (cf. Sormani 2014c). How was this triple contingency dealt with in STS and CA, respectively? What “disciplinary politics” did it disclose so far?¹⁵

¹⁴ For further analysis of such moral bewilderment and its “routine grounds,” see Garfinkel (1967: chap. 2).

¹⁵ In contrast to Livingston, my concern is *not* «who is to speak authoritatively about the tribe» (1995: 135), but *how* that is done, *procedurally*, regarding «its knowledge, its members’ practices,

In the field of STS, *Respecifying Lab Ethnography* (Sormani 2014a) has been reviewed and discussed as a critical intervention in STS, rather than as a substantive contribution to ethnomethodology (e.g. Cousineau 2015; Gill 2016). Ironically, this STS-tilted reception has also been nourished by ethnomethodologists (cf. Hoeppe 2015; Lynch 2015). The skeptical line of argument that the book brings to bear on STS' "relevance" with respect to the critical treatment of "grand challenges" (climate change, financial speculation, etc.) has itself – quite rightly – been met with skepticism:

Building on these efforts [by previous ethnomethodologists] to reinvigorate sociology, Sormani has encountered a paradox. Sormani argues that he contributes to STS discourse by analyzing members' common sense knowledge instead of importing the concepts popular in STS. But in order to do this, he has to use and analyze concepts that are probably unfamiliar and/or unimportant to the anthropologists and sociologists who maintain an interest in lab studies. Thus, emphasizing member relevancies poses the risk of estranging the scholars whose work it challenges and who are in a position to describe and circulate its contributions to STS discourse (Cousineau 2015: 88);

[h]is main lesson for STS is a deflationary one that questions both the methodological basis (in practical understanding) and precise meaning of the claim that physical facts are constructed or somehow inflected with culture and politics. Sormani himself wonders if this lesson was worth the arduous task he undertook to engage more deeply with the performative details of a contemporary physical science than had been done in any prior STS or ethnomethodological study (Lynch 2015: 613);

[t]he extent to which [the described] process of discovery can be translated to greater engagement between society at large and scientists remains to be seen (Gill 2016: 213).

However, the outlined *tu quoque* argument with respect to broader STS "relevance" passes over the disciplinary intervention that the book, and practice-based video analysis in particular, makes in the field of ethnomethodology, not only by calling into question the programmatic coherence of two-step "respecification" as outlined in the introduction, but also by challenging the conventional boundaries between different strands of ethnomethodological

and the worldliness of its projects» (*ibidem*), indeed. This concern is further elaborated in Sormani (*forthcoming*).

inquiry (CA, the “studies of work” program, “multi-modal” video analysis, etc.). More importantly, practice-based video analysis offers not only an internal critique of the field, but it also constitutes an arguably new, hybrid and heuristic development, inviting the video analyst to ‘practice the phenomenon’ and, thereby, to make explicit the «recognizable work of its production» (Livingston 2008: 246). An empirical reminder of the «phenomenon-locating feature» (Wieder 1999: 15) of ethnomethodological inquiry and its practical requirements is thus offered. The same point holds with respect to ethnographic endeavors more broadly, given that practice-based video analysis also constitutes a particularly “strong participant-observation requirement” (Lynch 1993: 274)¹⁶.

Up to this day, practice-based video analysis has not been embraced in the field of CA. Quite the contrary, the field has not only gone “multi-modal” (Deppermann 2013) but also gone “reflexively” multi-modal (Broth *et al.* 2014). That is to say, it has offered a range of video-based analysis of a range of practical uses of video, both lay and professional (including live TV editing practices, for example). In doing so, it has not radically questioned (what Livingston calls) “the grounds of the politics of reading” that remain deeply embedded in the practices of CA and its professional community (e.g. the practices of reading transcripts). Rather, it has extended those grounds to cover new ground, including the (video) analyst’s own practices of video recording (e.g. Mondada 2014). Practice-based video analysis, in turn, was designed to make explicit the technical intelligibility of “esoteric” practices, such as cutting-edge experimental physics, regardless of their being filmed or involving video as a constitutive moment of their ordinary conduct (simply, the video camera would be used as a “heuristic handicap” to make explicit that ordinary conduct, in and as its technical intelligibility). Nevertheless, I have made an initial attempt to reenact and reexamine particular exemplars of CA, notably by reenacting and reexamining a recent analysis of alleged “information transfer” in interaction (cf. Sormani 2014c). For space reasons (among others), it is not possible to go into the details of this practice-based video analysis and the resulting “re-specification” of CA. Suffice to note the ambivalent disciplinary politics that this attempt has disclosed so far¹⁷.

¹⁶ Needless to say that the ethnographer is not dispensed from *judging* when, whether, and how this requirement is to be fulfilled, relaxed or circumvented. Simply, he or she should make this judgment explicit as such, as an incidental expression of his or her local involvement. On the “unique adequacy requirement of methods”, see e.g. Garfinkel (2002: 175-176).

¹⁷ The empirical point, to begin with, was not to question or challenge the analysis of conversation in terms of “information exchange” (e.g., Heritage 2012), but rather to raise the question of how conversationalists would actually talk, on and as any given occasion, so that their talk

On the one hand, I got *sotto voce* compliments by colleagues at the pub: «Great move, but, you know, we cannot do this kind of analysis here, lest I'd like to diminish my chances of getting my next project funded» (personal communication). On the other hand, I got relatively harsh (pre-)peer review dismissals from CA experts consulted by a leading journal in communication studies. These anonymous dismissals went along the following lines (the wording is no literal quotation but keeps the spirit of the dismissals): «there's nothing substantial to be gained from this weird, pretentious, self-indulgent exercise; shame on the author for this preposterous exercise – filled with 'ethno' jargon, but analytically inept; a service is rendered not only to the public, but also to the author, if this piece remains unpublished». The ambivalent reception of the piece has indeed not exactly been an invitation to work it into a full-blown publication. At the same time, its reception has offered another momentary expression of disciplinary politics to be reflected upon, and an interesting opportunity for the present position statement to be formulated. To the irony involved in formulating this position statement for an approach – namely, practice-based video analysis – that addresses the basic question “what do I have in front of my eyes?” (rather than “why must whatever I say be important?”), I should like to respond with Kierkegaard: «irony is a disciplinarian feared only by those who do not know it, but loved by those who do» (quoted from his *On the concept of irony*, in Parcero Oubiña 2006: 255).

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could be interpreted and understood in these terms at all (for a related line of analysis, see Turner 1976).

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