Complicating Common Sense:

The Theory, Practice and Politics of Critical Ethnography in CSCL

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Abstract: As a still-emerging interdisciplinary field of research and practice, CSCL has an opportunity to incorporate the full power of ethnographic analysis into its understanding and scaffolding of collaborative learning. By challenging common sense understandings and revealing cultural assumptions embedded in system designs, the work of Diana Forsythe exemplifies the promise and peril of critical ethnography. Within CSCL several challenges must be confronted, including: the intensifying attacks on the value of qualitative educational research; the perception that ethnography is merely a methodology that any researcher can use regardless of context; and the pervasive, generally unacknowledged influence of positivism. The inextricable, reciprocal connections between method and theory necessitate an approach to ethnographic analysis that is explicitly grounded in social theory. Four strands of current theory and practice (design ethnography, activity theory, ethnomethodology, and situated learning) could all contribute to the development of a critical ethnography of—and for—CSCL.

Exemplary Ethnography

Anthropologist Diana Forsythe's (2001) *Studying Those Who Study Us: An Anthropologist in the World of Artificial Intelligence*, provides an excellent introduction to the goals, concerns, and challenges of critical ethnography. Of particular interest is Forsythe's participant-observer account of a three-year project to build a natural language patient education system for migraine suffers (2001, pp. 93-118). The developers envisioned an interactive computer system that would elicit a patient's symptoms and medical history and use that information, in conjunction with the physician's diagnosis, to present individually-tailored information about diagnosis and treatment. The project team included computer scientists, cognitive psychologists, physicians, and anthropologists. As a major author of the grant proposal—which explicitly proposed the use of ethnographic analysis to support system design—Forsythe began the project as a full-fledged co-investigator. In addition to documentation of project meetings, her fieldwork included observation of doctor-patient visits in neurology, informal interviews with physicians and patients, and extended formal interviews with migraine sufferers. Forsythe was simultaneously conducting ethnographic analysis *of* and *for* the system design project. However, as the project progressed she found it increasingly difficult to reconcile the roles of participant and observer because of the epistemological and practical tensions between the "relativist understandings of ethnographic data" and "the positivist expectations and procedures of normal system building". Despite compilation of a rich body of ethnographic data about migraine and a shared intention to incorporate anthropological insights into an innovative system design, the resulting prototype "reflected much less ethnographic input than we had originally envisioned." (2001, p. 98).

Cultural analysis is particularly important in biomedical technologies because designers are generally unaware of the tacit assumptions about privileged perspectives and information flows they built into their systems. Once the system is designed, these assumptions are generally invisible and are unlikely to receive much scrutiny. Forsythe's analysis of the migraine patient education system demonstrated how the designers' cultural and disciplinary assumptions were embedded in every stage of development: project team selection, problem formulation, knowledge acquisition, writing system code, and evaluation of the prototype. The *project team* assembled for the patient education system included "half a dozen faculty members [including two doctors], two programmers, a research assistant, and about six graduate students." However, Forsythe noted two glaring omissions from the make-up of the team: nurses and the intentional inclusion of migraine sufferers. Even though nurses view patient education as a vital component of their work, their absence from the project team "reflects the characteristic muting of nurses' voices in medical informatics in relation to those of physicians." The failure to consciously include migraine sufferers on the research team reflected a fundamental assumption of the project, that patients need information about migraine and that physicians, through an intelligent patient education system, should provide that information. It is assumed that the physician already knows what information the patient needs. Coincidently, the

54 project team did include four members who were migraine sufferers. In private interviews these members described
55 their pain and fear of migraine attacks, the stigma of being a migraine sufferer, and the failure of biomedicine to
56 provide sustained relief. Although these private accounts made it clear that the project physicians' understandings of
57 what patients know and want to know were quite divergent from their own experience, these team members never
58 spoke up as migraine sufferers. In Forsythe's analysis, this silence reflects the ubiquitous privileging of "expert"
59 biomedical expertise over the experiential reality of the patient (2001, pp. 101-102).
60
61 In the *problem formulation* stage the need was defined in technical terms. If migraine sufferers had better
62 access to individually-tailored biomedical information their understanding of the condition would be enhanced and
63 patient compliance would improve. It was assumed that the fundamental issue is that physicians do not have
64 adequate time to provide lengthy explanations during office visits. However, Forsythe's research demonstrated that
65 most migraine sufferers already have a great deal of information about their condition and that they were not
66 necessarily in search of additional biomedical information. During ethnographic interviews patients often expressed
67 other kinds of unmet information needs, like how to carry on a normal life in the face of such an unpredictable
68 disability. Moreover, Forsythe found that a shortage of time during office visits was not the fundamental problem.
69 Many questions were asked and answered during the course of a typical physician-patient encounter, although most
70 of the questions were being asked by the physician and answered by the patient. In addition, the ethnographic
71 analysis demonstrated that doctors were not able to "hear" some of the most fundamental concerns of patients, such
72 as veiled references to a fear of death from brain tumors. The power of physicians to control doctor-patient discourse
73 and avoid topics they were not prepared to discuss was not only unacknowledged, it was actually built into the
74 system (2001, pp. 101-105).
75
76 Ideally, the initial *knowledge acquisition* phase of the project would have been devoted to the compilation
77 of ethnographic data about migraine sufferers and physician-patient encounters. However, the senior physicians and
78 computer scientists insisted that the ethnographic fieldwork and the writing of code proceed simultaneously. It was
79 assumed that results of the ethnographic analysis could be "added in" at a later date. To jump-start the project,
80 developers performed their own knowledge acquisition, which consisted of interviewing a single neurologist (the
81 designated "expert") about issues like treatment strategies and the use and side effects of different migraine drugs.
82 Based upon his years of experience, the neurologist also provided a model dialog of a typical doctor-patient
83 encounter. In his model, patients and doctors speak in unambiguous declarative sentences and the distinction
84 between questions and answers is quite clear; it is also assumed that all communication is verbal and context-
85 independent. Forsythe found that patients' speech was often rambling and repetitive and unintelligible in the absence
86 of nonverbal and contextual cues (2001, pp. 154-155). In the positivist worldview of the developers, information
87 needs exist "out there," awaiting discovery by the anthropologist. These needs are "stable (at least in the short term),
88 inherently ordered, characteristic of and shared by groups or categories of people, and knowable by others." (2001,
89 p. 106). However, when the time came to "add in" the results of the ethnographic analysis fundamental
90 contradictions were revealed. The perspective of the neurologist, which privileged the knowledge and categories of
91 formal medicine, were already incorporated into the basic design of the system. First and foremost, these
92 assumptions conflicted with the ethnographic findings, which saw the patients' and the physicians' perspectives as
93 being different, but equally valid (2001, pp. 105-107).
94
95 During the system building, or *code writing* phase, two examples highlighted the fundamental disparities
96 between the worldviews of designers and anthropologists. First, although the project team included social scientists
97 experienced in the construction and piloting of questionnaires, the job of developing the history-taking module was
98 assigned to a programmer. To the developers, this module was a complex coding task that happened to involve a
99 questionnaire. For Forsythe, this task was fundamentally a questionnaire that happened to involve some
100 programming. An example of tacit embedded cultural assumptions is provided by the available responses to the
101 question "Did anything happen to you at about the same time you started having this kind of headache?" The
102 choices included: "accident," "illness," and "started or stopped taking some drug or medication." A respondent
103 selecting "accident" is then presented with a pop-up window with three more specific options: head injury, injury
104 other than an head injury, or car accident. Given that 75 percent of migraine sufferers are women and there was
105 repeated mention during project meetings that some of the study subjects appeared to be victims of domestic
106 violence, it is remarkable that the available categories preclude the possibility that the migraines were preceded by
107 intentional injury. Secondly, the early decision to "hang" the explanation module on the doctor's recommendations
108 from the last patient visit was also manifested during the code writing stage of the project. The designers were
109 keenly interested in promoting user confidence in the system in order to improve patient compliance. Therefore, the

110 system was designed to re-present the patient's specific symptoms as representing the general case of migraine, and
111 symptoms typical of migraine but not reported by the patient were suppressed by the system. The designers believed
112 it was more important to convince patients of the correctness of the doctor's diagnosis than to provide information
113 that might cause them to doubt the diagnosis. Consequently, a system purportedly designed to empower patients
114 actually ran the risk of increasing the power differential between patients and physicians (Forsythe, 2001, pp. 100,
115 109-112; see also Hakken, 2003, pp. 37-38).

117 Finally, in the *evaluation* phase the designers' wanted to demonstrate that the system offered patients more
118 information than they had received from their doctors in the past. Accordingly, they asked questions like "Did the
119 program ask for more or less information than doctors you have talked to about your headaches?" Rather than
120 examining the quantity of information provided, Forsythe was more interested in its quality and appropriateness,
121 asking "Did the information make sense to you? [If not] What didn't make sense?" (2001, p. 112).

123 Critical anthropologists are very concerned with the issue of *positionality*, the awareness of how their own
124 personal history, power, privilege, gender and racial identities, and perspectives influence their analyses. Instead of
125 aspiring to a position of scientific impartiality, they frame their work with questions like "What are we going to do
126 with this research and who will benefit?" (Madison, 2005, p. 7). Forsythe addresses issues of positionality and
127 power throughout her work. Rather than aspiring to impartiality and neutrality, she argues for a stance of
128 epistemological awareness. She embraces her responsibility to "speak truth to power (2001, p. 185) and work on
129 behalf of the less powerful. Describing herself as a "computer science princess" (2001, p. 194), Forsythe is very
130 aware that her gender and family heritage give her a unique position as a researcher. Both of her parents were
131 pioneers in computer science. Her father was the founding chair of the nation's first computer science program at
132 Stanford in 1965, where a hall still bears his name. Her mother was also a talented computer scientist, but—in the
133 face of gender discrimination—never attained a university position. The annual George and Sandra Forsythe
134 Memorial Lecture at Stanford honors both of her parents (2001, pp. xii-xiv). Although this heritage gave her entrée
135 into the world of AI research, it also created a range of expectations and preconceptions that limited the work she
136 could do. Forsythe is also quite aware that her own experiences and the discrimination faced by her mother have
137 sensitized her to the ongoing devaluing and "disappearing" of women in the computing world (2001, pp. 163-182).
138 Because she observes people whose status and power are generally greater than her own, Forsythe sees her work as
139 an example of what Laura Nader (1969) calls "studying up," which is the antithesis of traditional colonialist
140 anthropology (Forsythe, 2001, pp. 73, 119-131).

142 Although not directly related to CSCL, Forsythe's ethnographic studies of AI research and practice raise a
143 host of complex and contradictory issues. On one hand, her work provides an excellent example (surpassing
144 anything I've seen in the CSCL literature) of the potentially holistic and thoroughgoing nature of ethnographic
145 research. Her analysis of the migraine patient education system demonstrates how tacit cultural and disciplinary
146 assumptions are built unwittingly into every stage of a design project, from the initial selection of the project team
147 through prototype evaluation. Her reflexive awareness of positionality eschews any pretensions of impartiality and
148 neutrality; rather, she argues for a stance of epistemological awareness. Instead of "going native" to elicit and
149 uncritically reproduce the perspectives of her informants (be they migraine sufferers or neurologists), she believes
150 the ethnographer's method should be a continual "stepping in and stepping out" of the field situation (2001, pp. 71-
151 72). Ethnography is predicated on the creative tension inherent in the oxymoron "participant observation." The
152 researcher must balance the cultural immersion required for meaningful participation with the critical distance
153 required for observation and analysis. On the other hand, Forsythe's work demonstrates how difficult it is to
154 incorporate foundational, critical ethnographic insights into intelligent systems design, particularly when there are
155 substantial, often unrecognized epistemological differences between the worldviews of system designers and critical
156 social scientists. Most of what passes for ethnographic research in CSCL (and HCI generally) merely reifies and
157 reinforces researchers' prior common sense understandings. Forsythe's work is presented in hopes that CSCL
158 researchers will be inspired by her example and cautioned by her challenges.

The Qualitative Imperative

161 Since CSCL research and development in the US is generally conducted within the ambit of federally-
162 controlled educational funding, analysis of the intensifying assaults on qualitative educational research provides an
163 essential foundation for the development of a genuinely critical ethnography. A few clarifications and qualifications
164 should be offered at the outset: Although the terms are not strictly synonymous, for the current discussion *positivism*
165 can be thought of as a specific form of *empiricism*. Therefore, an analytical distinction should be maintained

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166 between *empirical research* as a methodology and *empiricism* as an epistemological stance. As anthropologist
167 Nancy Scheper-Hughes observes, "though empirical, our work need not be empiricist" (1992, p. 23). Similarly, there
168 is no necessary correspondence between quantitative research and empiricism. Some quantitative research is
169 explicitly and consciously anti-empiricist (e.g. Bourdieu, 1984) and, as we will see, there is no reason to believe
170 qualitative research is inherently non-empiricist (Hammersley, 2004). In fact, one of the hallmarks of critical
171 ethnography is a critique of the positivism inherent in earlier anthropological accounts (Forsythe 2001, p. 70). I also
172 reject any dichotomization that views quantitative research as "theory-driven" while qualitative research is "data-
173 driven." In that spirit, I vigorously object to the common perception that quantitative research is innately
174 "analytical" while qualitative research is merely "descriptive" (Denzin & Lincoln, 2005). Finally, we should be
175 particularly wary of any bright line distinction between what does and does not constitute "science," particularly
176 when defined in positivist terms. In her introduction to *Naked Science*, Laura Nader (1996, p. xii) writes "The
177 important questions about science ideology, practice, and consequence encounter walls of resistance to reasoned
178 thinking when issues are phrased in binary modes—science and antiscience." (quoted in Hamann, 2003, p. 441).

180 If you download the pdf of the No Child Left Behind Act of 2001 (NCLB) and search on "scientifically
181 based" you should get an impressive 119 hits ("No Child Left Behind Act of 2001," 2002). Clearly, the current
182 administration is signaling an overwhelming commitment to "scientifically based research" (SBR) in education
183 (although we should never discount the importance of political cronyism, see Dillon, 2006; Office of Inspector
184 General, 2006). It behooves us, therefore, to examine carefully the provenience and ramifications of SBR (Lincoln
185 & Cannella, 2004; St.Pierre, 2006). Although NCLB *at the moment* applies only to primary and secondary
186 education, its conception of science pervades all federally-controlled educational research. In 2002 the National
187 Research Council (NRC) refined the federal definition of SBR (National Research Council, 2002). On its face, this
188 report appears to welcome diverse research approaches. However, a closer reading reveals a textbook example of the
189 positivist conception of science (Bloch, 2004; Popkewitz, 2004). The report was created to "provide a set of guiding
190 principles that undergird all scientific endeavors" (2002, p. 27). In a remarkable exercise in boundary maintenance,
191 the authors write, "[t]hese principles help define the domain of scientific research in education, roughly delineating
192 what is in the domain and what is not." (2002, p. 24). Citing as examples "education, physics, anthropology,
193 molecular biology, or economics," the report states, "[a]t its core, scientific inquiry is the same in all fields" (2002,
194 p. 2). Rejecting their own caricature of "the postmodernist school of thought when it posits that social science
195 research can never generate objective or trustworthy knowledge," the authors "assume that it is possible to describe
196 the physical and social world scientifically so that, for example, multiple observers can agree on what they see."
197 (2002, p. 25). Richard Shavelson, Chair of the NRC committee that produced the 2002 report, and his co-authors
198 followed up with a 2003 article in *Educational Researcher* that, once again, appeared to embrace a range of research
199 perspectives. However, they decry the "narrative turn suggested by some theorists and by some members of the
200 design-study community" and assert a rigid continuum of validity: "We believe that the wide range of questions
201 posed in educational research calls for a healthy diversity of scientific methods. The questions and methods may
202 range from *pre-science* exploration to well-warranted descriptive, causal, and mechanism-driven studies."
203 (Shavelson, Phillips, Towne, & Feuer, 2003, p. 28, emphasis added).

205 A wide range of qualitatively-oriented educational researchers have been responding to the practical,
206 theoretical, and political challenges posed by SBR. For example, Denzin and Lincoln (2005, p. 9) urge adoption of a
207 dual strategy:

209 Qualitative researchers must learn to think outside the box as they critique the NRC and its
210 methodological guidelines. . . . They must apply their imaginations and find new ways to define
211 such terms as *randomized design, causal model, policy studies,* and *public science*. . . . More
212 deeply, qualitative researchers must resist conservative attempts to discredit qualitative inquiry by
213 placing it back inside the box of positivism.

215 Regarding the 2002 NRC report, let us leave the last word to Marianne Bloch, an anthropologically-trained
216 quantitative and qualitative educational researcher with over 30 years of experience in the US and elsewhere:

218 If we do not challenge this governance both intellectually and physically, valued possibilities,
219 theory, research, policy, practice, and financial support for alternative types of research will be
220 excluded. . . . Political critique is ever more necessarily part of good research; this report is an
221 excellent illustration of why this is so. (2004, p. 108).

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223   According to St.Pierre, the "paradigm wars" of the 1960s and 1970s fostered an array of epistemologies
224 (e.g. critical theories, race theories, feminist theories, queer theories and neo-Marxist theories) that challenged "the
225 failures of an exhausted liberalism with its false promises of equality and of a science that ignored the voices of the
226 disenfranchised." She notes that "researchers employing these epistemologies found qualitative methodology
227 particularly useful because it is grounded in face-to-face interactions with particular (not random) people."
228 Therefore, "[d]ismissing qualitative inquiry is often equal to dismissing those epistemologies and the people who
229 thought and continue to think and live them." (2006, p. 242). In an analogous context, David Hakken's *Cyborgs @*
230 *Cyberspace: An Ethnographer Looks to the Future* notes the need for a collective, mutually protective response to
231 the backlash against science, technology and society research:
232
233   While presented to date primarily as a critique of analytic stances, the backlash against STS also
234   targets ethnographic practices with feminist and similar sensibilities. Protecting involves
235   projecting a clearer, more collective model of alternative ways to practice science, both within
236   anthropology and more generally. (1999, p. 60).
237
238   The analysis presented herein does not disparage quantitative research *per se,* nor does it necessarily
239 condemn the theoretically-conscious combining of disparate approaches, In fact, "[t]he combination of multiple
240 methodological practices, empirical materials, perspectives in a single study is best understood . . . as a strategy that
241 adds rigor, breadth, complexity, richness, and depth to any inquiry." (Denzin & Lincoln, 2005, p. 5). However,
242 Denzin and Lincoln do critique the way qualitative research is commonly used in "mixed methods" SBR, where
243 there is a dichotomization between exploration (qualitative) and confirmation (quantitative). This approach "takes
244 qualitative methods out of their natural home, which is within the critical, interpretative framework" (2005, p.9) and
245 values qualitative analysis only to the extent that it supports and validates the "real" research.
246
DIY Ethnography and the Pervasive Invisibility of Positivism
248   In 1999 Bonnie Nardi and Yrjö Engeström co-edited a special issue of *Computer-Supported Cooperative*
249 *Work* entitled "A Web on the Wind: The Structure of Invisible Work." Diana Forsythe's contribution to the volume,
250 entitled "'It's Just a Matter of Common Sense': Ethnography as Invisible Work," links the "invisibility" of
251 ethnographic research to the development of do-it-yourself (DIY) ethnography in AI and medical informatics
252 (Forsythe, 1999; reprinted in Forsythe, 2001, pp. 146-162). Since, in the eyes of informants and research colleagues,
253 the ethnographer engaged in fieldwork is merely "hanging out" and chatting with folks, the anthropologist's
254 training, experience, and expertise are rendered invisible. In fact, unobtrusiveness in the fieldwork setting may be an
255 essential component of successful research. Although fieldwork does involve talking to people, "this is no more the
256 entire task than systembuilding is 'just typing' or medical diagnosis is 'just talking to patients.'" (1999, p. 131). This
257 invisibility is accentuated by the tendency of system designers to "delete" the social or "articulation" aspects of their
258 work, a deletion that is also manifested in the systems they design (1999, pp. 142-143). Folks watching the
259 ethnographer in the field may not appreciate subtle interplay between method, theory, and epistemology that
260 structures her research and analysis (1999, pp. 128-129). Forsythe lists six common misconceptions about
261 ethnography:
262
263   1.   Anyone can do ethnography—it's just a matter of common sense.
264   2.   Being insiders qualifies people to do ethnography in their own work setting.
265   3.   Since ethnography does not involve preformulated study designs, it involves no systematic
266   method at all—anything goes."
267   4.   Doing fieldwork is just chatting with people and reporting what they say.
268   5.   To find out what people do, just ask them!
269   6.   Behavioral and organizational patterns exist "out there" in the world; observational research is
270   just a matter of looking and listening to detect these patterns. (1999, p. 130).
271
272   Note how these misconceptions encourage a belief that ethnography is an easily borrowed research method
273 that anyone can use in any situation. According to Nyce and Löwgren (1995), this decontextualization trivializes
274 ethnography, reducing it to "an inventory of (someone's description of) behavior and belief. What this kind of
275 ethnography cannot pick up are those cultural categories that give behavior and interaction their significance and
276 meaning." (1995, p., 39). Writing for a library and information science audience, Thomas and Nyce make an explicit
277 connection between positivism and the trivialization of ethnography, noting the potential insights lost in the process:

278   "The reduction of ethnography to a method may make it easier to teach and to do, but it seriously misrepresents
279   what ethnography is at the same time that it undermines the considerable contribution ethnography makes in
280   allowing researchers to ask and answer hard questions that relate to social issues and the social order." (1998, p.
281   112). Finally, Bader and Nyce's analysis of the connection between positivism and American folk wisdom regarding
282   the nature of reality warrants extended quotation:

283

284   Almost no one today believes that positivism can yield valid statements about the world. In this
285   sense, positivism is no longer taken seriously as grand theory. Nevertheless in the development
286   community positivism has not gone away. Instead, it has become synonymous with common
287   sense, particularly when it comes to how the design community understands human behavior and
288   social life. In part, we believe this has happened because American folk beliefs about what is real
289   and how to determine what is real have much in common with positivism. Pragmatism and a kind
290   of naïve empiricism underlie and inform how Americans make sense of the world. In short, if it
291   can be seen, touched or counted, it's real. Because the development community has not spent
292   much time thinking about how to study social life, it has defaulted to common sense, i.e.,
293   American cultural beliefs on the subject. (1998, pp. 9-10).

294

295   Examples of the three issues discussed above—the rigid dichotomization between "data-driven" qualitative
296   and "theory-driven" quantitative research, the perception that qualitative research is valid only to the extent that it
297   can confirm and generate hypotheses and data for quantitative research, and the reduction of ethnography to a data
298   gathering methodology—can be found throughout the CSCL literature. For example, Hans Spada and colleagues
299   explain that some research approaches are "completely data-driven and qualitative in nature, often following the
300   ethnographic tradition." (Spada, Meier, Rummel, & Hauser, 2005, p. 623). They provide this description of their
301   research program:

302

303   Our goal in developing a new assessment method has been to combine the benefits of data-driven
304   as well as theory-driven approaches, and qualitative as well as quantitative methods. First, relevant
305   dimensions of the collaborative process were extracted from the data in a qualitative procedure.
306   Then these dimensions were implemented in a rating system that enables the user to evaluate the
307   quality of collaborative process in a quantitative way, such that the resulting ratings can be
308   subjected to statistical analyses. (2005, p. 623).

309

310   Spada, et al.'s discussion of the ethnographic tradition cites Guribye, Andreassen, and Wasson's use of grounded
311   theory (GT):

312

313   The research presented in this paper was based on an ethnographic approach, and made use of the
314   raft of related methods and techniques for data collection . . . . The most prominent feature of our
315   analytical strategy was the dedication to a theoretical sensitivity to the emergent categories,
316   influenced by the principles of grounded theory (Glaser & Strauss, 1967). . . . Hence we placed
317   emphasis in identifying concepts and patterns as they emerged from the data . . . ." (2003, p. 338).

318

319   In qualitative research generally, grounded rounded theory is a commonly-cited research strategy. One of
320   the cofounders of GT explains: "In our approach we collect the data in the field first. Then start analyzing it and
321   generating theory. When the theory seems sufficiently grounded and developed, then we review the literature in the
322   field and relate the theory to it through integration of ideas." (Glaser, 1978, p. 31; cited in Seldén, 2005, p. 123).
323   GT's research strategy is not without its critics. For example, in "On Grounded Theory—With Some Malice" Lars
324   Seldén writes:

325

326   A fundamental weakness in GT is connected to theoretical sensitivity. Conceptualizations do not
327   emerge from data. Their source is within the researcher and is dependent on the extent to which
328   he/she is widely read in scholarly matters. If the opposite were true, inexperienced researchers
329   would be the best investigators. . . . You want to advance from everyday knowledge to a scholarly
330   level and theory is the tool that will achieve it. Data do not generate theory. The researcher
331   generates theory. If one's theory stands in connection with earlier advances in theory one achieves
332   substantially for the academic community. (2005, p. 127).

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Resources and Opportunities for CSCL

Four strands of theory and practice within CSCL have the potential to contribute to the development of a critical ethnography.

Critical Design Ethnography

One of the characteristics of critical ethnography is a thoroughgoing rejection of the empiricist concept of the detached neutral observer. Instead, post-modern ethnography

> emphasizes the cooperative and collaborative nature of the ethnographic situation in contrast to the ideology of the transcendental observer. In fact, it rejects the ideology of 'observer-observed,' there being nothing observed and no one who is the observer. There is instead the mutual, dialogical production of a discourse, of a story of sorts. (Tyler, 1986, p. 126)

Critical design ethnography (CDE) is an excellent manifestation of this critique within CSCL. According to Sasha Barab and colleagues,

> the goal is to empower groups and individuals, thereby facilitating social change. In contrast to traditional ethnographic research in which the researcher seeks primarily to understand (not change) the conditions of the community being studied, participatory action research assumes a *critical* stance, in which the researcher becomes a *change agent* who is collaboratively developing structures intended to critique and support the transformation of the communities being studied. (Barab, Thomas, Dodge, Squire, & Newell, 2004 emphasis in original).

Collaboration between the ethnographers, instructional designers, and community stakeholders (parents, teachers, and students) results not only in the "dialogical production of a discourse" (Tyler, 1986, p. 126) but also in the design, evaluation, and implementation of an instructional artifact.

Activity Theory

Activity theory arose from the work of Russian psychologist Lev Vygotsky (1896-1934), whose cultural-historic analysis found that consciousness is located in the activity systems of everyday practice, in the interpenetration of the individual, other people, and artifacts, including language (Vygotsky, 1978). The fundamental tenet of activity theory is the unity of consciousness and practice because "consciousness is not a set of discrete disembodied cognitive acts (decision making, classification, remembering), and certainly it is not the brain; rather, consciousness is located in everyday practice: you are what you do." This belief in the unity of practice and consciousness is mirrored by the activity theory conception of context. Context is not "an outer container or shell inside of which people behave in certain ways." Rather, context is constituted through the enactment of an activity involving people and artifacts. It is simultaneously internal and external to the individual (Nardi, 1996, p. 76).

Ethnomethodology and Conversation Analysis

The field of ethnomethodology (literally "folk methodology") was developed and named by Harold Garfinkel, a student of Talcott Parsons at Harvard, and was elaborated through extensive empirical studies at UCLA (Garfinkel, 1967, 2002; Heritage, 1984). Inspired in part by the work of phenomenological sociologist Alfred Schultz, Garfinkel is particularly interested in the intersubjective, situated, and common sense understanding created between social actors. Because meaning and commonsense understandings are created through interactions between individuals, this knowledge should be "accountable," that is, recognizable and describable in context.

It should be noted that debates about ethnographic theory and practice are complicated by a tendency to conflate the terms "ethnography" (a general term that can indicate a wide range of qualitative research methodologies or a subfield of anthropology) and "ethnomethodology" (a very specific research program originating in sociology). According to Shapiro (1994), "It has become a shorthand or simplification in CSCW to speak of ethnography when in fact what is often involved is ethnomethodological ethnography." (1994:418). Within HCI the ethnomethodological approach is exemplified by Suchman (1987). The field of conversation analysis (Have, 2004), including some forms of IM chat log analysis (O'Neill & Martin, 2003), is also derived from ethnomethodology.

Situated Learning

In *Situated Learning: Legitimate Peripheral Participation*, Lave and Wenger (1991) present anthropological studies of apprenticeship in five different cultural contexts. In each they found that learning is an intrinsically and irreducibly social practice wherein novices become full-fledged experts by participating in communities of practice. Accordingly, "[a] community of practice is an intrinsic condition for the existence of knowledge . . . . Participation in the cultural practice in which any knowledge exists is an epistemological principle of learning." (1991, p. 98-99). Lave and Wenger developed "legitimate peripheral participation," as a consciously challenging concept to capture the complexity of the situated learning process. They do not mean to suggest a distinction between legitimate and illegitimate participation, but rather to draw analytic attention to the "*form* that the legitimacy of participation takes" (emphasis added). In other words, how—and to what extent—does the novice learner feel she has a right and responsibility to participate in a community of practice? Similarly, the term peripheral is not used in a pejorative sense, as in a distinction between center and periphery. Rather, "[p]eripherality, when it is enabled, suggests an opening, a way of gaining access to sources for understanding through growing involvement." Finally, Lave and Wenger do not believe peripheral participation should be contrasted with either "central participation" or "complete participation." They prefer the concept "full participation, " which "is intended to do justice to the diversity of relations involved in varying forms of community membership." (1991, p. 35-37).

Conclusions

Since there are no examples of sustained, in-depth ethnographic analyses within CSCL, we have reviewed Diana Forsythe's (2001) research in the fields of AI and medical informatics to provide a vision of the promise and perils of critical ethnography. Her work demonstrates the subtle interplay between methods, theory and philosophy that characterizes ethnographic work, challenges common sense understandings, and reveals how implicit cultural assumptions become embedded in system designs. However, we have also seen that it is difficult to implement these insights because of fundamental epistemological differences between system designers and ethnographers. Because of positivist misunderstandings about the nature of critical social science research, ethnography seems like a mere methodology that anyone can use. It is hoped that this discussion does not come across as a merely semantic squabble about what should and should not be called ethnography. There will always be circumstances when short-term, descriptive, unstructured qualitative data gathering is a useful and appropriate research strategy. Whether or not one calls this "ethnography" is not the fundamental issue; rather, when CSCL's conception of ethnographic research is *limited* to this decontextualized image we lose sight of the its potential contributions. This trivialization then becomes a self-fulfilling expectation. The growth of DIY ethnography attenuates the potential space for development of a genuinely critical, theoretically-informed ethnography.

The current hegemony of "scientifically based research," particularly in the US, should concern all CSCL practitioners, regardless of theoretical orientation. It must be recognized that SBR does *not* set the standard for all forms of scientific inquiry. In fact, its narrow, positivist, epistemologically oblivious conception of human knowledge represents "nostalgia for a simple and ordered universe of science that never was," particularly in its denigration of narrative and qualitative research (Popkewitz, 2004, p. 62). Unless and until qualitative research is recognized as potentially rigorous and valid in its own right, the development of all forms of critical social inquiry will be impeded. I reiterate Laura Nader and David Hakken's admonitions: This is not a matter of science versus anti-science; instead, we need to project "a clearer, more collective model of alternative ways to practice science, both within anthropology and more generally."

I see many reasons for optimism within CSCL. I've briefly highlighted four strands of current theory and practice: design ethnography, activity theory, ethnomethodology, and situated learning, that have the potential to contribute to the development of a truly critical ethnography of and for CSCL. Although Nyce and Löwgren criticized Suchman's early ethnomethodological research and Nardi's early activity theory analyses as being narrow in scope and not truly ethnographic (1995, p. 41), their recent studies, while not strictly within the realm of CSCL, represent critical ethnographic analyses of collaborative activity (Nardi, 2005; Suchman, 2000). I am also heartened by the field's emerging critique of individualism. The dialectical relationship between the ideology of individualism and the epistemology of empiricism is a matter of historical record (Wood, 1983), and we should not view "the individual" as an innate, unchanging biological or social entity. Similarly, Gerry Stahl has pointed out the inappropriateness of using individualist philosophies of knowledge (e.g. rationalism or empiricism) to study social cognition (Stahl, 2006, pp. 286-290). An explicit connection has also been noted between SBR and the ideology of individualism, the effect of which is "to make us all think about ourselves as individuals who calculate about

ourselves, 'add value' to ourselves, improve our productivity, live an existence of calculation, make ourselves relevant." (Ball, 2001, p. 266).

In 1998 Baber and Nyce offered a rather pessimistic assessment: "The difficulty is that knowledge about the social construction of reality is not the kind of knowledge the development community values, can do much with, or seems to be much interested in." (1998, p. 6). They conclude: "There is, we believe, a demonstrable, fundamental gap between the knowledge the development community values and that which cultural analysis yields. Much of what goes on in social life developers and programmers simply do not see as having any relevance for their work." (1998, p. 10). However, as an emergent field of inquiry explicitly concerned with the collaborative, social construction of knowledge, CSCL has a chance and an obligation to seize the opportunity the larger development community has thus far eschewed.

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