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Chapter 34

Epistemological Encounters in Multivocal Settings

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Abstract (for e-Book ONLY!)

Researchers usually work and evolve in the scientific frameworks in which they were trained, without questioning their epistemological foundations. However, this may be required when researchers coming from different disciplines and paradigms try to work together on the same object of study. This chapter reflects on epistemological encounters in a five-year project of multidisciplinary collaborations in the analysis of interaction. We argue for maintaining diversity of epistemological traditions while either achieving complementarity within explanatory frameworks on different levels or maintaining productive tension. We then present the extent to which researchers in our project and a similar project encountered each other's epistemologies when they compared their analyses of shared corpora. The majority of comparisons in various contexts led to engagement between epistemologies and some of these epistemological encounters were productive and glitch free, others had difficulties, but still led to productivity, while still others led to missed opportunities and in one case to radicalizing incommensurable stances. A minority of comparisons in other contexts did not lead to engagement, but could either still be fruitful or not productive at all. In conclusion, we summarize the consequences of engaging with epistemologies through the comparisons researchers make of their analyses in multivocal contexts, showing how epistemological encounters can help to bridge between isolated traditions that work on similar objects of study.

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Introduction

What makes it difficult for a heterogeneous group of differently trained researchers to collaborate? They may not share the same assumptions about the nature of scientific knowledge, the role of theory, their research object, or how to gather and represent data. They may not agree on the appropriate relation between researcher and data, about how a particular analytical construct should be defined, or about what constitutes relevant units of analysis. They may not share the same bases for value judgments they make about their data. They may also differ in their opinion on which methods to apply, how to apply them or about how to validate their results. These assumptions and others are anchored in a researcher's epistemology. An epistemology is a logos, or reasoned discourse on *epistémé* or the nature of knowledge and how it can be acquired. An epistemology is learned in the early years of research training, and it is often learned implicitly; its consequences are not always explicitly laid out. Sometimes a researcher may not be aware of his or her epistemology and only later discover its constraints through discussions with researchers with different epistemologies. This chapter reflects on epistemological encounters in a five year project of multidisciplinary collaborations in the analysis of interaction (Suthers, Lund, Rosé, & Teplovs, Chapter 31, this volume).

How can different epistemologies encounter each other?

Typically, if researchers have contrasting epistemologies, we think of them as being from different disciplines (e.g. linguistics and psychology), but they can also be from the same general discipline (e.g. generative linguistics and interactional linguistics). Situations in which researchers desire to collaborate, but where they have contrasting epistemologies, are multivocal and have the potential to be productive. Van den Besselaar & Heimeriks (2001) take a narrow and homogenous view of a disciplinary research field where "a group of researchers [work] on a specific set of research questions, using the same set of methods and a shared approach (op. cit., p. 706)". They argue that there are different ways of combining elements from various disciplines (and frameworks within disciplines) in order to get them to productively interact. Such interactions can take many forms, ranging from communicating and comparing ideas, exchanging data, methods and procedures to mutually integrating concepts, theories, methodologies and epistemological principles (op. cit., 2001). However, since researchers most often gravitate towards interactions within their disciplinary tradition's boundaries, it is not uncommon that different communities of researchers work separately on similar objects of study without benefitting as much as they could from each other's efforts. We maintain that this is not an efficient use of resources at the community level. Our efforts towards multivocality in this book project were meant to explore ways of engaging communication between different traditions.

In this chapter, we argue for maintaining diversity of epistemological approaches while either achieving complementarity within explanatory frameworks on different levels or maintaining productive tension. We then present the extent to which researchers in the five case studies comprising our project encountered each other's epistemologies when they compared their analyses of shared corpora. We also include two examples from a similar

project. Some comparisons in specific contexts (e.g. some research results, tool use or the alignment of representations) did not lead to engagement between epistemologies whereas other types of comparisons in other contexts (e.g. other research results, units of analysis, analytical objective, whether or not categorizing interaction is seen as acceptable, analytic constructs, the notion of what counts as data, the relation between researcher and data, validation of research) did lead to engagement between epistemologies and in one case to radicalizing incommensurable stances. These epistemological encounters (whether they led to epistemological engagement or not) were productive in either a seamless way, in a way that began with difficulties or in ways that could be construed as missed opportunities.

Relating epistemologies within a multivocal approach

In research on group interactions, as elsewhere, the nature of a researcher's epistemology affects his or her dialogue with other researchers. So, what happens when people holding different theories confront their epistemologies around the analysis of a shared artifact? If all of the differences that define research traditions were starkly contrasted and if all researchers held to their epistemologies with the utmost tenacity, then discourses within disciplinary traditions would be incommensurable, and our efforts towards multivocality fruitless. What we have found, however, is that if researchers are open to engaging with others over epistemological issues, then they can find ways to gain insights from each other — or from the process of engagement — even if they agree to disagree.

We lead you now on this journey with us. Why, you might ask, would we want to take the trouble to try and relate one epistemology with another? Researchers who study group interactions come from a large variety of disciplines, use many epistemological frameworks and don't necessarily interact in the same research communities, so it makes sense to develop a forum within which to examine epistemological frameworks that strive to take into account similar empirical data or seem to manipulate similar concepts. The premise of our project is that it is productive to compare epistemologies through multiple analyses of shared corpora, without necessarily having a specific common goal. Doing comparative work between theoretical frameworks generates fundamental questions for the scientific communities involved, for example how do theoretical assumptions drive research? In addition, combining theoretical perspectives has the potential benefit of bridging between similar areas of literature that are traditionally isolated, and showing why it is interesting for two research traditions that don't usually communicate to work together.

Keeping the diversity

Our goal is not to merge neighboring theoretical perspectives into a single "super theory" that would account for all aspects of group interactions in a coherent way. On the contrary, diversity in theoretical assumptions and methodological approaches is an unavoidable and desirable characteristic of a multidisciplinary research community. Indeed, diversity is one of the strengths that we wish to maintain since dialogues about analytical constructs between researchers that differ in their ontology and epistemology are particularly enlightening (Abend, 2008). Stahl (2011) points out that the study of group interactions reveals distinct phenomena at different levels of description, and that because these phenomena interact with each other in complex ways — notably in Computer Supported Collaborative Learning (CSCL) settings — it is highly likely that CSCL requires multiple theories so that different aspects of the interaction can be studied at different levels (micro, meso, macro) and at different time scales with methods of investigation that are appropriate for the questions posed.

Different explanatory frameworks can be complementary

Sometimes working at these different levels of description means that we only accept a particular type of explanation as valid and that we are not sensitive to other types of explanations at other levels of description. Indeed our historically anchored disciplinary training teaches us to distinguish between better or worse explanations and to prefer certain types of explanatory schemata (Morange, 2005) in which these explanations exist, but sometimes, "competing" explanations that occur at different levels can both be true. For example, there are two possible explanations for migratory phenomena. A bird migrates because climatic or daylight changes trigger physiological modifications in the bird's organism. It also migrates because moving elsewhere will bring it more food, thus favoring its survival and reproduction capacities. The first is a proximal cause, understood by mechanical explanatory schemas from biochemistry, molecular biology and physiology and the second is an ultimate cause, understood by natural selection and Darwinian explanatory schema. Both explanatory schemas contribute to understanding the phenomenon of migration, but give a more complete picture when combined. Similar combinations of explanatory schemas can be done for the study of group interactions.

The rest of this chapter is organized as follows. We analyze the extent to which researchers engage with each other's epistemologies when they compare aspects of their analyses of group interactions in a multivocal setting, both in this project and in another similar setting. We discuss what might be behind the fact that certain comparisons lead to a researcher engaging (or not) with another's epistemology. The majority of comparisons in various contexts led to engagement between epistemologies and some of these epistemological encounters were productive and glitch free, others had difficulties, but still led to productivity, while still others led to missed opportunities and in one case to radicalizing incommensurable stances. A minority of comparisons in other contexts did not lead to researchers encountering each other's epistemologies, but could either still be fruitful or not productive at all and these are the examples we take up first, in the next section.

Comparisons that do not lead to epistemological encounters

Three examples are discussed in this section, one from the Origami Fractions section (Part 2), one from the Asynchronous Knowledge Building section (Part 5) and one from the Multimodal Electricity section (Part 4). The first two examples are productive interactions, despite the fact that they do not lead to epistemological encounters and the third was a very problematic interaction, precisely because epistemological concerns were not addressed.

Reinterpretation of another's results is compatible with ones own epistemology

In this first example, Shirouzu (see Chapters 4 and 5, this volume, for presentation of data and the data provider Shirouzu's analysis) is able to enrich his view of group interaction by reinterpreting Chiu's results (Chapter 7, this volume) in his own framework. First, Chiu performed new analyses with his Statistical Discourse Analysis (SDA) method that focused on the class discussion activity phase of the paper-folding pedagogical task on fractions (2/3x3/4=?) after understanding that Shirouzu had a special interest in it. Shirouzu then demonstrated that he was able to match new meanings to Chiu's interpretations of pivotal moments (occurring in Chiu's framework) that were relevant to Shirouzu in his own framework. For example, Chiu viewed a particular pivotal moment as indicating the end of a period of frequent ideas, occurring just after a teacher acknowledgment. First, Shirouzu noticed that this moment could also be considered as a collective display of new understanding, something that he had not seen previously. Indeed at the moment when collective understanding is reached, a drop in new ideas could occur because learners are

consolidating their knowledge in terms of concepts already expressed. Secondly, re-examining this moment in terms of Chiu's definition of ideas as "new" or "old" led Shirouzu to suggest that in his own framework, new ideas could correspond to conceptual or procedural changes of how to view the solutions (e.g. as a physical area, or as an algebraic equation), progressing potentially towards a collaborative pivotal moment. Third, Shirouzu noticed that Chiu's five breakpoints corresponding to frequency of new ideas also corresponded to when and how the pedagogical designer's intentions were actualized by students' behavior. This example shows that when researcher A studies moments considered pivotal by researcher B in B's framework, but that A does not originally consider as pivotal, researcher A can appropriate these new pivotal moments to be meaningful in his own framework. Such appropriation proceeded without difficulty because there were no fundamental differences in the researchers' epistemologies.

Implicit epistemological compatibility allows researchers to focus on tool integration

In the Asynchronous Knowledge Building section (Part 5), there were no fundamental epistemological incompatibilities between the three groups of analysts. Teplovs and Fujita (Chapter 21) and Law and Wong (Chapter 22) are representatives of the knowledge building community. Chiu (Chapter 23) argues that his Statistical Discourse Analysis (SDA) can be used with multiple theoretical frameworks, but his methodological assumptions show that he subscribes to the theoretical assumptions of social metacognition, likened by Fujita (Chapter 24) to the knowledge building principle of collective cognitive responsibility. All three groups strove to innovate new techniques to inform future work. Researchers were also stretched to imagine how the other analytic tools used in the section could inform their understanding of their own approaches. For example, Law & Wong could imagine how the KISSME analytic tool (Chapter 21) could serve to better identify semantic markers of interest to them. Conversely, Teplovs imagined how the use of keyword sets derived from the Law & Wong approach could be used to improve the analytic capabilities of KISSME. Chiu's SDA could be enhanced by using the latent semantic analysis capabilities of KISSME to aid in ensuring notes were correctly tagged, and Chiu's SDA could be used to identify breakpoints for examination from the perspective of discourse markers (Law & Wong) or network analysis (Teplovs & Fujita). It seems reasonable to argue that this example illustrates how the analysts' epistemological compatibility ensured that their diverse higher-order goals could be combined together, all involving ways of improving knowledge building.

Difficulty in aligning analytic representations due to different transcript needs

As seen in the Group Scribbles data section (Chapters 14-19, this volume), there is a tension between the need to align analytic representations for comparison purposes and for each analysis to have representations optimized for their own purposes. For example, some analyses need uniform sampling such as 30 second intervals, some work at the granularity of recognizable acts, and some also work at granularities of episodes defined by participant activity but also in relation to the focus of analysis (e.g., inscription-construction, artifact-manipulation, or multimodal reformulation episodes). These different representations may be aligned for comparison purposes if there is a common dimension of reference, such as time in a shared video data source. But even if so aligned, conclusions may be attached to units of activity that do not coincide exactly. However, as discussed in Chapter 19 (Suthers, this volume), one can learn as much from the mismatches between analysis-specific representations as from comparisons concerning the conclusions of representations that have been carefully aligned. That being said, had the researchers engaged early on in relation to what they considered as adequate data for the assumptions they held about learning interactions, perhaps all the troubles they experienced concerning the transcript and the

subsequent difficulties in aligning the representations that were inferred from them could have been avoided. They would have then been freer to engage in discussions about what could possibly be considered as more fundamental issues, such as how to qualify the learning going on.

Summary of comparisons that do not lead to epistemological encounters

In conclusion, in this section, we illustrated both how multivocality could be productive even when researchers do not specifically engage with others' espistemologies, but also how not engaging can lead to difficulties that prevent researchers from collaborating effectively on deeper issues. In the first example, a researcher's reinterpretations of another's results were possible, largely because his epistemology allowed them, even if this coherence was never made explicit. In the second example, implicit or latent epistemological compatibility allowed for seamless reflection on integrating tools. In the third example, a lack of discussion concerning what researchers needed from the data had the consequence of making analyses and comparison difficult, thus taking energy away from discussion involving comparisons. However, the struggles to get into a position to compare – for the most part led by the discussant of the section – led that discussant (and not the analysts themselves) to pinpoint discrepancies in analyses (cf. the sections below entitled 'Interrogating the underlying epistemological assumptions of claims about learner agency and learner activity' and 'Interrogating the underlying epistemological assumptions of claims about the evaluation of learning'.

Comparisons that do lead to epistemological encounters

Eight examples are discussed in this section. The first two come from the Origami Fractions section (Part 2), the third comes from Part 5: Designing Biology, the fourth from Part 3: Peer Teams Chemistry, the fifth and sixth examples are from Part 4: Multimodal Electricity and the last two examples are from a project different from the one in this book, yet very similar called the "MOSAIC" project, described when we get to those examples.

Missed opportunities for debating changes to key analytic constructs

In this first example, we show how the comparison of Shirouzu's (Chapter 5, this volume) and Trausan-Matu's (Chapter 6, this volume) pivotal moments lead to Trausan-Matu integrating aspects of Shirouzu's viewpoints into his own *problématique* in two different ways. The collaboration described in this book introduced Trausan-Matu to the analysis of transcribed oral conversations, encompassing both talk and gesture, a type of corpus he had not focused on before. Adding gesture to his analysis of human interaction amounted to extending the domain of application of his PolyCAFe tool but more importantly to extending the concept of Bakhtin's 'voices' to include gestures (both communicative and technical, the latter referring to manipulation of the origami paper); this was the first way he modified his *problématique*[1]. We interpret this re-conceptualization of 'voices' to mean that when Trausan-Matu was confronted with a corpus that presented forms of interaction he was not used to analyzing (i.e. gestures), he was able to re-consider the types of data he took into account as important for understanding learning and to integrate them into his theoretical and methodological framework. This change in conceptualization illustrates how closely related our theoretical frameworks are to the nature of the data we analyze. We argue that in this example theoretical convergence occurs in that Trausan-Matu widened Bakhtin's framework in order to take into account new types of corpora and by doing so, came closer to Shirouzu's framework.

[1] "Problématique" is a French word used to name a coherent set of problems and assumptions. It provides a coherent framework to express problems, why it is interesting to solve them and how the current research described is able to do so. This term is not a synonym for the English word "problematic".

However, there was no one else in the fractions group who was expert in Bakhtin's framework and so the act of modifying the concept of "voices" to include gestures was not debated from an epistemological standpoint.

As a result of engaging with the patterns Shirouzu defined in his analyses, Trausan-Matu modified the parameters of two other conceptual terms, but in the framework of conversation analysis; this was the second way he modified his *problématique*. Trausan-Matu extended the definitions of both 'utterance' and 'adjacency pair' to reflect the inner speech that Shirouzu included in his analyses. Including inferred inner speech that fit with the context of the interaction (e.g. talk, gestures, manipulation of origami paper, and writing on the blackboard) allowed both Shirouzu and Trausan-Matu to constitute coherent stories about how the interaction played out. For Trausan-Matu, utterances were now not only verbal, but could also be inferred thought as well as different types of actions and instead of being essentially individual or co-elaborated, they could be group generated, such as all students moving their chairs to move closer to their origami papers, in chorus. Pairs of utterances were considered to be "adjacent" even if shared ordering could just be inferred, for example between an external utterance (talk or action) and an internal one that was presumed by the researcher to be "thought" by the learner. In contrast to the modification of the concept of 'voice' in the Bakhtinian framework, both Shirouzu and Trausan-Matu were challenged in the discussions going on in that section to explain how they backed up their claims about inferred speech. However, submitting such a radical change of definitions of utterance and adjacency pair to the larger conversation analysis community is another matter. Both this and the previous example illustrate a danger that multivocality may lead to the modification of existing analytical concepts without explicitly taking into account the epistemological assumptions that underlie these terms. It seems likely that changing the definition of analytical terms already in widespread use will hamper researchers' ability to effectively communicate.

Comparing pivotal moments leads to epistemological modifications that enrich analyses

In this second example, after discussing how each of them defined pivotal moments with both Shirouzu and Trausan-Matu, Chiu (Chapter 7, this volume) decided to look at the context around which his pivotal moments occurred (thus enlarging his unit of interaction). He extended his notion of a pivotal moment to beyond the single turn, seeing that a single turn could be interpreted in relation to what comes before and after. This extension had two consequences. The first is that Chiu broadened his understanding of why his breakpoints were pivotal using qualitative analyses to supplement his quantitative Statistical Discourse Analysis approach and the second is that he thus became more convinced of how quantitative and qualitative methods can be used in concert to obtain more complete understanding of group interactions. In a sense Chiu maintained two visions of a pivotal moment, one that encompassed a single turn, which he needed in order to perform his analysis that located breakpoints in an interaction, and a second that took into account the context around a breakpoint in order to better understand it qualitatively. Chiu saw that a detailed qualitative analysis of the interaction around the pivotal moment could uncover hypothesized mechanisms that change the interaction; these could be searched for across case studies in order to determine their hypothesized robustness and then specified through operationalized variables in order to be statistically tested. This example illustrates how a researcher can adapt mixed methods for deeper understanding.

Opening up to Engagement with Different Epistemological Assumptions

In the Biology section (Part 6), a variety of methods were applied to the analysis of a data set produced in a first iteration of a Design Based Research cycle. In order to test their

hypotheses, researchers who provided the data applied quantitative summative evaluations and coding and counting process analyses. Other researchers applied ethnographic, ethnological, and SNA methods to the analysis. At the start of the multivocal process, while the researchers were all aware of the epistemological underpinnings of their approach, researchers were less aware of a much more subtle difference in theoretical assumption, namely the assumed idealized role of a facilitator in a collaborative learning interaction. This difference played a much greater role in the interaction between researchers.

Quite orthogonal to the diversity of methodological approaches represented by the analytic team, the collection of analysts brings together two different research communities, one from classroom discourse where we get the Academically Productive Talk (APT) framework (Resnick, O'Connor, & Michaels, 2007) developed largely from research on primary school learners and whole classroom interactions facilitated by teachers; and another from CSCL where we get theories of problem based learning (Hmelo-Silver & Barrows, 2006) and group cognition (Stahl, 2006), both of which have largely been developed from analyses of older learners, and in the case of problem based learning, largely advanced learners (i.e., medical school students). Between the two communities, much is shared in terms of desired characteristics of the student interactions. Correlational analyses, largely from within the collaborative learning research community, offer empirical support for the value of certain characteristics of interactions between students for triggering learning processes. In the CSCL community, there has also been a large amount of research on how to achieve these kinds of interactions in small groups of students (e.g., scripted collaboration as well as study of expert PBL facilitation techniques). The PBL facilitation research is the easiest to directly compare with APT since it involves characterization of rhetorical moves used by humans to engage groups of students in discussion. Much can be found in common, for example, between the Hmelo-Silver work on PBL facilitation and specification of APT moves. However, if one looks deeper into the assumptions about the ideal positioning of the facilitator, one finds that within the PBL facilitation viewpoint, as in the Group Cognition framework, the idealized role of the facilitator is much more minimal than in an APT classroom where the instructor plays an integral role throughout the discussions, sometimes offering nearly half of the contributions that make up the discussion, while carefully self-locating outside of the interaction between the students so that student reasoning is kept at the center, and students maintain an authoritative footing within the interaction. Whereas in Stahl's view, the teacher should get out of the way, in APT, the teacher is constantly an integral part of the interaction. This view of the idealized role of the teacher colors the way three of the chapters evaluate the work.

The work on the Biology conversational software agents (see Dyke et al., Chapter 25) did not begin with the goal of addressing the question of the ideal role of a facilitator. In fact, the data providers were not fully aware of the extent of the distinction in views until the multivocal process had begun. Instead, their goal was to accomplish several things within the theoretical framework of APT, a goal that was not successfully communicated to the other analysts at the inception of the multivocal process.

Empirical validations of the theory of APT show that when expert teachers use APT facilitation with whole classes, the classes achieve high test scores across subject areas, and maintain their advantage for years (Resnick, Asterhan, & Clarke, in press). Around these studies, a belief about the mechanism for the effect has emerged but never validated through careful experimentation. The treatment has always been complex, involving multiple facilitation moves used with whole classes, where a human teacher insightfully decides when and with whom to use each move. Thus, it is not clear whether there are differential effects across the individual facilitation moves, or whether there are preconditions either at the group

or individual level for their effects. Furthermore, there is a belief about the connection between results on achievement and impact on identity, motivation, and affective dimensions, although this had never been formally tested. The series of controlled studies with small groups of students that involves APT agents was meant to fill this empirical gap, giving the data providers the opportunity to carefully manipulate how and when each move was used, and investigating the effect on cognitive, motivational, and social dimensions and how those effects interact with individual differences between students and with group composition characteristics.

In the Howley et al. analysis (Chapter 26), the role of the agent as an APT instructor is taken for granted, and the evaluation is with respect to how successful that intervention was in achieving the desired end in the interaction itself, with the goal of answering the research questions as outlined above. The Stahl and Goggins analyses (Chapters 28 and 29, respectively) took a different view – rather than taking the role of the agent for granted, they assumed an idealized minimal role for the agent and then evaluated the interactions in terms of how minimal the role of the agent was. The Stahl analysis ignored the differences between conditions, whereas the Goggins analysis did look for differences between conditions. The Cress analysis (Chapter 27) ignored the agent altogether and instead focused on the environment and task setup. So that chapter did not deeply participate in the multivocal discussion about the role of the agent, although the analysis was nevertheless useful for informing the redesign.

In connection with the issues we began this section with, consider in particular Stahl's criticism of the software agents as dominating the group discussions and getting in the way of student interactions (Chapter 28). In Stahl's analysis, the data was first formatted in the columnar representation suggested by Ochs (1979), and then analyzed as an uptake network structure with the goal of illustrating how participation in the conversation was distributed. Interpretation of the agent as "getting in the way" came from a count of the number of words in the agent column relative to those in the student columns as well as a representation of the focus of attention layered on Stahl's hand constructed graph representation. The analysis was conducted in a generic way, without separating out phases of the discussion that were set apart in the task design for different purposes and where students and the tutor agent were intended to play different roles. Arguments in Stahl's analysis are made as interpretations of interactional patterns within uptake networks and the particular layout of the transcription, both of which are displayed in Stahl's chapter. Although only a single transcript was selected for the main analysis presented by Stahl, a causal interpretation of the interactional structure, i.e., that the agent's role taking as interpreted by Stahl was the cause of other observed patterns in student interactions, is inferred in Stahl's analysis, where this would not be warranted in other epistemologies. These interpretations begin with mostly unstated assumptions about the meaning behind interactional structure (i.e., in terms of value judgments about the facilitator based on positioning in the uptake network). Thus, identification that the desired pattern is not present is then interpreted by Stahl as a failure and a cause of other undesirable behavior identified within the same interaction, where it might be interpreted differently using other analytic lenses.

At first, the data providers were frustrated with the chapters from other analysts because they seemed to be operating within a different theoretical plane – the other analysts were not trying to answer the data providers' questions and did not offer them insights related to those specific questions. But what they did do was question the theoretical assumptions the data providers were making about what the role of the facilitator should be. In the case of Stahl and Goggins, there was a direct challenge to the teacher to "get out of the way". In the case of Cresse, it was more a process of questioning whether the environment was conducive enough

to learning and collaboration that we could get a meaningful read on whether the manipulation worked in the first place. One could see all of these chapters as questioning whether the data providers were really asking the right questions to begin with.

In the end, the data providers reluctantly took the hard feedback to heart. Their own analysis did point to places where their intervention was getting in the way through poor timing. Problems with the agent's timing were indeed pointed out in multiple places throughout the analysis chapters. Addressing these issues with a new architecture (Adamson et al., 2012) was one major technical advance between the first study, which provided the shared dataset, and the subsequent studies, which were more successful in terms of producing learning gains and other positive effects. In the data providers' redesign, they also scaled back the extent to which the agents were intervening in the discussion in the subsequent, much more successful trials. Even with the scale back, however, they have hung on to the basic moves from the APT theoretical framework (Dyke et al., in press; Adamson et al., in press; Clarke et al., in press) and have not changed their commitment to an APT style ideal facilitator. As the data providers have continued to investigate the impact of the facilitation moves on learning and motivational variables like self-efficacy (Howley et al., 2012), they have found results that validate the line of questioning they began with. In particular, the results have called into question the simplicity of the APT theory's model of facilitation as increasing self-efficiacy through positively positioning students as authoritative in interaction. Instead, they find differential effects of the moves depending on student ability and self-efficacy. In connection specifically with multivocality, this example shows that while it was initially frustrating to get advice and feedback that seemed to be ignoring the theoretical framework the Data Providers were working within, researchers coming from other theoretical frameworks were able to look at what they were doing with less of a tunnel vision and were therefore in a better position to push them to reconsider things that might have hindered them from answering their questions if they had not stopped to make adjustments to their experimental setting.

Alternative operationalization brings out different aspects of a complex analytical construct

In the Chemistry section (Part 6), we observe a similar situation to the Biology section with respect to fine-grained distinctions in operationalizations, but one that played out more smoothly. Here we compare the two processes and work towards understanding why they played out so differently.

The team of analysts who worked on the Chemistry dataset was diverse methodologically in the same way as the Biology team. In particular, one analysis team (led by Keith Sawyer, Chapter 10, this volume) took an ethnographic approach. Another team (led by Jun Oshima, Chapter 12) took a social network analysis approach. And two teams (led by Carolyn Rosé and Jan-Willem Strijbos respectively, and reported in Howley et al., Chapter 11) took a coding and counting approach. Despite these similarities between the two multivocal processes in terms of team composition, however, the circumstances of the data collection for the Chemistry effort were quite distinct from the design based research process that provided the context for the Biology data collection. This distinction in turn led to a very different dynamic in terms of the communication between analysts and data provider.

In particular, the goal of the Chemistry data collection was to explore a process that was tried and true (i.e., Peer Led Team Learning) and to understand better how it was working rather than to evaluate the first instantiation of an intervention that was at an early stage and determine how to make it better. Thus it was natural for the process to be framed as a casual discussion about what different lenses applied to the data might teach us about the reasons why the intervention may have been working. The data providers for the Chemistry data had

already completed their analysis and were satisfied with the answers they had found to their own research questions. From the data they collected and had examined, they selected two focal groups for the purpose of making an interesting contrast rather than evaluating the efficacy of the intervention. Because of the small size of the dataset and the absence of any experimental manipulation, there was no implicit invitation for analysts to provide a value judgment on the intervention. This stands in contrast to the Biology data effort where the purpose of the study was clearly to evaluate an intervention that was manipulated experimentally in the data, and the whole corpus was shared, with all flaws exposed. Considering this contrast, it is not surprising that most of the analyses of the Biology corpus were framed as evaluations of the quality of the intervention, and the focus of many of the write ups was on what went wrong and what the authors thought the data providers should have done differently.

Among the analysts of the Chemistry data, a common lens used to facilitate discussion among groups was the idea of leadership and role taking within groups. Similar to the Biology collaboration, the analysts brought in to their work unvoiced assumptions about leadership roles and what they look like. These subtle distinctions in unvoiced underlying assumptions went unnoticed at first when discussions were at the level of broad strokes discussion about the style of interaction within the two student teams whose discussions were the focus of the analysis. However, when conclusions about individual students and their role taking within the interaction was compared across analyses, differences in conclusions led to line by line comparisons, which in turn eventually led to insights about distinctions in definitions. Upon reflection, the distinctions that were revealed led to interesting discussions about how expansive and complex the idea of leadership is.

The interaction around the questions of what leadership really is, including comparisons across operationalizations, were productive. The differences did not lead to conflict or friction among the analysts. The contrast to other case studies might have stemmed from the fact that evaluation of who was taking a leadership position in groups was not as value laden in this analysis. The distinctions in operationalization did not carry implications for design, per se, and did not reflect positively or negatively on anyone's work. Operationalizations themselves are not right or wrong. Instead they are either successful or not at capturing particular phenomena faithfully. There are always choices to make in operationalization, and differences across alternative operationalizations offer the opportunity to triangulate. A researcher may evaluate the validity of another researcher’s operationalization. However, while discussions surrounding the patterns found in the Chemistry dataset centered on the idea of leadership in a number of interactions between analysts, none of the analysts were particularly invested in convincing others to see the role taking in the interactions in any specific light or saw the value of their constructs in terms of the extent to which they could convince others to view leadership taking the same way. The fact that one researcher’s alternative operationalization brings out different aspects of a complex construct like leadership does not necessarily detract from the value of an alternative operationalization that brings out other facets that are also interesting. Thus, the discussion may proceed comfortably for all despite the disagreements that come up.

The contrast between what happened when researchers compared analyses from the previous Biology example and in this Chemistry example highlights the importance of taking care in how data is shared in a multivocal process, with clear communication about goals and expectations conducted up front so that the exchange can be comfortable and productive for all.

Interrogating the underlying epistemological assumptions of claims about learner agency and learner activity

Epistemological comparisons need not only be grounded in comparisons between the conclusions of analyses. They can also be grounded in comparisons of representational devices and empirical claims that evidence how analyses constitute the object of study in the first place. For example, the discussion of the Group Scribbles analyses (Suthers, Chapter 19, this volume) examined the units of agency and activity about which analysts made claims. Such groundings begin close to the data, but uncover epistemological assumptions that may not be discussed explicitly by researchers. Some analyses (e.g., Lund & Becu-Robinault, Chapter 17, this volume) focus on individuals' acts (reformulations across modes and media) to characterize their conceptual understanding: they clearly focus on the individual as the agent of learning. Others (e.g., Jeong, Chapter 18, this volume) do not track individuals at all, taking artifacts produced by individuals as evidence for collective or group understanding: the group is clearly the agent of learning. Some interesting nuances can be found by comparing how analyses characterize sequences of events across media. For example, Looi et al. (Chapter 15) and Lund & Becu-Robinault both examine how content changes as it is expressed in one medium and then another, implying that activity takes place in a given modality and diachronic features (change over time) are most important for understanding learning; while Medina (Chapter 16) discusses how events taking place synchronically or nearly simultaneously in multiple modes converge to constitute a single activity, implying that there is agency at the group level and activity is simultaneously distributed across modalities. Some of these analysts did not state these epistemological positions explicitly in their chapters: rather, we uncovered these positions by comparing the units and relations of their analysis. We should add that these comparisons draw conclusions about analyses, not about researchers. For example, other discussion and citations by Lund & Becu-Robinault shows that they hold a synchronic view of activity as well as the diachronic view exemplified by their analysis. This example shows how the units of agency and activity that researchers used in their analyses actually *embody* their underlying epistemological assumptions. The first assumption concerns where the researcher is looking for learning — at the individual or group level, or using a combination of both. The second assumption concerns how the researcher conceptualizes learning. This is discussed more specifically in the next section.

Interrogating the underlying epistemological assumptions of claims about the evaluation of learning

Comparisons of analyses of two different data corpora from the Group Scribbles setting (Looi & Chen, Chapter 14, this volume) both exposed epistemological differences on the part of analysts concerning criteria for evaluating the quality of participant activity. Analysts first analyzed a Group Scribbles corpus on fractions that was ultimately not used for this book. As discussed in Chapter 19 (this volume), an analyst (van Aalst) approaching the data from the theoretical perspective of Knowledge Building found the data uninteresting, as students' verbal discussion did not display evidence of students taking control of the learning opportunity. Simultaneously, another analyst (Medina) influenced by ethnomethodological and conversation analytic traditions analyzed students actions in the workplace to show how a graphically-expressed proposal was contingent upon the setting in a manner evidencing the development of shared representational practices. These different assessments are not merely due to one analyst having analyzed only talk while the other analyzed workspace actions. There is a fundamental difference in whether student interaction should be approached with theoretically driven standards, or whether instead this interaction should be taken on its own terms as displaying organized group participation. A similar epistemological encounter occurred in the electricity data corpus analyzed for this volume. Lund and Becu-Robinault

(Chapter 17) evaluated student actions in terms of whether individuals displayed understanding of conventions, such as for diagramming electrical circuits, that are taught in school and used by professionals, while Medina (Chapter 16) evaluated student actions in terms of how group activity led to innovations. Consequently and as discussed in Chapter 19, their assessments of one particular individual, "Bruno", differed. Lund and Becu-Robinault found that Bruno failed to exhibit a canonical representation of how a wire connects to a battery in a diagram, while Medina found that Bruno's innovation of stacking batteries on top of a wire was one of the various contingencies enabling a group innovation, "two batteries, two bulbs". Again, the difference is not only on whether we focus on diagramming or building circuits, but also in analysts' fundamental epistemological criteria: should student acts be evaluated in terms of pedagogically driven criteria or by the internal logic of their collective accomplishments? This example delves more deeply into how a researcher's way of evaluating learning reveals his or her epistemological assumptions. One possibility shown in this dataset (cf. the Lund and Becu-Robinault's analysis) is to define learning as change over time where the focus is on the learner's evolving capacity (but as a member of a group that may or may not exhibit this capacity collectively) to be able to translate domain concepts from one representation to another while respecting taught domain knowledge. This desire of the researcher to track such an evolving capacity does not deny that learners also use different modalities simultaneously when they (co)construct knowledge in groups. A second way of conceptualizing learning in this dataset (cf. Medina's analysis) focuses more specifically on the group and describes how converging acts in multiple modalities and media are brought together to accomplish the group's activity, without evaluating the knowledge being constructed from any domain standard.

Integrating methods on the basis on compatible epistemologies

In the remaining two sections, we take examples from the MOSAIC project, financed by the French National Research Agency. This project was also multivocal, involving psychologists (of different specializations) and linguists (working on conversation analysis). Although its primary goal was to understand collaborative design processes in the domain of architecture (see analyses in Détienne & Traverso, 2009), its secondary goal was to specifically confront methodologies around an analysis of shared data. The researchers involved intended to construct a bridge between theoretical and descriptive research on interaction carried out in the language sciences on the one hand and studies of cognition and dialogue in complex collective design activities carried out in ergonomics on the other. During one collaboration within the MOSAIC project, it proved possible to integrate a socio-cognitive interactionist approach with a cognitive ergonomics approach, and this was to a great extent because of similar epistemological positions. In that collaborative effort, a discursive dimension accounting for argumentative and enunciative[2] activities was analyzed together with an epistemic dimension that accounted for the intermediate design products as well as the knowledge mobilized during these activities of elaboration and reconstruction (Baker, Détienne, Lund & Séjourné, 2009). Both parts of this integrated analysis were built up from coding the interaction, based on a priori categories gleaned from the literature and that were confronted with the data. In this example, although the psychologists represented different specialties, they all had a similar epistemological approach to studying group interactions and so integrating their approaches was seamless. For instance, they all agreed that researchers could define analytical categories, in relation to theory and research questions and then refine them in relation to part of the corpus. They also agreed that the researcher's task is to observe and analyze data, in order elaborates theories and models of the data set. They also agreed that

[2] Ducrot defines an enunciator as the instance of *the source of a viewpoint* expressed in the propositional content of an utterance (Brandt, 2013)

the validation of research concerns so-called objective markers, indicators of categories, and intersubjective agreement between independent coders.

Recognizing incommensurability radicalizes researcher positions but also makes researchers more aware of their constraints

In another collaborative effort within the MOSAIC project, it proved impossible to actually integrate the interactional linguistics approach with the cognitive ergonomics approach, largely because of their differing epistemological positions. For instance, whereas for the cognitive ergonomists, data selection was in large part determined by theory and model, the interactional linguists attempted to take into account the minute details of interaction in a way that was not conditioned by prior theorization. The two approaches do not agree on what constitutes "the corpus" and it is arguable that agreeing on what constitutes the corpus in the first place can more quickly allow researchers to compare their respective analyses (and access deeper conceptual issues) because they will be able to collectively refer to the same parts of the empirical material. On the other hand, the very act of deciding what the corpus should be obligated the researchers to be specific about their epistemological positions regarding what they needed to answer the questions that interested them and that were pertinent to ask in their respective theoretical frameworks. In general, the act of comparing their respective methods led to the cognitive ergonomists and interactional linguists detailing the very specific differences that illustrated the consequences of foci stemming from epistemological positions and these led to defining "zones of maximal analytical vigilance" (Traverso & Visser, 2009: p. 169), where researchers had to be particularly careful in respecting their methodological constraints. For example, the interactional linguist worked to make her description of the interaction coincide with how (*she understood that*) the participants themselves interpreted and demonstrated the interaction, and although the cognitive ergonomist's descriptions were also formed from the activities of the participants, she recognized that her analysis was a personal construction (differing perhaps from other colleagues' descriptions), colored by theories and models she would render explicit. Her descriptions included inferences that were based on activities that were implicitly present within the interaction, but that could be argued to be present, based on observables (much like Shirouzu and Trausan-Matu argued in the Origami Fractions section, Part 2). The crucial question here is the extent to which researchers are able to substantiate their analytic claims. Both cognitive ergonomists and interactional linguists claim to base their interpretations on observables, but the difference seems to occur on two intersecting planes. The first is the extent to which an object, event, or phenomenon can be considered observable. Is being "observable" some kind of proxy for "objective" or is it impossible to separate observing human interaction from our own human experience so that we necessarily both view and make inferences about it? And the second is the extent to which analyses of human action are effectively grounded in what is observable (i.e. are interpretations about human interaction (the interactional linguistics position) more grounded in observables than inferences about human interaction (the ergonomist position)?

These last two examples from the MOSAIC project show the crucial roles that differing epistemologies played in bringing about productive multivocality. Here, underlying epistemological assumptions determined whether or not methods could be integrated and when they could not, the comparison of such assumptions served to determine how researchers grounded their analyses and interpretations.

Summary of comparisons that do lead to epistemological encounters

In the first example (Origami Fractions section, Part 2), modifications of major analytical concepts of two theoretical frameworks were carried out and functioned locally, but an

opportunity was missed to discuss the ramifications of these modifications with representatives of the relevant research communities, thus potentially leading to a confusing use of terms. If researchers are unilaterally redefining analytical constructs that are in widespread use, then how can other researchers evaluate work done with those constructs, if they don't mean the same thing anymore? On the other hand, if the innovative changes in definition respect the epistemological constraints of the framework in which they are supposed to function, then perhaps being free of having to conform to community norms is what will allow for scientific progress.

In the second example (also Origami Fractions section, Part 2), an epistemological modification led to mixed methods being incorporated in a coherent way. There is a well-known danger to mixing different methods, but Chiu did not fall victim to it. The *incompatibility thesis* critiques naive methodological eclecticism (in other words, mixing methods without examining their underlying assumptions) with the claim that methods from diverse traditions are based on incompatible philosophical assumptions, and so cannot be combined without incoherence (Yanchar & Williams, 2006). However, identifying the specific ways in which methods intrinsically carry assumptions while also defending the agency of researchers in choosing and combining methods allows researchers to escape methodological determinism and apply methods coherently in different theoretical frameworks than in which they originated.

The third example (Designing Biology, Part 5) shows that it can be fruitful to open up to engagement with the differences in underlying assumptions between our own theories and methods and those of others. Researchers may come to a consensus about how they might strategically relax or reformulate some of their methodological or theoretical assumptions in order to find some common ground on which to exchange insights. Alternatively, they may not come to such a compromise, but at least their perspectives will have been broadened by honestly asking the relevant questions. It should also be noted that even though data providers did not sufficiently communicate their context to analysts, thus leading to misunderstandings about the data, this ultimately led to insights about how to improve the pedogogical situation through Design Based Research.

In the fourth example (Peer Teams Chemistry, Part 3), unvoiced assumptions about the analytic construct of leadership were discovered through differences in conclusions that led to line by line comparisons of coding categories by the analysts and eventually to distinctions in definitions. Alternative operationalizations are beneficial, but also make for a harmonious researcher interaction when they can bring out complementary aspects of an analytical construct without taking away from another operationalization.

The fifth and sixth examples (Part 4: Multimodal Electricity) show how the units of agency and activity that researchers used in their analyses actually *embody* their underlying epistemological assumptions about where learning takes place and how to track it. In addition, this interrogation led to an analysis of how researchers use different criteria to evaluate learning. There is a tension between describing in detail the ways in which groups work together and how individuals contribute to the collective, and doing that in addition to evaluating the individual and group activity on the basis of whether or not it reflects an understanding of taught (or canonical) domain knowledge.

In the last two examples from the MOSAIC project, researchers maintained their original assumptions, coherent with their own frameworks, but they specifically sought out the comparison of epistemological assumptions with researchers of different traditions to see how the assumptions affected methods in order to gain insight about collaborative processes. We showed that when epistemological frameworks were compatible, it was easy to combine

methods but also that when frameworks were not compatible, attempting to combine methods only served to radicalize the researchers involved. At the same time, explicating their epistemological positions also had the effect of those radicalized researchers more clearly understanding how they were expected to respect the constraints of their respective frameworks and so made them more careful researchers.

Conclusions

In this chapter we have explored what occurs when researchers encounter each other's epistemologies in multivocal settings. We have argued that one of the major reasons that researchers from different traditions may find it difficult to collaborate is that they do not share the same epistemological assumptions about the nature of scientific knowledge and how it can be acquired. We further argued that researchers in the same community, but from different traditions may work separately on similar objects of study without benefitting from each other's efforts and that this is not effective at the community level. We reflected on how multivocal contexts could remedy that by studying the extent to which researchers who compared aspects of their analyses of shared corpora had encounters with other researcher's epistemologies. Our hypothesis was that engaging with researchers with different epistemologies could help bridge between traditions and make for more effective collaboration at the community level. Sometimes the comparisons researchers made about aspects of their analyses led to epistemological engagement and sometimes it did not.

In the cases where comparisons of aspects of analyses did not lead to epistemological engagement, multivocality could still be productive. For example, when the underlying epistemologies of researchers were already coherent (so there was no pressing impetus to engage), they were able to reinterpret each other's analyses in their own frameworks and focus on integrating each other's tools. On the other hand, when the underlying epistemologies of researchers were not coherent and they did *not* engage, this proved problematic for obtaining the kind of data researchers needed to do their analyses.

In the cases where comparisons of aspects of analyses did lead to epistemological engagement, multivocality was either 1) productive and glitch free, 2) productive, but difficult or 3) led to missed opportunities. For example, during productive glitch free epistemological engagement, researchers successfully mixed qualitative and quantitative methods, used multiple operationalizations of an analytical construct in a harmonious way in order to bring out the complementary aspects and combined methods of analysis from different traditions once it was ascertained that epistemologies were compatible.

During productive but difficult epistemological engagement, analysts experienced misunderstandings about data due to insufficient communication about its context by the data provider, and although in this latter's view, analysts were not treating the issue at hand (analysts even questioned the legitimacy of the pedagogical situation), the data provider ended up gaining insights from their analyses. In two other examples of productive but difficult epistemological engagement, the discussant was doing the engaging on behalf of the researchers who had expended their energy on attempting to get the data they needed and then attempting to align their analyses without an obvious common empirical basis. Despite the difficulties, the discussant was able to draw out differences in learner agency, learner activity and evaluating learning, but unfortunately not all researchers engaged with the discussant, so this example also could be classed in part in the category of missed opportunities.

During epistemological engagement that led to missed opportunities, a researcher — influenced by a colleague's analysis — modified the meanings of analytical constructs that were in widespread use; on the one hand he could be construed as innovating, but on the

other, he missed an opportunity to debate those changes within the larger community. Our final example also has both positive and negative aspects. Researchers from different traditions were not able to integrate their methods due to incommensurable differences in epistemologies. They became more entrenched in their respective stances and perhaps lost faith in the multivocal process. However, the act of engaging made them more reflective.

We are now in a position to offer up some concluding remarks. First, comparing aspects of analyses can be productive without leading to engaging with other epistemologies. That being said, many more examples were productive once researchers did engage. At the same time, some situations of productive epistemological engagement also led to difficulties or to missed opportunities. In the end, it all comes down to the researcher's will to participate in multivocality. When there were difficulties, they were due to communication breakdown (see Rosé, & Lund, Chapter 32, this volume), to a non-willingness to put in the necessary effort, but also to epistemological entrenchment or in other words a non-willingness to strategically relax or reformulate some of their methodological or theoretical assumptions in order to find some common ground on which to exchange insights. Researchers who are willing to engage in multivocality are innovators. They are on the cusp of interdisciplinarity, beginning to mutually integrate concepts, theories, methods and epistemological principles (Van den Besselaar & Heimeriks, 2001). Our hypothesis was that engaging with researchers with different epistemologies could help bridge between traditions and make for more effective collaboration at the community level. This chapter illustrates that we have taken some steps towards that goal for disciplinary traditions interested in collaborative learning.

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