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Modeling the Individual Within the Group: an Interdisciplinary Approach to Collaborative Knowledge Construction

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Kristine Susanna Lund

Modeling the Individual Within the Group: an Interdisciplinary Approach to Collaborative Knowledge Construction

SYNTHESE

Pour l'Habilitation à Diriger des Recherches

Sciences de l'Education

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JURY

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Executive Summary

Constructing knowledge with others is fundamental for all human activity, and many disciplines have sought to understand how the individual, other people, and the context, all influence collaborative knowledge construction, be it individual or group knowledge. The goal of this *Habilitation à Diriger des Recherches* is to present an analytical model of the relations of the individual to the group in situations of collaborative knowledge construction. The model is inspired by the work of Levinson (2005) who proposes variables that mediate between kinds of systems (e.g. linguistic, interactional, social) that are interlocked in various ways. After describing my personal trajectory and acknowledging those who have been influential in this work, I review the literature with five goals in mind.

First, I examine the interests and dangers of integrating work across disciplinary boundaries and I make a case in favor of interdisciplinarity. Second, I perform a cross disciplinary analysis of the individual versus the group in learning contexts. Here, I select studies that represent the four major paradigms used to study learning: behaviorist, cognitivist, sociocognitivist, and sociocultural and I analyze how those paradigms shape the assumptions researchers make about their object of study. I also illustrate the tensions that exist regarding how different approaches conceptualize the place of the individual in relation to the group, on what timescale and whether these tensions are fruitful or on the contrary, contribute to hindering scientific progress. Third, I introduce the notion of methodological determinism and argue against it, showing that theoretical assumptions are not embedded in methods and that researchers have agency in choosing those they wish to adhere to, thus avoiding possible incoherence when combining research methods from different fields. Fourth, I review how different research fields attribute explanatory value and I examine three ways to bridge across levels of analysis that relate the individual to the group. In the analytical model I propose, I opt for both bridging by explaining different aspects of a phenomenon and bridging through intermediate variables. Fifth, I compare methods of investigation for connecting levels of analysis through a discussion of causality and generalization that has implications for interdisciplinary work.

These five goals give the background needed to perform the analysis of a selection of my own interdisciplinary collaborative work that give rise to my analytical model, called “Multi-grain” knowledge building where “Multi-grain” stands for a MULTi-theoretical and Interdisciplinary model of the GRoup And Individual. The collaborative work I chose to analyze was carried out within educational sciences and language sciences, and more specifically in physics didactics, educational psychology, pedagogical debate, argumentation, interactional linguistics, and psycholinguistics.

Multi-grain knowledge building allows researchers to connect cognitive, linguistic, interactional, and social systems through the use of intermediate variables that are composed of facets of human interaction. Mutually influential facets allow us to view complex behavior as a system of interrelated systems (Levinson, 2005). Multi-grain knowledge building gives a framework that allows systems within different disciplines to “speak” to each other and defines the space in which explanatory models can be proposed about the different forms of knowledge co-construction. In each of the models issued from my collaborative work that served to develop the Multi-grain knowledge building model, the intermediate variable is one whose nature changes over time in a way that is comparable to conceptual change : 1) the semiotic bundle can illustrate conceptual change in physics for both individuals and groups, 2) the procedural explanation can illustrate changing competence (both cognitive and interactional-pragmatic) as children’s

language develops, 3) overall emotional framing of a debate can illustrate the group's argumentative complexity and degree of dialogism, as it is constructed over time while also illustrating individual competencies concerning emotional positioning of argumentative claim and 4) level of collaboration can illustrate the trajectory of a participant as she becomes a more active member of a community of practice.

Multi-grain knowledge building makes evident that some of the knowledge we co-construct in human interaction is a focus of explicit teaching while other knowledge is (more or less) naturally acquired as a function of the contexts in which we find ourselves. Learning physics is definitely a target of explicit teaching, but the pragmatic competencies involved in managing interaction while giving finalized procedural explanations has never been a teacher's preoccupation. Nor has any teacher written pedagogical tasks that help students to construct the emotional positioning of an argumentative claim. And achieving a level of collaborative relationship in a community of practice has never been the focus of training. The distinction between competencies that are taught and those that are naturally acquired raises questions about how societies select knowledge to be taught in a formal manner.

Perspectives for future work include understanding more deeply the multidirectional causality between the different facets of interaction within my current collaborative work. Second, I will also use my work in scientometrics to increase the scope of the Multi-grain model of knowledge construction and to pinpoint new areas where Multi-grain knowledge building can help in decompartmentalizing the research in education, opening it up to other disciplinary perspectives.

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1 Presentation of the HDR

“Cognitive appraisal should consider that much scientific knowledge is collaborative, so we should evaluate particular cognitive strategies in part on the basis of how well they promote collaboration. Conversely, social appraisal should take into account the cognitive capacities and limitations of the individuals whose interaction produces knowledge. Determining how to facilitate the growth of scientific knowledge, like the more descriptive task of explaining this development, depends on appreciating the complex interdependencies of mind and society.” (Thagard, 1994, p. 644).

The goal of this HDR is to propose an embryonic multi-theoretical, multi-level model, built analytically on mixed methods, and concerning how participants co-construct knowledge during group interactions. The collaborative work on which I base the model was carried out within educational sciences and language sciences, and more specifically in physics didactics, educational psychology, pedagogical debate, argumentation, interactional linguistics, and psycholinguistics. I call it a MULTI-theoretical and interdisciplinary model of GRoup And INdividual knowledge building. In other words: “Multi-grain” knowledge building). The multi-grain aspect is meant to reflect the idea that knowledge building occurs through the use of different facets of human interaction and that different facets are made salient by particular disciplinary foci and approaches. My hope is to unmask false scientific oppositions between different paradigmatic approaches (Lahire, 2012; Lund, Rosé, Suthers, & Baker, 2013). In addition, to translate Lahire (2012) and apply his words to the context of this HDR, I would “reknit the invisible links” between the different ways to study knowledge co-construction. In some cases, such a descriptive model *will* be designed to predict the co-construction of knowledge. In other cases, I propose it as a “thinking-tool”, used by researchers to challenge conventional disciplinary wisdom and search for more complete descriptions of multi-faceted phenomena, the first step being to link together what currently exists in a broader framework. As, the co-construction of knowledge is viewed by different disciplines at different levels of granularity (e.g. neuronal, cognitive, social), this HDR will work towards describing particular levels in a way that “speaks” to other levels (Sperber, 1997). The idea is not to throw out “carefully nourished babies” of different disciplines on the basis that they do not subscribe to particular assumptions (Levinson, 2005), but rather to explore the extent to which those assumptions can co-exist. Finally, a research program will be laid out.

In section §2 *Personal trajectory*, I present a brief history of how my professional and academic experiences led me to write the present manuscript.

Section §3 acknowledges more specifically the people who have been influential for this HDR, both in terms of developing the content and in terms of supporting the process.

The objective of section §4 *Integrating across disciplinary boundaries: interests & dangers* is to examine the scientific and social reasons for both working towards interdisciplinarity and for staying within a mono-disciplinary context.

Section §5 *A cross-disciplinary analysis of the individual versus the group in learning contexts* reviews learning definitions within four overlapping paradigms (behaviorist, cognitivist, sociocognitivist, and sociocultural). This analysis reveals how theoretical assumptions about learning, causality, and reality have evolved, changing the way learning is studied. I position this historical analysis in relation to the tensions that exist regarding how different approaches

conceptualize the place of the individual in relation to the group and on what timescale. The objective is to set the stage for exploring the dynamics of learning in an interdisciplinary context and to examine where disciplines can join forces. Such a review of the literature sets the stage for how I put together an abstract model in section §9 *A MULTI-theoretical and interdisciplinary model of GRoup And Individual (“multi-grain”) knowledge building*.

Section §6 *Methodological determinism and resercher agency* explores the supposition that underlies the incommensurability thesis and the related practice, called naïve methodological eclecticism (Yanchar & Williams, 2006).¹ The idea behind this section is to illustrate that not all theoretical assumptions are anchored within methods and that researchers may choose the assumptions underlying the methods they use, assumptions that are compatible with their own objectives. Although research that mixes methods risks mixing incoherent assumptions, this is not a fatality. When a researcher chooses which theoretical assumptions underlie the method being used, then methodological eclecticism becomes a conscious process and the researcher does not risk performing incoherent work. This section will illustrate how the conscious process of examining assumptions underlying methods used to analyze the same corpus of learning within a group led to a number of insights, both on the research process and on the conceptualization of learning. I argue in this HDR that this sort of careful interdisciplinary reflection is particularly productive and I show how I worked to apply it in the collaborative projects I review.

The goal of section §7 *Explanations that compete across levels of analysis* is to first review two major types of scientific explanations that have been given for learning, and second, to consider the extent to which these two types of explanations can co-exist both from a philosophy of science perspective and in a way that is valuable for placing the individual in relation to the group. Third, given that I position myself in favor of pluralistic explanation, I present its principal danger, a danger that researchers particularly risk in interdisciplinary contexts. In reviewing the selection of my collaborative work, I discuss the scientific explanations types we strove for and how they could be complemented.

Section §8 *Methods of investigation for connecting levels of analysis* examines how the different ways of viewing causality and generalization function in different paradigms and have consequences for understanding the role of the individual within the group. This review argues for holding multiple views of causality in order to obtain access to a broader base of methodological tools. In writing this HDR, I became progressively more aware of the importance of this aspect and I argue for its relevance in my future research program.

Section §9 *A MULTI-theoretical and interdisciplinary model of GRoup And Individual (“multi-grain”) knowledge building* proposes an interdisciplinary abstract model of the individual within the group during collaborative knowledge construction. This model is based on my collaborative work in the education research fields of physics didactics, pedagogical debate, and educational psychology and in the language sciences subfields of psycholinguistics,

¹ This section was built upon the following article: Lund, K., & Suthers, D. D. (forthcoming). *Le Déterminisme Méthodologique et le Chercheur Agissant*. *Education & Didactique* 10(1). Some of the main ideas in this article were initially expressed in a presentation Dan Suthers gave in Lyon in 2012. Here in §6, I take a personal enunciative stance with the use of “I”, to illustrate that I also hold these views.

argumentation, and interaction analysis. The model is constructed by reviewing a succession of five focused models from different collaborative interdisciplinary projects. Each model is reviewed in terms of:

- the research questions giving rise to it;
- how learning was conceptualized;
- how we expressed researcher agency in doing the work;
- the interdisciplinary challenges we experienced;
- how our explanations crossed levels of analysis;
- how our methods of investigation provided for connecting levels of analysis.

Section §10 *Perspectives for future research* proposes a research program for increasing the scope of the multi-grain model of knowledge construction, taking its impetus from my recent collaborative work at the crossroads of scientometrics and education.

My notational and formatting choices have been made in the interest of facilitating the work of the reader. They are quite classical, with only a few variations. Throughout the manuscript, direct citations of a longer length are presented in boxed format. In §9 *Building a MULTi-theoretical and Interdisciplinary model of GRoup And INdividual (“Multi-grain”) knowledge building*, I present quotes from published papers either as is (in the case of the paper abstract) or that I comment on (in the case of quotes about learning). Both are presented in table format with the appropriate headings (e.g. Abstract of XYZ (year), quotes on learning, and commentary on learning approach).

2 Personal trajectory

“Cooperative work is a social art and has to be practiced with patience.” It needs “extensive experience of working and thinking together” (Brozek & Keys, 1944, p. 512).

The training we get and the research that we do stems from a mixture of our interests, our encounters with others, the institutional opportunities that we seize upon, and additionally, the constraints that we accept. I have always been curious about how we learn, and I was also interested in languages from a young age. My mother Kerttu Lund is Finnish and spoke to my younger brother Jon and I in her mother tongue for two or three years after we were born. As a child, I traveled twice to Finland and Finnish relatives visited us a number of times in Minnesota. Things Finnish were a large part of my childhood and early adulthood. At 12 years old, I became a “villager” at *Salolampi*, the Finnish Concordia College Language Village in northern Minnesota, where young people went to learn Finnish language and culture. I spent probably 15 summers there, first as a villager, then as a counselor-in-training, counselor, staff counselor, and finally assistant dean. There are currently 15 language immersion programs available. Had I not moved to France in 1991, I would still be involved in that program, helping young Americans to learn about other cultures, gain knowledge about the world, and hopefully, tolerance about other ways of life. This experience of discovering different perspectives on the world through a foreign language initiated my interest in languages and in learning, in general.

2.1 Degrees in Computer Science, Artificial Intelligence, and Cognitive Science

Thanks to government loans and my paternal aunt’s and her husband’s financial help (Marge and Layton Kinney), I did my undergraduate degree at a small, Swedish, Lutheran, liberal arts school in southern Minnesota — Gustavus Adolphus College — where one “learns how to learn”. In preparation for skills I thought I would need in any profession I would later choose, I majored in computer science. I also began to study Cognitive Science in extra-curricular discussions about the scientific literature with the psychology professor Mark Kruger. I was unable to obtain a minor in Cognitive Science because I spent my junior year abroad in Finland becoming fluent in Finnish and visiting relatives and did not have enough credits. That said, although I spent most of my time at a Folk High School² in Joutseno in Finnish Karelia, I also followed a class in Artificial Intelligence methods at the technological university in Helsinki (*Helsingin Korkea Koulu*). There I sought out the professor Joukko Seppänen whom I knew worked on modeling languages with artificial intelligence techniques. I needed to develop a project as part of my Computer Science degree and I wanted to combine my interests in Artificial Intelligence and languages. Under his guidance, I wrote (in Pascal) a morphological analyzer of the Finnish language that took Finnish words as input and gave their morphological analysis as output. There was nothing remotely artificially intelligent about it as the algorithm just consulted lists of root grammatical forms I had inputted, but it did require building knowledge of the Finnish language. When I went back to Gustavus for my senior year, my courses were almost exclusively in

² The character of folk high schools differ from country to country, but usually institutions have the following common features: a large variety of subjects, no entrance or final exams, a focus on self-development, and pedagogical freedom. Courses last between a few months and one year, with per-course fees. See https://en.wikipedia.org/wiki/Folk_high_school

Computer Science, as I needed to make up for the time spent abroad in order to obtain the major. I was not a talented programmer, and I didn't do very well having to concentrate on only one subject. If I managed to get the degree, I understood that it was in part because of the unusual and perhaps interdisciplinary nature of my Finnish project.

After graduating from Gustavus in 1987, I worked for about a year at the Minnesota Nuclear Weapons Freeze Campaign, canvassing in Minneapolis and St. Paul neighborhoods in order to lobby our politicians to vote for a comprehensive test ban on nuclear weapons. In 1988, I began work as a systems administrator and programmer for David Yuen, a geophysicist at the Minnesota Super Computer Institute, part of the University of Minnesota. He sent me to Sweden to the Hans Ramberg Plate Tectonics Institute, where I did systems administration work for three months. Around this time, one of the counselors at Salolampi, who was also a professor in Finnish language and literature at the University of Minnesota, suggested I do graduate work with him. But I was more interested in languages, per se, and in learning. So I applied to get into graduate school in linguistics at the University of Minnesota, but I was unsuccessful. They rejected my application because my score on the quantitative part of the entrance exam was a predictor for failure in their program. But as the University system permitted it, I then began to audit linguistics classes without being part of the program; my intention was to do the program anyway. I had time to get an "A" in my first class — phonetics, before meeting my future husband Yanick Ricard, and then moving to Paris in early 1991. In 1990, I had been sent to work in Yan's geophysics laboratory at the *Ecole Normale Supérieure* in Paris by my boss, David Yuen, and that was where we met.

I entered the CNRS as an *ingénieure d'étude* in the fall of 1991, as a systems administrator for the LPNHE (*Laboratoire de Physique Nucléaire et de Hautes Energies*)³, located at the University of Paris 6 on the *Jussieu* campus. Our first daughter Taina was born in 1992 and I also began my Masters studies in Artificial Intelligence that fall, on the same campus as where I worked. It was difficult being a young mother, working, and going to school, while not wholly mastering French and I had to redo the first year because I hadn't understood that I needed to take all the "common trunk" courses the first year to be able to do my Masters thesis the second year. Some of the classes were very difficult for me (e.g. Lambda Calculus) and when I took (or retook) all of them the second year, the average of all my grades was borderline, but I passed.

We decided to leave Paris in 1994 and move to Lyon and I found a laboratory (the COAST team within the IRPEACS laboratory, and housed at the *Ecole Normale Supérieure*) where I could continue to be a systems administrator as well as finish my Masters degree. I began working with Michael Baker who co-directed my Masters thesis, with Monique Baron, a professor at Paris 6, and I finished it in 1996, the same year our second daughter, Yasmine was born. My thesis topic enabled me to move more towards learning as I contributed to designing and developing a computer supported collaborative learning system in physics. I also began branching out within my engineering position, doing programming, and designing our team's first web pages, in addition to systems administration. In 1997, I began a Ph.D. Degree in the Cognitive Science program in Grenoble under the direction of Michael Baker and Andrée

³ In Lund & Quignard (forthcoming), I relate an anecdote involving one of my colleagues in particle physics. He was visiting, saw our cat and claimed to be convinced that only female cats could have three colors. When I told him that our three-colored cat was in fact a male, he changed his perception and declared that he now only saw two colors. Did he modify his data (and not only his data, but his perception!) to conform to his theoretical views?

Tiberghien, while continuing to work full-time. This was possible because I integrated my personal Ph.D. work into the laboratory's on-going research projects. As our team's focus of study was physics learning and teaching, this became my terrain and I began gathering data within a collaborative project funded by what was then the IUFM (*Institut Universitaire de Formation des Maîtres*). This allowed me to build an analytical model of how pre-service teachers explained students working together on physics labwork, the contribution of my Ph.D.

I began to do more work on other research projects as an engineer in the human and social sciences, in addition to my work as a computer scientist and I enjoyed the former more so I asked the CNRS if I could change my position. In 2000, I changed professions from a computer systems engineer to an engineer in human and social sciences, specializing in data analysis (still *ingénieure d'étude*). In that same year our team leader Andrée Tiberghien announced her retirement and so Michael Baker and I co-founded another research team within our laboratory, entitled Interaction & Cognition. In 2003, I defended my Ph.D. and in 2004 I became a research engineer (*ingénieure de recherche 2ème classe*) in the human and social sciences through an internal CNRS promotion process. The French academic system is a mixture of “clocking time” and merit and as soon as I had spent the required time in a particular position, I participated in the next internal promotion process, carried out on a national level. In this way, I became an *ingénieure de recherche 1ère classe* in January of 2007 and an *ingénieure de recherche hors classe* in January of 2013.

2.2 Pivotal research projects that shaped my interdisciplinary outlook

I had begun getting involved in large, European or French funded, collaborative research projects during my Masters degree and all of them were interdisciplinary (e.g. ICALTS, SCALE, VIRTUALIS, and LEAD for the European projects and ApprenTICE, EIAH, ASPIC, and COSMOCE to name some of the French projects). They were always in education, language sciences, psychology and computer science, but later included industrial design. In some of them I co-directed or am co-directing Ph.D. theses, in educational sciences, computer science, language sciences, and industrial engineering. These projects went generally very well and we met the research objectives that we collaboratively defined, even if we had to negotiate from our different disciplinary stances to do so.

2.2.1 Epistemological encounters of a difficult kind

One of them, however, stands out as being particularly difficult because we uncovered a serious epistemological conflict when cognitive ergonomists and interaction analysts attempted to collaboratively analyze a corpus of architects doing design. The project was called MOSAÏC (2001-2003) and it stood for *Méthodologie d'analyse pour la modélisation de situations coopératives en conception de produit* or Analytical methodology for the modeling of cooperative product design situations and it was funded by the French government under the programme *Cognitive — Cognition, Interactions Sociales*. 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. The following paragraphs describe my interpretation of what happened. I wrote it four years later in the context of a similar collaboration called the Productive

Multivocality Project in a chapter where we reflected on epistemological encounters during interdisciplinary collaborations (Lund, Rosé, Suthers, & Baker, 2013):

During one collaboration within the MOSAÏC 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 enunciative⁴ 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 to elaborate theories and models of the data set. They also agreed that 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 MOSAÏC 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

⁴ Ducrot defines an enunciator as the instance of *the source of a viewpoint* expressed in the propositional content of an utterance (Brandt, 2013).

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 [...] 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)? (Lund, Rosé, Suthers, & Baker, 2013, p. 671-72)

Experiencing first hand the epistemological arguments about data given by researchers in different disciplines in the MOSAIC project made me want to better understand the challenges posed by interdisciplinary collaboration. Others were also interested, having motivations stemming from their own contexts, and our discussions gave birth to the Productive Multivocality project, developed through a series of workshops at the International Conference on the Learning Sciences (ICLS) in 2008 and 2010, the Computer Supported Collaborative Learning (CSCL) conference in 2009, and the Kaleidoscope and STELLAR⁵ Alpine Rendez-Vous (ARV) in 2009 and 2011. An interim report was also presented at a CSCL 2011 symposium (Suthers, Lund, Rosé, Dyke, Law, et al., 2011). Our collaboration included over 30 researchers from 13 different countries and we sought to engage researchers from different analytic traditions (i.e., multiple "voices") in productive dialogue with each other while analyzing shared data from group interactions in collaborative learning settings. This work culminated in an edited book (Suthers, Lund, Rosé, Teplovs, & Law, 2013).

Our goals were to help researchers understand and collaborate with colleagues from other traditions, and help students who want to broaden their understanding of theoretical and methodological traditions available to them as well as understand how they might be brought into coordination. In addition we published analytic results on particular learning settings on five data corpora. This made up the body of the book and offers a vision of how fields of study (such as the Learning Sciences) that are comprised of diverse traditions can counter tendencies towards fragmentation and achieve some level of coherence (Suthers, 2013a). This collaboration was seminal for me. It was a highly stimulating experience, involving people who were open to

⁵ Kaleidoscope and Stellar were European Union funded Networks of Excellence that focused on Technology Enhanced Learning and involved researchers mainly from educational sciences, psychology and computer science.

examining their epistemological assumptions in order to see if their work could be fruitfully combined. I had found the way I wanted to do science.

2.2.2 Our methods only tell a partial story

Shortly after the MOSAIC project ended, I had another experience where approaches seemed incommensurable, but with hindsight, this would not have been obligatory. In the fall of 2004, my husband was invited to Yale University to work with a colleague in geophysics and I obtained a year's salary from the CNRS to work with Robert Sternberg as a visiting scholar at the PACE (Psychology of Aptitude, Competence, and Expertise) Center. In addition to being a great year for our family, this was an important year for me methodologically. Sternberg was running a very large Randomized Field Trial on a dynamic assessment technique using his triarchic theory of intelligence (Sternberg, 1985). The write-up, appearing as a document under the auspices of the National Research Center on the Gifted and Talented was titled *Dynamic Instruction for and Assessment of Developing Expertise in Four Ethnic Groups*. In total, 1,500 students and 71 classroom teachers, in 24 schools across 6 school districts participated in the study and they were put into 7 different experimental conditions (Sternberg, Grigorenko, Birney, Fredine, Jarvin, & Jeltova, 2007). The global hypothesis of the project was that triarchic instruction (framing questions in analytical, creative, or practical ways), dynamic instruction, and dynamic assessment⁶ will decrease or eliminate the differences typically obtained between ethnic groups on conventional static tests in 4th grade math in the United States.

Back in 2004, the experiments were taking place in many primary schools throughout New York City and in other cities on the east coast of the United States. All analyses were going to be carried out quantitatively in terms of pre and post-test results, related to experimental condition and I proposed to film a set of the pre and post- tests which were in fact individual dynamic assessment interactions. I wanted to understand what was really going on during those interactions, as there was a very strict protocol to adhere to and I was skeptical that assessors and learners were actually sticking to it, given my background in studying human interaction. Although I wondered about internal validity, I was more curious about qualitatively understanding the nature of dynamic assessment interactions as they actually occurred. However, I ended up wondering if some other factor did indeed either cause or perturb the outcome of the experiment other than the hypothesized factors (Trochim, 2006), Sternberg, et al. (2007) defined internal validity differently — as making sure that the dynamic assessment interactions measured the same construct (i.e. competence) across all four cognitive modalities (analytical, practical, creative, and memory – this last modality was added to the original triarchic theory that contained only the first three). They argued that their testing procedure yielded internally consistent results across all instructional units (geometry, measurement, and equivalent fractions), so they conclude that the same competencies were measured no matter what cognitive modality was used. Yet in my view, this does not wholly demonstrate internal validity because even if the measuring of competencies gives similar results across modalities, if something else is happening in the

⁶ *Dynamic* assessment or testing (as opposed to *static*) is the systematic elicitation and determination of intra-individual variability during the course of a test (Guthke, Beckmann, & Dohat, 1997). These techniques are designed to identify more validly the current status of a mental trait and /or its modifiability. The aim is the quantification of the learning potential of a child during the acquisition of new cognitive operations (Grigorenko, Sternberg, 1998) and also to figure out the difference between latent capacities and developed abilities as a proposed measure for true potential for growth.

dynamic assessment interaction other than what was supposed to happen, it may be that this is what is establishing the causal link between intervention and outcome. This is why the characteristics of the human interaction are important. In this project, the intervention is the dynamic instruction using triarchic theory, in addition to the pre-and post-tests and these latter are carried out as dynamic assessment interactions. The outcome is the difference in scores between the pre-and post- tests, so the way those interactions are managed is crucial.

During these dynamic assessment interactions, used as pre and post tests, teachers had to give a certain number of hints to help students answer a set of questions and they had to respect a particular order in which to give the hints. For example, for any given question (e.g. one question in the practical condition was “Using a string, how would you measure the distance around a can of soup?”), there were six hints to give in the following order: 1) ask the learner to reread the question, 2) help identify the question by asking “What do you need to do? What is the real task here?”, 3) paraphrase the question and ask “Are there any words here that are unfamiliar?”, 4) prompt for conditional knowledge by asking “How would you go about explaining the answer to this question? What approach may work here?”, and 5) prompt for procedural knowledge by asking “What would you need to do in order to come up with the actual distance around a can of soup? What will your steps be?”.

Children and teachers in pedagogical interactions being what they are, the interaction never went as planned in the twelve individual dynamic assessment interactions that I videotaped in the measurement unit (6 pre-test and 6 post-tests, falling between 23 minutes and 1h05 minutes in duration). I had them all transcribed and I documented the different ways that the assessor deviated from the script. First of all, it was normal that not all of the prompts be given since the assessor stopped prompting when the learner gave the right answer. But often, the prompts were not given in the right order and more seriously, as far as internal validity is concerned, spontaneous prompts that deviated from the script were given, such as leading questions, verifications, repetitions of learners’ words, and content-related prompts. As I was discovering this, I thought - in all honesty, how can a teacher *not* respond naturally to a child in a learning situation? These deviations were a potential internal validity threat for the particular 5-prompt method, as it was not respected, at least in the 12 interactions I recorded. This meant that it was not the 5-prompt based dynamic assessment interaction that was influencing the answers the learners gave to the questions, but rather the way in which that interaction actually played out.

Although 1,500 learners were involved and I only report a small sample of qualitative analyses here, many of the same assessors carried out the other dynamic assessment interactions, so these interactions may possibly be representative of the entire set. That said, one should also consider how the assessors’ management of their interactions with learners changed over time as the assessors gained experience and this goes for learners as well who experienced different modules of content over the semester (i.e. geometry, measurement, and equivalent fractions). Learners may have internalized the prompting process, as time went on, and indeed this is what Sternberg, et al. report (op. cit.). In my view, construct validity — the degree to which inferences can legitimately be made from the operationalizations in the study to the theoretical constructs on which the operationalizations are based (Trochim, 2006) — remained intact in this project, but only because all of the types of deviation I recorded were still pertinent to the dynamic assessment process, per se. But again, what happened in the many hundreds of other interactions that I did not record? How were the dynamic assessment interactions *actually* managed? Could things have happened that modified the ways the learners answered the questions?

However in regards to that, in terms of validity and reliability more generally, I also recorded the errors the assessors made in filling out the scoring card they had for each dynamic assessment interaction. Assessors either made a mistake in which questions they were scoring for, or they made a mistake in the number of prompts they had to use for the particular question. In the participating classes I recorded the error rates were respectively 2% and 9% for the two research assistant assessors regarding errors for which question they were scoring and 4%, 6%, and 26% for the three teacher assessors, respectively. The error rates for recording number of prompts used was respectively 0% and 9% for the two research assistant assessors and 7%, 15%, and 23% for the three teacher assessors, respectively. Since all the analyses in the project were taking place on the dynamic assessment interaction scores as recorded by the assessors, this posed a real potential problem, if indeed these error rates were representative of how all of the assessors over time filled out their score cards.

Unfortunately, I only got an EARLI (European Association for Research in Learning and Instruction) presentation out of this project (Lund, 2005), and before that I gave an in-house seminar at PACE. I wanted to do a mixed methods journal article with a colleague from the project in order to understand the extent to which the variation I saw in the individual dynamic testing was influential in the experimental conditions, but this didn't materialized for reasons I never understood.

I had never been involved in a randomized field trial before, and even if I had participated in small-scale psychology experiments, my research up until then concentrated on qualifying human interactions that had a goal, such as collaborative learning. I felt that limiting ones data to quantitative results of pre and post-tests without understanding the real context in which experimental testing was carried out — especially if it was the *process* we were interested in — seemed to give only a very partial view of the phenomenon. Based on my qualitative analyses, I suspected that assessors gave help differently to lower scoring and higher scoring students, especially when the assessors were teachers that knew the students. But I didn't get the opportunity to explore this hypothesis.

2.2.3 Using one method to reveal the possible shortcomings of another

In 2015, the LIDILEM (*Laboratoire de Linguistique et Didactique des Langues Etrangères et Maternelles*) organized the Language, Cognition, and Society conference in Grenoble, under the auspices of the *Association Française de Linguistique Cognitive (AFLICO)*. I submitted a paper with colleague Audrey-Mazur-Palandre to AFLICO 2015 that was entitled “When the experimenter becomes a participant” (Lund & Mazur-Palandre, 2015). We sought to illustrate how an experimenter participated with her subjects during an experiment she was carrying out. The reader may not find this surprising, given the Yale study just described, but in this experiment, the interaction was between two children, one a child-explainer and the other a child-learner. Yet, we described how the experimenter still participated in that child-child interaction through her dynamic, interactive, and organized practices with the children. In a sense, it was an exercise in how ethnomethodological approaches could inform the interpretation of experimental data and perhaps invalidate hypotheses through showing how the experimenter intervened in ways that had unintended consequences. At first glance, this seems like very basic psychology and it is indeed also a question of internal validity. How can we make sure that alternative explanations for results are excluded or that something unexpected has not perturbed the results that we see? Indeed, one of the most important rules of carrying out experiments is that when taking data, experimenters should not deviate from established experimental protocols. This

guarantees that procedures are applied in a consistent manner and that each that element in a given condition is “equivalent” to the other. If these constraints are respected, analyses can be carried out on a coherent dataset and chances are better for replication of results to succeed if ever the experiment is attempted again. This remains the case if researchers do their analyses on a process that is part of a task (e.g. an interaction) instead of on an outcome (e.g. a written post-test). That said, those of us who study human interaction know it is almost impossible to script it – at least without technology (Dillenbourg & Jermann, 2007). In the work presented at AFLICO, the child-child interaction was not scripted in any way, contrary to the project at Yale. The children were given a task and they could carry it out in the way they saw fit, using what is known in language sciences as “free dialogue” (Bavelas and Healing, 2013). So here, there is no internal validity threat concerning whether or not the participants deviate from a script. However, there may be an internal validity threat concerning the experimenter making the child interaction deviate from its natural course, given the constraints that the task imposes.

In general, when the process being evaluated does not include the experimenter as a focus for interest, it has not been common to evaluate the nature of the human interaction between the experimenter and the subjects as playing a significant role in the experimental outcomes. We did this, however, for Lund & Mazur-Palandre (2015) and it was part of the reason this study was pivotal for me regarding methods.

First, looking closely at the experimenter responded to questions about internal validity threats we had from colleagues in conversation analysis when we presented the original study, positioned in psycholinguistics. Exploring this made me understand how the methodological focus of a researcher in one discipline can help a researcher in another discipline to be more rigorous. And although this was a case where Conversation Analysis helped us to do better analyses in psycholinguistics, I think this is a larger truth and demonstrates one of the interests of interdisciplinarity. It was also something I had begun to do by studying the real-life interactions within the randomized field trial during the project at Yale, something that had not been originally planned at all. One of our video clips analyzed for the AFLICO paper clearly showed how the child-explainer and the child-learner included the experimenter in the participation framework (Goffman, 1974) and this called into question our results⁷. For example, one of our results was that children make different linguistic and gestural choices, depending on whether they are explaining a process or explaining instructions to carry out a task (Mazur-Palandre, Colletta, & Lund, 2014). So if it were the case that the experimenter either prompted the child-explainer or if the child-explainer looked to the experimenter for either approval or feedback and the experimenter gave it, then these interventions could change the linguistic and gestural content of the child’s explanations and therefore change our quantitative results involving that content and the comparison of it between explanations of process and explanations of instructions. It turned out that in the experimental phase we published about (Mazur-Palandre, Colletta, & Lund, 2014), the experimenter was not interventionist in a way that affected explanations or really very interventionist at all, but in a subsequent phase, not yet fully analyzed, this was more the case. Therefore, we need to be careful to check that her interventions do not influence the types of linguistic and gestural behavior we are trying to qualify in that phase. If they do, we need to eliminate the instance of that child-explainer/child-learner pair from the data of that phase. A

⁷ The criticism wielded at us was in fact broader and I address this in §9.4.6 *Challenges in studying the multimodality of human interaction within an experimental paradigm*.

critique could respond that it is obvious that the experimenter should not intervene if the interaction is supposed to be between the children and this would be true, but human interaction being what it is, especially with young children, experimenters will sometimes succumb to answering children's questions or intervene if they see a child struggling.

This brings us to the second reason why this AFLICO paper was pivotal for me, and it's more to do with taking further the aforementioned discussion of the interest of interdisciplinarity in examining methods. The idea that the role of the experimenter has an effect on subjects is not new. Indeed, McGarrigle & Donaldson (1974) showed how the experimenter crucially influenced children for Piaget's traditional conservation task. These authors described how children were taking into account the actions of the experimenter and not just the experiment, per se, when formulating their answers to the questions the experimenter asked them. In brief, if the experimenter was performing an action and then asking the children if something changed, they responded that something did change because why would the experimenter ask them if something had not changed? This taking into account *theory of mind* (Antaki, 2004) as an important element in interpreting experimental results was illustrated when the children responded correctly when a "naughty teddy bear" intervened, manipulated by the experimenter, and who, according to the experimenter, had the habit of "messing up toys" or "spoiling the game" (McGarrigle & Donaldson (1974). Bringing this alternative explanation of results to light gave a potential explanation for why it was difficult to replicate the age at which children were supposed to understand conservation of matter.

The lesson I have taken away from the examples in this section, be it MOSAÏC, the Productive Multivocality project the Yale project, the AFLICO paper, or the Piagetian conservation study, is that viewing methods and results that are derived from them with different perspectives is an effective way to eliminate alternative explanations for results and to thus be more sure of the causal link between condition/treatment and outcome.

2.3 Co-founding the company CogniK at disciplinary and institutional crossroads

In the fall of 2008, Vincent Tautzia and Stéphane Renaud contacted me in order to participate in a company they wanted to start that would change how parents and children watch educational television. They wanted to propose a television channel that eliminated advertising while personalizing learning according to each child's learning trajectory. My work in human computer interaction and its relation to learning had caught their attention and they asked me to participate in their project, chosen for funding by the Crealys start-up incubator in the Rhône-Alpes region. I was interested in the idea of adapting content to what learners knew and to what they liked and exploring founding a company in France was a new challenge, so I accepted. Our initial groundwork involved Assistant Professor Anna-Rita Galiano and the students in her developmental psychology class (*Développement cognitif de l'être humain*) at the *Université catholique* in Lyon. In March 2009, three of us founded the company Cognikizz (later changed to [CogniK](#)). I am still Chief Science Officer and am authorized by the CNRS and the French Deontology Commission to spend 20% of my time on the company in exchange for a budget for my research. Stéphane Reynaud was Chief Technology Officer and is now Chief Executive Officer with Jacques-Henri Michaud having taken over as Chief Technology Officer. Vincent Tautzia was initially CEO and now he works for Netflix. We currently employ over ten people, including a former Ph.D. student in Computer Science and post-doc of mine — Gregory Dyke — who is now Head of Research and Development.

The first interdisciplinary challenge in this context is working at the crossroads of computer science and developmental psychology. It requires understanding how characterizing — according to age level — the cognitive and social content of educational games and videos could be combined with recommendation algorithms so that children obtain content that is adapted to their learning needs, and that they like, but that is also in accordance with what their parents would like them to focus on. The second challenge involves coordinating the different constraints stemming from academia and industry within each in-house project. This is not unlike getting multi-expertise teams to work collaboratively and so the challenges are similar to bringing together members of different disciplines to work on a research project. It has proven difficult, however, to frame the content characterization process in a way that is publishable in academia, both in terms of maintaining proprietary information and in terms of proposing a scientific contribution. This is ongoing work, with submissions in progress and six Applied Cognitive Science, Cognitive Visualization, and Educational Data Mining master's theses that I have co-directed over the years (Besacier, 2010, Nowakowska, 2010, Girard, 2010, Salles, 2011, Becker & Simpson, 2012, & Skunkittyut, 2014).

Thanks to CogniK, I obtained a European and Regional FEDER grant that ran from 2010-2012, entitled PAMEALE — *Publication Adaptative Multi- Ecran d'Activités Ludo-éducatives* or Adaptive Publication of Multi-Screen Educational Games. I hired Audrey Mazur-Palandre as a post-doc researcher and this was the beginning of my interdisciplinary work in psycholinguistics. Mazur-Palandre had done a Ph.D. in child language development that had been wholly concentrated on verbal productions. I introduced her to the idea that gestures were part of language development and we studied the role that gestures in combination with verbal productions played when 6-year old children explained CogniK's on-line educational games to each other. This work has continued, in collaboration with both Mazur-Palandre and Jean-Marc Colletta, and it will be reviewed in §9 A MULTI-theoretical and Interdisciplinary model of GRoup And INdividual ("Multi-grain") knowledge building. The results of this research were not directly exploitable for CogniK — but could have been, had we chosen to develop an animated cognitive agent that accompanied children when they played games or viewed educational content. However, the results allowed me to discuss with our Chinese partner of that period when I presented our work at the post-conference event in Beijing, organized by the Computer Supported Collaborative Learning conference in Hong Kong (Lund, Tauzia, Reynaud, Edwards, & Mazur-Palandre, 2011).

A third challenge involving my participation in CogniK that I believe happens in collaborative and multidisciplinary contexts is that in view of my training, I was asked to produce work that was not immediately within my area of competence. As Chief Science Officer, I was asked to develop a scheme for coding music videos. We originally focused on the relations between music and cognition but my exploration of the scientific literature showed that the relations between music and emotion were more interesting. So I proposed a method for coding music videos based on the extent to which characteristics of the audio and video signals can be said to evoke particular emotions. And since November of 2015, this has become the MyMTV product, the only personalized music video channel that allows the user to choose music based on time period and musical genre, but also based on what type of emotion it may evoke. The first research question that comes to mind is one of validating the proposed algorithm: to what extent *does* the music evoke the emotion in our context, as defined by the audio and video characteristics, as developed in the scientific literature? But there are many other possible research questions, for example, can individual profiles of musical preferences be combined to

propose music that is emotionally satisfying for collaborative listening? There are two points that are interesting about this example for an HDR that focuses on interdisciplinary model building. First, multi-expertise teams may lead particular experts to move outside of their comfort zone, if he or she is the expert that is the closest to the desired expertise. I was not an expert in the relations between emotion and music, but I am a trained scientist and can read the scientific literature, so I developed some initial competence in this area. Second, this is not the case where a researcher takes ideas from her established research program and contributes to inventing a marketable product, such as for the personalization of educational content. Rather, the market forces and client contacts are such that a product is designed on the basis of recently acquired scientific knowledge. The research questions are only developed in a second phase. Such an analysis illustrates a basic truth involving the feedback loops that exist between ideas and applications: sometimes ideas give rise to applications and sometimes applications give rise to ideas. A group of us from around the world (Australia, France, USA, and Denmark) have been accepted to discuss our international experiences with building bridges between research and practice at the next International Conference of the Learning Sciences in June of 2016 in Singapore (Jacobson, Lund, Hoadley, Vatrappu, Kolodner, & Reimann, forthcoming). In 2015, I was invited to a [round table](#), organized by the CNRS to discuss how we could help to develop companies within the human and social sciences that dealt with similar issues.

In 2012, Viacom and CogniK launched My Nickelodeon Junior in France — the world’s first personalized educational television channel. It is now deployed through thousands of personalized channels in the UK, the US, Mexico, China, and Malaysia. Future research projects in this area that we plan to pursue include 1) comparing the pedagogical content that parents choose in each country in order to see if there are any cultural trends that can be explained and perhaps reacted to, 2) developing machine learning techniques that can accompany manual cognitive and social characterization of educational content, and 3) investigating the usage of our products in terms of implicit and explicit ratings (e.g. like/dislike), and their link with the recommendation algorithm and profile creation. These are all topics that are both useful for the company and potentially interesting for different research communities (e.g. human-computer interaction, cultural studies, artificial intelligence, and sociotechnical computational analytics).

2.4 Organizing interdisciplinarity through policy at the community level

As more and more institutions demonstrate, it’s also possible to strive for organizing interdisciplinarity through policy at the community level. I am participating in multiple initiatives of this sort, two local, one regional and many international, the latter within the Learning Sciences community. They all have their importance for this HDR.

2.4.1 Local initiatives

At the local level, I am on the piloting committee of the Laboratory of Excellence ASLAN. This multi-million euro 9-year program funds two interdisciplinary language sciences laboratories – ICAR (*Interactions, Corpus, Apprentissages, Représentations*) and DDL (*Dynamique du Langage*). The focus of ASLAN — Advanced Studies on Language Complexity — is to 1) theorize the complexity of language, 2) study the origin, evolution and diversity of language and languages through descriptive linguistics, the history and ecology of languages, and languages that are in danger of extinction, and 3) within an individual to group perspective, study language development, model the uses of language and interactions in context, and study language acquisition and learning contexts.

I am co-animator for the work package “Individual to Interaction” and evaluating the projects in this areas for a number of years contributed to my reflection of the relation between the individual and the group. The work package co-animators present the projects that ASLAN members propose for financing to the piloting committee so they can be evaluated. The criteria we use for acceptance are standard: quality and originality of work, detailed and reasonable budget, and feasibility of work. However, we encourage projects that combine researchers from DDL and ICAR and this involves creating a set of research questions that are interesting for different sub-disciplines of language sciences and that often involve other disciplines (e.g. psychology, education), since both of our research labs are multidisciplinary. In addition, we encourage projects that have a specific social impact that has been negotiated at the outset of the project with the relevant social actors. Two great examples are the ETUDYS project and its successor DYS’R’ABLE, both led by a former post-doc of mine Audrey Mazur-Palandre. In terms of social impact, their goals were to determine the number of students with dyslexia using a survey tool, determine the nature of their obstacles using diagnostic tests and interviews, and propose remedial tools and programs in order to raise awareness of dyslexia among university teachers and staff and in order to better integrate dyslexic students at the university level. The partnership included researchers and engineers from ASLAN, from the Lyon center for research in neurosciences, from the Lyon Institute of Human Sciences, hospital practitioners, and the University of Lyon’s student handicap service. Project members had competence in linguistics, psycholinguistics, English phonetics, neuropsychology, and statistics. Their research questions centered on describing and evaluating from different disciplinary perspectives the difficulties encountered by dyslexic students in two areas: 1) learning English and 2) acquiring competence in coherent production of both oral and written discourse. A project of this nature illustrates on a very general level how combining different expertise — both from several disciplinary fields, as well as from different sectors of society — is necessary for making progress on fundamental questions of science while helping to resolve difficulties people may encounter in their lives. This is the type of research I find inspiring.

Another example of an interdisciplinary project within ASLAN — of which I am a co-organizer — is our plan to publish a special issue or book on re-conceptualizing complexity. There is currently no agreed upon definition for the notion of complexity and the difference between complex and complicated is not clear. A group of us is preparing a call for papers in which we will ask potential contributors to position their work in relation to one of the definitions of complexity, and to make a distinction between complicated and complex. In addition, given that most of the current research takes an etic approach to complexity, we are encouraging both emic approaches and approaches that integrate both etic and emic views of the phenomena studied. Contributors will also be asked to articulate analytical levels and perhaps orient their contribution to one of the concepts currently under discussion in the complexity sciences (e.g. emergence, self-organization, feedback, non-linearity, adaptation, non-determinism...). The fields that we solicit contributions from are human interaction, education, acquisition, and language description. An international workshop is planned for June 2016, in Lyon where I will present part of this HDR as a contribution towards integrating emic and etic approaches.

Still at the local level, but with national support from the CNRS, in addition to local support from *Ecole Normale Supérieure de Lyon*, a group of us has created a new interdisciplinary incubator space — *le Laboratoire de l’Education* (LLE). It officially exists since January of 2016

and has five founding institutional members, each with several supporting higher education umbrella organizations.⁸ Our 12-person piloting committee is made up of members of these founding institutions. The disciplines involved are language sciences, disciplinary and professional didactics, sociology, history, and political science. Our goal is to fund interdisciplinary work in education that involves collaboration between these disciplines, where at least one founding partner member is involved in the proposed project. We have named an international Scientific Advisory Committee and are in our first round of project evaluation. In addition, we have obtained funding from the interdisciplinary mission of the CNRS where all of the aforementioned disciplines work together to analyze inequalities that may occur in contexts of learning. Three themes will be studied: 1) inequalities related to gender (sociologists, political scientists, cognitive psychologists) 2) professional training and innovative digital didactics (sociologists, linguists, computer scientists specialized in multi-agents, 3) Digital resources for the analysis and the political framing of inequalities in school (historians, linguists, physicists, cognitive and computer scientists). Each of the researchers in these themes work together with the relevant social actors who contribute to organizing how our work can positively impact the experience of inequalities at school. It is in the last theme where my own collaborative work is situated, based on the CNRS funded EducMap project, for which I am principal investigator. The EducMap project proposes an interactive tool that allows exploration of research in education using the Scopus database. One of our goals is to uncover missed opportunities for collaborative research on education. I will present more about this idea in §10 *Perspectives for future research*.

2.4.2 A regional initiative

At the regional level, Jean-François Pinton — the leader of the Initiative of Excellence (IDEX) that unites the cities of Lyon and St. Etienne — is in competition with other French regions to be selected for funding by the government. We will know if we have been funded in 2017. Pinton mandated André Robert, Daniel Simon and me to begin organizing the academic college that came to be known as EduCoLa (Education, Cognition, & Language). Academic colleges are organized in different disciplinary groupings; they are part of the governing body of the IDEX and their roles are multiple. They should ensure that the research community in question shares information and coordinates strategies for hiring, answering grants, purchasing large technological infrastructure, and putting into place new educational programs. This is accomplished through a piloting committee of around 15 members that represents the research laboratories, the masters and bachelors programs, the doctoral schools, and any organized structures in fields of interest, such as federations. The recommendations coming from academic colleges are transmitted to the IDEX Scientific Advisory Board and to the COMUE (*Communautés d'universités et établissements*), where they reach the partner institutions (universities, *grandes écoles*, etc.). In January of 2016, André Robert, Daniel Simon and I organized the first EduCoLa meeting and over 200 researchers attended. I suggested inviting Anne-Nelly Perret-Clermont as one of the keynotes, since her work is at the crossroads of

⁸ L'Institut Français de l'Éducation (IFÉ), le laboratoire Interactions, Corpus, Apprentissage, Représentations (ICAR, UMR5191, CNRS / Université Lyon 2 / ENS de Lyon), le Laboratoire de Recherche Historique Rhône-Alpes (moderne et contemporaine) (LARHRA, UMR5190, CNRS / Universités Lumière Lyon 2, Jean Moulin-Lyon 3, Grenoble Alpes / ENS de Lyon), le Centre Max Weber (UMR5283, CNRS / Universités Lumière Lyon 2, Jean Monnet Saint-Etienne / ENS de Lyon) et le laboratoire Triangle : Action, discours, pensée politique et économique (UMR5206, CNRS / ENS de Lyon / Universités Lumière Lyon 2, Jean Monnet Saint-Etienne / IEP de Lyon).

education, cognition, and language, and her presentation on theoretical tools that allowed her to cross boundaries was much enjoyed. The participants, however, mostly represented research communities in education as the communities in language (ASLAN) and cognition (CORTEX) are already organized in Laboratories of Excellence. One of the major stakes of the near future is to write the next long term research programs to be financed, if Lyon and St. Etienne are chosen for an IDEX. Long terms research programs stand a better chance of being funded if they answer complex questions that in addition to addressing fundamental science, have societal impact. Such questions are more effectively addressed from an interdisciplinary perspective because different viewpoints allow for targeting specific parts of a multi-faceted problem. If researchers are able to take into consideration how each part of the multi-faceted complex problem they are addressing can mutually influence each other, then their research will be stronger. This requires coordinating between disciplines at the level of the initial definition of research questions.

2.4.3 International initiatives

At the international level, I have been elected to the Board of Directors of the International Society of the Learning Sciences (ISLS) beginning in 2010 for a seven-year mandate. I am part of the executive committee of the Network of Academic Programs in the Learning Sciences (NAPLeS) whose overall goal is to foster high quality Learning Sciences programs internationally through several mechanisms that support teaching and learning:

- Examples of syllabi used in existing Learning Sciences programs;
- Resources prepared by renowned learning scientists on specific topics in the Learning Sciences;
- Visiting scholarships for students to Learning Sciences programs other than their own;
- International supervision of doctoral research.

I also co-chair the Education Committee whose objective is to mentor the professional needs of the ISLS members and to support, through its activities, the integration of new members into the Society. Our committee strives to foster a community in which members are mentored to effectively collaborate within the interdisciplinary and highly international membership of the society. Specifically, the Education Committee is responsible for:

- Managing and promoting the Doctoral Consortia, the Early Career Workshop and now the Mid Career Workshop at International Conference of the Learning Science and Computer Supported Collaborative Learning meetings in collaboration with the conference committees for each individual conference;
- Managing and Promoting a selection of workshops to be held at each ICLS and CSCL meeting in collaboration with the conference committees for each individual conference;
- Making proposals about other educational activities ISLS should provide for its members;
- Making proposals about educational outreach activities;
- Setting the criteria and evaluating proposals members make about educational activities;
- Working with those who will be appointed or selected to be in charge of any of these activities to provide visionary guidance from ISLS;
- Actively contributing to the Living Handbook of the Learning Sciences, a practical guidebook that records our procedures in order to facilitate future implementation and more easily integrate members who take on new responsibilities.

In collaboration with the ICAR laboratory, with the newly created Laboratory of Education, and with the support of the Labex ASLAN and the CNRS, we have put in a bid to organize the 2018 International Conference of the Learning Sciences with a suggested theme of “Reconsidering the Boundaries of the Learning Sciences”. As lead conference co-chair (that is, if our bid is accepted), I thought it would be fruitful to consider the origins of the Learning Sciences, the disciplinary boundaries as they have developed, and to look for ways to make research in education and learning stronger by reaching out to other similar communities:

As Tim Koschmann, a founder of the Learning Sciences tells the story, the International Society of the Learning Sciences was to be modeled after the Cognitive Science Society where psychologists, philosophers, computer scientists, linguists, anthropologists, etc. gather to talk across their disciplinary boundaries on topics related to cognition. Our founders wanted to accomplish the same thing, but focus on learning. Around a quarter century later — the first ICLS conference was held in 1992 — it’s time to reconsider those disciplinary boundaries. How have they evolved since that initial conference back in the early nineties? How we can fruitfully connect to research on learning and education in research communities that may have not actively participated in the Learning Sciences over the years, but whose results and insights can resonate with our own work? This is the objective of the Lyon bid (Lund & Niccolai, 2016).

There are many other research communities with which it would be important to connect. Some of these communities would fit perfectly well within the Learning Sciences, but have been going to other conferences and publishing in other journals. Holding the conference in Lyon with the theme we propose is a chance to bring them on board. Other communities don’t necessarily fit with the boundaries of the Learning Sciences, as they have evolved today, but we both could benefit from closer contact. Whether the communities in question fit today within the boundaries of the Learning Sciences or could fit into a reconsidered vision of the boundaries, we propose to break these research communities into four main groups. They include research on learning and education in the human and social sciences, in computer science, in the neurosciences, and finally in what the French call *didactique des disciplines* (in other words, teaching and learning that is specifically related to disciplinary content). In the bid, I argued for the interest of connecting more deeply to each.

These initiatives, be they local, regional, or international, all share the objective of implementing policies that support interdisciplinary work at the community level. I am involved in these initiatives because I believe that interdisciplinary research is more powerful than research carried out in a single discipline. This is not a view shared by everyone and part of the goal of this HDR is to explore the space of debate around this idea. What are the arguments in favor and against doing such research? To what extent are these arguments warranted? How have I dealt with the challenges I’ve faced in my own interdisciplinary collaborations?

In summary, although my educational trajectory has been multidisciplinary and my research project history varied, I have continued to nourish both my interest in learning and my interest in languages. The contexts I describe above also fostered a fascination with epistemology and its relation to methods. Founding a company furnished yet another context in which to build bridges as did my activity in organizing interdisciplinary through policy at different community levels.

My hope is that the model of “Multi-grain” knowledge building that I propose will help to understand how individuals and groups build knowledge together, a cornerstone of all human activity.

3 Acknowledgements

“Ideas have a lifetime. Like the people who mind them, ideas come to be and pass away, suffer the vicissitudes of fate and the determination of will, and remain forever creatures of context” (Wilder Mott, 1981, p. 5).

Writing this section is daunting as there are so many people to acknowledge. My thoughts go first to my immediate family, my husband Yan, and our two daughters Taina and Yasmine, the people most important to me, and with whom I love living adventures 🏠👍👩🏫. You have supported me with love and kept me grounded (unless we are flying together)! Keeping with family, I am grateful to my mom Kerttu for teaching me the meaning of *sisu* with many examples in her life, to her partner Gene for his sense of responsibility and his easy-going nature, and to my brother Jon for his steadiness and good humor. Although I kept trying, I never did understand the explanations my dad gave me when I was still in high school about his work on a unified field theory of physics and I believe this is partly responsible for my own research on explanation and my attempts to understand other disciplines. I’m especially grateful to my parents-in-law Ninou and Roger for providing me with such a nurturing family environment when we go to visit.

Next, I would like to thank the members of my jury, whom I thought might be interested in discussing this work with me. I’m grateful to Keith Sawyer, François Pellegrino, and Jean-Pierre Chevrot for agreeing to write reports. Keith is the Morgan Distinguished Professor in Educational Innovations at the University of North Carolina in Chapel Hill. He is a renowned scholar on the creative process, having studied with Dr. Mihaly Csikszentmihalyi. I have known Keith for a number of years from the Learning Sciences community and we worked together in the Productive Multivocality project, but I only recently discovered his theoretical work in sociology, central to understanding the relations between the individual and the group and the emergence of phenomena in human interaction. That, combined with his multidisciplinary training (undergraduate degree in computer science and Ph.D. in psychology) and his highly cited research on collaboration and learning make him uniquely qualified to weigh in on this manuscript. Anecdotally, but pertinent for this HDR, the *Ecole Normale Supérieure de Lyon* published an article of his (*La conversation comme phénomène d’émergence collaborative*) on exploring creativity using Conversation Analysis in the study of dialogue improvisation in theatre.

François Pellegrino is Director of Research in the CNRS in language sciences in the Dynamique du Langage laboratory. He is the director of the ASLAN laboratory of excellence, and in this capacity he is encouraging us to develop contacts with other disciplines in order to broaden the perimeter of our scientific influence. His multidisciplinary training (a Masters Degree in electronic engineering and signal processing, a Ph.D. in computer science and an HDR in language sciences), his connections to cognitive and neuropsychology, and his work on speech communication within the sciences of complexity make him an ideal candidate for evaluating my multi-theoretical and multi-level model.

Jean-Pierre Chevrot is a professor in language sciences at the University of Grenoble-Alpes and is currently on a sabbatical, financed by INRIA (*L’Institut national de recherche en informatique et en automatique*) at the Institute of Complexity in Lyon. In what has become an anecdote that I’ve told more than once, I joked on Facebook about the fact that when submitting a paper to the AFLICO conference, one had to choose a theme and two of the options were sociolinguistic cognition and cognitive sociolinguistics. I joked because to the outsider, such a distinction seems humorous, but to the serious interdisciplinary scholar — such as Jean-Pierre —

this distinction is crucial. As Jean-Pierre was on the organizing committee for AFLICO, he made sure this distinction existed. He puts it this way (Chevrot, 2016): the former takes a cognitive approach to the social whereas the latter takes a social approach to the cognitive. The researcher doing work in sociolinguistic cognition studies the cognitive processes that underlie the reception and the production of sociolinguistic information and the methods used are often experimental. The researcher doing work in cognitive sociolinguistics studies the link between language and communities where grammar is 1) a conventionalization of usage in addition to 2) a conceptualization of the world. The methods used are often based on surveys and studies of corpora of naturally occurring interactions where participants are culturally and socially situated within heterogeneous communities.

When initially submitting with Mazur-Palandre the paper I presented at AFLICO 2015, despite the joking, we had to stop and consider: were we doing sociolinguistic cognition or cognitive sociolinguistics? First, we were studying children's language and gesture production, but hypothesizing that their characteristics changed according to type of "how" explanation whose cognitive content was crucial: giving instructions or describing a procedure. We were more focused on language than on cognition, but our underlying framework (Mazur-Palandre & Lund, 2012) was Kita and Özyürek's (2003) model of multimodal production, and this was an abstract model focused on how language and gesture are co-produced from a standpoint of cognitive processes in the brain, while taking into account the body as being the object of processes of motor control. Second, we used an experimental protocol, but the children used "free dialogue" in order to accomplish their task, so their interaction was principally constrained just by the task they were to carry out. These reflections moved us toward deciding we were doing sociolinguistic cognition. Even though some argue against being pre-occupied with labels involving disciplines and that what counts is establishing what kind of analytical object we are building (D. Vernant, personal communication), such labels can perhaps help others to quickly understand the assumptions underlying our work, our theoretical background, and our methods. If such labels are correctly understood, they can also set the stage for potentially fruitful comparisons.

I am also very grateful to the other three members of my jury: Anne-Nelly Perret-Clermont, Nicolas Balacheff, and Erica de Vries. Anne-Nelly is Honorary Professor at the *Institut de psychologie et éducation* and the *Faculté des lettres et sciences humaines à l'Université de Neuchâtel*. I mentioned above that her research is carried out at the crossroads of education, cognition, and language. Her themes include social interaction and cognitive development in children, and argumentation and learning. When she presented at our EduCoLa academic college meeting, she told of being a student of Piaget's and always wondering (and later studying) why he chose to do the research he did. She also gave motivations for her own presentation, which was a first look back on her influential career. This inspired me to reflect more on my own experiences, my motivations and on the encounters I have had with so many interesting people over the years and how all this has formed my path of research. The trajectory and acknowledgements sections are the result of this reflection.

Nicolas Balacheff is *directeur de recherche émérite* in the CNRS and a *didacticien de mathématiques* at the University of Grenoble Alpes. He was head of the Kaleidoscope Network of Excellence in Technology Enhanced Learning and on the piloting committee for the STELLAR Network of Excellence in the same theme. We have known each other for many years and worked on our first project together from 1999-2001, entitled *Foundational notions: Study of*

the implementation and the elaboration of foundational notions in teaching situations: the case of teaching science and math. I thought about asking him to be on my Cognitive Science Ph.D. jury, but I was warned that he was critical. I thought that sounded positive, but I ended up not asking him. He and I began discussing my HDR probably in 2010 and had regular meetings for at least three years, but I had too many projects that were in progress and so I wasn't yet ready to reflect upon them, as a group. I even involved Nicolas in one of those projects. This took time away from the HDR, but was important for applying my views on interdisciplinarity in a new context (Balacheff & Lund, 2013). Our paper — Multidisciplinarity vs. Multivocality, the case of “Learning Analytics — was nominated for the Best Paper Award at the 2013 Learning Analytics Conference. I'm happy to finally have Nicolas on a jury of mine and am looking forward to his (constructive) criticisms.

Erica de Vries is my *réfêrante*, otherwise known as my (fairy) godmother. In the French system for the HDR in human and social sciences, her role is to present and make my case to the appropriate authorities. Interestingly enough — especially for how different academic sectors organize their own institutional procedures — this role does not exist for an HDR in the exact sciences. Erica is a professor in educational sciences at the University of Grenoble Alpes, and began her studies in the Netherlands as a cognitive psychologist. The conceptual frames that inspire her work come from multiple disciplines: cognitive science, psychology, philosophy of language, analytical philosophy, and semiotics. Her research themes include external representations and visualization, and design-based learning situations. We have also known each other for many years and met when she became a Pierre and Marie Curie post-doc in our research team in the late 1990s. Our paper from that period — Computer-mediated epistemic dialogue: Explanation and argumentation as vehicles for understanding scientific notions (de Vries, Lund, & Baker, 2002) was one of my first experiences in using both quantitative and qualitative methods within a single paper.

I began discussing my HDR with Benoit Habert in 2012, probably around the time when he and two other colleagues (Jean-Philippe Maguet and Jean-Michel Salaün) became members of my research team Cogcinel (*Cognition, Collaboration, Interactions En Ligne*). Benoit is a professor at the *Ecole Normale Supérieure* in both linguistics and computer science. He obtained a Ph.D. in both of these domains and his HDR is in language sciences. His current work focuses on the relation between memory, history, and digital media, the collaboration annotation of documents, flipped classrooms, and MOOCs (Massive Open Online Courses). Benoit is an excellent reader in that he uncovers all ambiguities of both form and function. He read many different sections of this HDR, some of which I have erased entirely (fortunately?) from the current manuscript. Unfortunately, the final version — including many new sections — did not benefit from his scrutiny, but I am very grateful to Benoit for all the discussion we had previously. Papers I had co-authored got published and my ideas finally started coming together as I began a writing marathon in order to finish within the schedule I had given myself.

Next, I would like to thank all the colleagues with whom I wrote papers which I review here and from which I build a *MULTI-theoretical and interdisciplinary model of GRoup And Individual – “Multi-grain” knowledge building*: Karine Bécu-Robinault, Audrey Mazur-Palandre, Jean-Marc Colletta, Claire Polo, Christian Plantin, Gerry Niccolai, Julia Eberle, Frank Fischer, Karsten Stegmann, Alain Barat, Heisawn Jeong, Pablo Jensen and Sebastian Grauwin. It has been extremely stimulating working with you all.

There are many other colleagues with whom I have co-authored papers, either published or in progress, and although these papers didn't make it into the analysis of this HDR, it doesn't mean that they are less influential for me. Seeing how they fit into *Multi-grain knowledge building* may be an exercise for the future: Gregory Dyke, G  lle Molinari, Matthieu Quignard, Sandra Teston-Bonnard, Vicky Markaki, David Shaffer, Amanda Evenstone, Anda Fournel, Jean-Pascal Simon & Jean-Marc Colletta. I have also been greatly influenced by colleagues with whom I am co-directing, or have co-directed Masters or Ph.D. students or worked on topics related to these PhDs: Guy Prudhomme, C  dric Masclet, Jean-Fran  ois Boujut, Daniel Brissaud, Frank Pourroy (in industrial engineering), Jean-Jacques Girardot, Annie Corbel (in computer science), Jean-Marc Colletta, Jean-Pascal Simon (in language sciences), Erica de Vries (in educational sciences), Bernard l'Et  , Jean Ecalle, Annie Magnon (in cognitive psychology), Anna-Rita Galiano (in developmental psychology), Nicolas Nova (in cognitive science), St  phanie Metz, G  lle Molinari, Pierre Dillenbourg (in computer science). It is my goal to continue these interdisciplinary collaborations. They would not have been possible without funding, first from my employer, the CNRS (*Centre National de la Recherche Scientifique*), but also from many other national and international funding bodies, both private and public.

Colleagues in all the interdisciplinary projects I participated in are much too numerous to name, but I will cite those who participated in the MOSA  C, Productive Multivocality and Dynamic Assessment projects, those I singled out as being particularly influential for my trajectory. These projects were funded by the French government, by the Kaleidoscope and STELLAR European networks of excellence in Technology Enhanced Learning, and by the National Research Development Centers Program, as administered by the Institute of Education Sciences, United States Department of Education.

- MOSA  C: Fran  oise D  tienne, Fran  oise Darse, Willemien Visser (cognitive ergonomics), V  ronique Traverso, Luca Gr  co, Lorenza Mondada, Sylvie Bruxelles (Interaction and Conversation Analysis), Arnauld S  journ   (physics didactics), Michael Baker (social cognition and argumentation). Another lesson I learned from this project is that some researchers prefer working inside of their comfort zone with other researchers who share their theoretical and methodological assumptions. This type of approach allows for detailed exploration of a well-defined and bounded phenomenon and some argue that this is a more efficient way of making scientific progress. It can be thought of as a *deepening* of questioning and can be compared to a *broadening* of questioning where the work takes place in a team where the researchers do not share theoretical and methodological assumptions. In such a context, developing research questions and sharing data requires rendering those assumptions explicit and evaluating them in the context of the analytical goals. This is hard work and it is often necessary to re-negotiate the phenomenon to be studied and the research questions to be asked. Some argue that this type of research is more suited to the complex problems that society faces in that different disciplines focus on different aspects of a problem, but that they do so in a coordinated way that is mutually influential. Each researcher makes a choice between deepening and/or broadening (*then* deepening, I would add) and this HDR clearly illustrates which choice I made.
- Productive Multivocality: This project included over thirty researchers from thirteen countries so here I will only name my co-editors of our Springer book: Dan Suthers, Carolyn Ros  , Chris Teplovs, and Nancy Law, the three authors that analyzed the

corpus “origami fractions” for which I was the discussant: Hajime Shirouzu, Ming Ming Chiu, and Stefan Trausan Matu, and finally those who analyzed the “electricity” corpus: Karine Becu-Robinault, Heisawn Jeong, Chee-Kit Looi, Wenli Chen, Yanjie Song, Yun Wen, & Richard Medina, and finally Suthers, who played the role of discussant for this corpus. The people in this project took the time and made the effort to understand the frameworks of their colleagues. They were often frustrated — as Suthers attests in §9.5.2 *Challenges in sharing a corpus with researchers analyzing from different perspectives*. That said, the effort was very rewarding, both in terms of better understanding the phenomena we analyzed and in terms of collaboratively inventing a new way to do research that is distinct from mixed methods.

- Dynamic Instruction for and Assessment of Developing Expertise in Four Ethnic Groups: Robert J. Sternberg, Elena L. Grigorenko, Nancy Fredine, Linda Jarvin, and Ida Jeltova. Working at the Yale PACE Center was crucial for me methodologically. I now conceive of taking into account (or not) the nature of the human interaction when an interaction process is an intervention in an experimental protocol as the difference between a Galilean and an Aristotelian approach to science (see §5.1.2 *Significance versus meaning in psychology*).

Colleagues at my own research lab ICAR, in my own team Cogcinel and in the larger structure InSitu (co-directed by Véronique Traverso and I), have provided a lively and enriching environment. I’ve been extremely lucky to work with Audrey Mazur-Palandre, Matthieu Quignard, Benoit Habert, Magali Ollagnier-Beldame, Jean-Philippe Magué, and Jean-Michel Salaün (Cogcinel) as well as Véronique Traverso, Isabel Colon de Carvajal, Catherine Kerbrat-Orechhioni, Christian Plantin, Sandra Teston-Bonnard, Emilie Jouin-Chardon, and Heike Baldauf-Quilliatre (people from the Languages, Interactions, and Situations team inside of InSitu).

Colleagues that are part of the lab’s support structure are serious and competent people that also know how to have fun and have made working all that more enjoyable: Daniel Valero, Justine Lascar, and Zeynab Badreddine (*Cellule des Corpus Complexes*), Lucie Bujon (general secretary), Agnès Bailly, Dekra Ouadah (financial services) and our lab director Sandra Teston-Bonnard and vice director Gerry Niccolai.

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All of my colleagues past and present at CogniK remind me that academia is not the only world in which to live. They challenge me to build bridges between research and the private sector and to gain competence in domains that are not originally my areas of expertise: Stéphane Reyaud, Jacques-Henri, Gregory Dyke, Sebastian Leguet, Margaret Edwards, Pierre Col, Gilles Allegranti, Boris Perevalov, Nicolas Brère, Camille Mougine, Camille Bret, George Jeng, and Vincent Tauzia.

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- Catherine Kerbrat-Orechionni for inspiring me to a rigorous eclectic methodological approach and for her thoroughly engaging books on verbal interactions (Kerbrat-Orechionni, 1990; 1992). I am also grateful for a conversation she had with me about my thesis involving the analysis of gestures and gaze, my first attempt at such work;
- Jacques Cosnier (1988) for his historical analysis showing how a succession of ideologies transformed psychology as well as neighboring disciplines. His trajectory has also been inspiring, taking him from studying the social behavior of animals and humans, to the discovery of a pheromone, to influential research on the relationship between non-verbal and verbal communication;
- Christian Plantin for the wonderfully enriching experience of co-directing with Gerry Nicolai the thesis of Claire Polo. His extensive work in argumentation and emotion has been foundational and our discussions on epistemology have been crucial for my understanding. And.... the *Dictionnaire de l'argumentation* has been published!
- Michael Baker for his “maxims”, his friendship, and for allowing me to do the Ph.D. I wanted to do, given the contextual constraints, and for encouraging theoretical and methodological exploration;
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- Steve Fiore for introducing me to the research communities of INGRoup (Interdisciplinary Network for Group Research) and the Science of Team Science, and for very stimulating discussions on a wide range of topics;
- Tim Koschmann for his work in establishing conversation analysis as a methodological approach for the study of learning;
- Paul Kirschner for discussions and writing about “paradigm wars” (Kirschner & Lund, forthcoming), prompting me to reflect more about researchers working together at the community level;
- Wendy Leeds-Hurwitz for arguing for the importance of telling the history of a research field as a way to facilitate newcomers into a community. She edited a book called *The Social History of Language and Social Interaction: People, Places, Ideas* (Leeds-Hurwitz, 2010) where the focus is on the authors and the institutions in which they worked, covering the early history of this work in the 1960’s and 70’s. Cognate disciplines of linguistics, anthropology, sociology, and psychology are considered only in so far as they contribute to research in language and social interaction. Leeds-Hurwitz (2010, p. 4) quotes Fitch (2005) who writes “scientific facts are discovered, and come to exist, through the interaction processes of human beings within particular speech communities” (p. 462). I have tried to take this to heart in the writing of this manuscript.
- Chris Hoadley, Janet Kolodner, and Tim Koschmann for telling me the history of the Learning Sciences.

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4 Integrating across disciplinary boundaries: interests & dangers

*“Le pari que les sciences sociales cherchent à relever est de produire des descriptions de l’activité humaine qui mettent un peu d’ordre dans son apparente confusion, sans prétendre la réduire ou l’éliminer. Toute la question est donc de savoir comment le faire de façon rigoureuse, convaincante et fondée”*⁹ (Albert Ogien, 2011 p. 16).

It is not uncommon that communities of researchers work separately on similar objects of study without necessarily taking into account each other’s efforts. For example, researchers may focus on different aspects of the “same” object of study without attempting to integrate how other studied aspects of the object could be relevant. They may also only accept a particular type of explanation as valid and not be sensitive to other types of explanations at other levels of description. In order to illustrate such behavior, I begin with two anecdotes of how researchers study similar objects, but in different ways. In the first example, biologists in two communities use different explanatory schema to describe the same phenomenon and in the second, physicists and biologists suggest using different methods to test the same hypothesis. Although these examples come from the exact sciences and I will focus on research in the human and social sciences, questions concerning the extent to which communities share objects of study, methods and epistemological foundations are valid across all the sciences and so the issues raised when exploring them are valuable for all disciplines. In what follows, I will also examine the benefits and drawbacks for staying within disciplinary boundaries and for venturing outside of them, on both cognitive and social levels.

4.1 Two explanatory schema in biology with different levels of description

Morange (2005) argues that our historically anchored disciplinary training teaches us to accept a particular type of explanation as valid and not be sensitive to other types of explanations at other levels of description. For example, one way of explaining bird migration is to say that climatic or daylight changes trigger physiological modifications in the bird’s organism. Another way is to say that a bird 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. Even though these two explanations are at different levels of description, they are not incompatible and can be brought together to give a broader understanding of the migration phenomenon. However, this may never occur if the separate communities of biology do not engage with each other.

4.2 Two explanatory schema (biology and physics) with different units of analysis

Richards (1995) relates an anecdote about how the model that two researchers from different disciplines constructed about a phenomenon guided what aspect of a phenomenon of interest they focused on. He was at a faculty party where researchers discovered that one batch of homemade beer was less bubbly than another one. A biologist suggested that it was because there was less air in the bottle and decreased oxygen meant that the yeast would die sooner, thereby converting

⁹ “The challenge that the social sciences are looking to meet is to produce descriptions of human activity that put a bit of order into its apparent confusion, without claiming to reduce it or eliminate it. The whole question is how to do this in a rigorous, convincing, and evidence-based manner” (my translation, Albert Ogien, 2011 p. 16).

less sugar to alcohol and producing fewer bubbles. A physicist countered that it was instead crucial to calculate how much pressure was building up in the bottle and that the increased pressure was what was probably killing the yeast and that what should be examined was what the effect of more fluid and less air would be on the amount of pressure in the bottle.

As Richards tells it, the party quickly formed into two groups: one of biologists and one of physicists, each discussing the theory that made sense within their respective scientific traditions. Neither group talked to each other and it was clear they were not going to compare results. Neither group was posing more interesting or more relevant questions, but perhaps if they had conversed and worked together, they would have discovered ways of converging. It may be safe to say that both groups were operating in positivistic paradigms, with their associated theoretical assumptions of discovery of objective universal laws and indeed, both were trying to understand the bubblyness of the batch of beer (purpose of analysis). However, each had a different unit of analysis (e.g. relation of oxygen quantity to yeast life vs. relation of pressure to yeast life) and therefore different representations and analytic manipulations. Richards doesn't give the solution to the enigma, but both hypotheses can be tested by first keeping pressure constant and decreasing oxygen level and then keeping oxygen level constant and increasing pressure and in both cases, checking to see if the beer is equally less bubbly in both cases than a "control" batch of beer, from which the experimental values of oxygen and pressure varied.

Whereas in the previous example from biology, the explanations offered for the phenomenon under question occur at different levels of description, in this example concerning biology and physics, it is not the level of description per se, that changes, rather the units of analysis were different, even though the purpose of analysis was the same. In the former, both explanations are valid whereas in the latter, one or both explanations may be true but we won't know until they are both tested by experiment. This last example helps us to see how some disciplinary views on what constitutes pertinent data, unit of analysis, and explanation of phenomena may be more difficultly reconciled.

4.3 Reasons to stay within disciplinary boundaries

Other than working at a particular level of explanatory description and using different units of analysis in order to describe the same phenomenon, there are other more social reasons for which researchers tend to stay within their disciplinary boundaries. They include the sense of community they obtain from shared visions, tools and methods as well as good knowledge of the disciplinary literature. In addition, researchers present their work during conferences and in publications that tend to be organized in terms of academic disciplines and one of the reasons for this is shared knowledge of evaluation criteria for research. Finally, promotions are generally determined from within discipline-inspired well known criteria and students and young researchers are most often educated and socialized in a specific disciplinary context, thus giving them a solid foundation for conducting their research.

4.4 Drawbacks of staying within disciplinary boundaries

However, there can also be negative consequences for what Sternberg (2014) calls "academic tribalism". First, tribal affiliations limit what topics are acceptable for study. Second, a widely accepted point of view held by researchers in one community can pit them against researchers in another community, who hold a conflicting, yet also widely accepted point of view. Third, a distrust of outsiders can develop, thus hindering the discovery of shared objects of study or

potential complementarity of methods. Fourth, foundational disciplines force researchers to choose sides in debates that divide the disciplines or because positions and promotions are doled out according to foundational disciplines and this again limits views on ways to solve research problems. Finally, experienced researchers often transmit a tribal value system to their students, in addition to disciplinary knowledge. Sternberg points out that transmitted tribalism viewed in this way can reinforce strongly categorical ways of thinking and therefore prevent students from considering how to combine different approaches in a useful way.

So on the one hand, there are good reasons to belong to a community, but on the other hand, there are also negative consequences for becoming an insular community member and not remaining open to the ideas, data, and methods of other researchers. In the next two sections, I examine the flip side of this question: what are the benefits and drawbacks of moving outside of disciplinary boundaries? Note that it will not necessarily be the case that the benefits of moving outside of a discipline will be equal to the drawbacks of staying within one nor will it necessarily be the case that the drawbacks of moving outside of a discipline will be equal to the benefits of staying within a discipline. The anchoring of the disciplinary view (or not) provides a rhetorical positioning that orients the argument and allows for focusing on different elements.

4.5 Reasons for venturing outside of disciplinary boundaries

Klein (1990) notes a wide range of objectives that educators, researchers and practitioners have pursued through interdisciplinary work: answer complex questions, address broad issues, explore disciplinary and professional relations, solve problems that are beyond the scope of any one discipline, and achieve unity of knowledge, whether on a limited or grand scale.

Let us also return to the idea that communities of researchers may work separately on seemingly similar objects of study without taking into account each other's efforts in order to set up another reason for venturing into another discipline's territory — the one that is at the heart of this HDR. In fact, the key word here is “seemingly”. Once researchers gather data and make explicit their purpose of analysis and methods, it becomes clear that what is considered the same phenomenon is in fact very different. Although the phenomenon in question may seem to be the same at a general level (e.g. group interactions), it often is not the same phenomenon once the data has been gathered and the purpose of analysis is made explicit. For example, experimental psychologists will typically gather data on group interactions in controlled laboratory situations that are specifically designed to test a hypothesis concerning how a variable affects either group process or outcome whereas interactional linguists will more likely gather data on group interactions in naturally occurring situations with the goal of describing the ways that participants co-organize their actions. At first glance, it may not seem obvious what these psychologists and linguists would say to each other.

But are they missing opportunities for the advancement of scientific knowledge on group interactions by staying anchored in their respective communities¹⁰? How is it different to do research within one discipline vs. in a way that reaches across disciplines? Could the latter be more productive or at least provide new opportunities for innovative research questions? More specifically for this HDR, I argue that it is possible to integrate research carried out in different academic communities on the co-construction of knowledge during group interactions in order to

¹⁰ Section §10 *Perspectives for future research* proposes a method for pinpointing missed opportunities.

obtain a more global or more cohesive picture of this phenomenon. In order to see how and set the stage for answering the questions above, it will be necessary to briefly review the origins of disciplinarity and how researchers view the concepts of interdisciplinarity, multidisciplinary, and transdisciplinarity

4.6 Disciplinarity, interdisciplinarity, multidisciplinary, and transdisciplinarity

Klein (1990) has written the first definitive synthesis of interdisciplinarity, focusing on history, theory and practice. She presents the concept of disciplinarity as a product of the nineteenth century, influenced by the evolution of the modern natural sciences, the industrial revolution and technology advancements. Both the development of more sophisticated instrumentation and industry demand led to disciplinary specialization. Disciplines recruited students as the modern university evolved and this led to socially anchored academic communities.

On the other hand, Klein argues that interdisciplinarity is viewed by many as being much older than the nineteenth century, rooted in the ideas of Plato, Aristotle, Rabelais, Kant, Hegel and other “interdisciplinary thinkers”. The term itself did not emerge until the twentieth century, but the ideas of a unified science, general knowledge, synthesis and integration of knowledge are quite old (e.g. Plato advocated philosophy as a unified science and the philosopher as the synthesizer of knowledge). Against this background, the modern concept of interdisciplinarity took shape in four major ways (Klein, 1990, p. 22):

1. by attempts to retain and, in many cases, reestablish historical ideas of unity and synthesis;
2. by the emergence of organized programs in research and education;
3. by the broadening of traditional disciplines;
4. by the emergence of identifiable interdisciplinary movements.

By the mid twentieth century the term “interdisciplinarity” had been applied to both the idea of grand unity as well as to a more limited integration of existing disciplinary concepts and theories. Two main types of integration were defined: bridge building and restructuring. Bridge building preserves disciplinary identities and often has an applied orientation whereas restructuring involves changing parts of several disciplines, either to integrate new organizing concepts or merge methodologies or skills.

In more recent literature with a narrower focus, Van den Besselaar & Heimeriks (2001) define a disciplinary research field as “a group of researchers working on a specific set of research questions, using the same set of methods and a shared approach” (op. cit., p. 706). Interactions occurring between disciplines can take many forms, ranging from communicating and comparing ideas, exchanging data, methods and procedures to mutually integrating concepts, theories, methodologies and epistemological principles.

4.6.1 Level of integration distinguishes type of research

The level of integration of disciplinary approaches has survived as an indicator that distinguishes between the forms of so-called “non-disciplinary” research: multidisciplinary, interdisciplinarity and transdisciplinarity. In van den Besselaar and Heimerik’s view, neither theoretical perspectives nor actual results from different participating disciplines are integrated during multidisciplinary. Rather, “the subject under study is approached from different angles,

using different disciplinary perspectives (op. cit., p. 706)". Choi & Pak (2006) hold a similar view, arguing that multidisciplinary draws on knowledge from different disciplines, but each researcher group stays within its own boundaries.

On the other hand, interdisciplinary research integrates contributing disciplines by creating its own theoretical, conceptual and methodological identity or in other words, "analyzes, synthesizes and harmonizes links between disciplines into a coordinated and coherent whole (Choi & Pak, 2006, p. 351)". The view emphasizing the integration of disciplinary perspectives as a marker of interdisciplinarity is a popular one (e.g. Birnbaum, (1981); Cotterell (1979); Hanisch and Vollman (1983); Hausman (1979); Klein (1990; 1996); Kockelmans (1979); Epston, Payne and Pearson (1983); Hermeren, 1986). But this notion of the necessity of some kind of integration for research to be labeled as interdisciplinarity is contested. Are participants in interdisciplinary projects purposefully taking an integrative stance? Lattuca (2003) argues that integrating presupposes a compatible framework in which such integration can take place — in other words, regardless of the disciplines concerned, interdisciplinary inquiry would naturally take the form of the scientific method found in the natural and physical sciences. This implies that each discipline's way of thinking about concepts, constructs, methods and theories are necessarily compatible if they can be integrated on an a priori basis into an agreed-upon general method of scientific inquiry. However, the measurement within a method is affected by the vantage point from which the phenomena in question are measured (Longino, 2013) and so if the general method of scientific inquiry or the levels of analysis are *not* compatible, then integration will be difficult. In addition, as Lattuca (op. cit.) also argues, perhaps some interdisciplinary projects attempt to redefine knowledge such as some scholarship in women's studies, ethnic studies, cultural studies and literary studies (Klein, 1996). So, while such redefinition might include integration of disciplinary perspectives, it may also include dismantling disciplinary perspectives rather than integrating them.

According to Gibbons, Limoges, Nowotny, Schwartzman, Scott & Trow (1994), transdisciplinarity takes interdisciplinarity a step further. Whereas interdisciplinary approaches explicitly formulate uniform discipline-transcending vocabulary or propose common methodologies, a transdisciplinary approach takes a common theoretical understanding and succeeds in integrating it into both participating disciplinary epistemologies. If enough researchers join in this effort, one could begin to refer to a new transdisciplinary field with a homogenized theory or set of models (e.g. social psychology or psycholinguistics). In other work, transdisciplinarity takes on a broader meaning. A transdisciplinary orientation works to overcome the disconnection between knowledge production on the one hand, and the demand for knowledge to contribute to the solution of persistent, complex, societal problems on the other hand (Jäger, 2007). For Hall, Vogel, Stipelman, Stokols, Morgan & Gehlert (2012), a hallmark of transdisciplinary research is its focus on advancing progress towards practical solutions to social problems - for example, translating research findings into practice and policy applications.

4.6.2 Why attempt integration and under what conditions?

In general, there must be some instigator (e.g. a complex social problem to solve) that brings about non-disciplinary research as most research is carried out according to the conventional classification of disciplines. Funding opportunities is another such instigator and indeed the European Union has provided grants for a number of years under the heading of Information Societies and Technologies where both computer scientists and psychologists join together to study some aspect of group learning. Multidisciplinary workshops and conferences are a third

instigator, such as the Alpine Rendez-Vous, organized by the Networks of Excellence Kaleidoscope and Stellar, both of which focused on Technology Enhanced Learning and involved researchers mainly from educational sciences, psychology and computer science. In multidisciplinary groups such as these and in communities such as the Learning Sciences and Computer Supported Collaborative Learning (CSCL) where similar work on group interactions is carried out, there is a danger of epistemological, theoretical and methodological conflicts (Hoadley, 2005) such as those revolving around the schism between naturalistic enquiry in human behavior and experimental enquiry in human behavior. Indeed, as Hoadley argues, the core questions of how to reconcile contextualized practice and generalizable research were never really resolved.

Although CSCL (for example) can be defined as a “Community of Practice”, a term originally coined by Lave and Wenger (1991) where “groups of people share a concern, a set of problems or a passion about a topic and who deepen their knowledge and expertise in this area by interacting on an ongoing basis” (Wenger, McDermott, & Snyder, 2002, p. 4), the researchers that study group interactions are still trained in their original research traditions and remain bound to the corresponding practices, even if they do interact (Kienle & Wessner, 2006). Alternatively, some members of such communities simply co-exist within sub-communities, attending conferences together but only really interacting with researchers from their home tradition. It seems therefore fruitful to analyze a representative set of the multidisciplinary research on the knowledge co-construction of groups in order to tease out any potential conflicts and consider to what extent they can be resolved or if they are best left in productive tension (Suthers, Lund, Rosé, Teplovs, & Law, 2013).

4.7 Drawbacks of working across disciplines

Klein (1990) notes three major difficulties facing interdisciplinary scholarship: general uncertainty over definition, lack of professional identity and dispersion of discourse. First is interdisciplinarity just nostalgia for a lost wholeness or is it a new stage in the evolution of science? Is it a historical quest for unified knowledge or is its goal to develop the frontiers of knowledge? Second, some proponents of such scholarship are wary of organizing professional interdisciplinary movements because institutionalization may bring about insularity, and avoiding insularity was one of main reasons they were attracted to interdisciplinarity in the first place. Third, the discourse on interdisciplinarity is widely dispersed and so commonalities that could be shared are simply not available for those who could benefit¹¹.

One of the more practical difficulties of working across disciplines is the time it takes to perform the intellectual work necessary to consider their compatibility or their incommensurability (Latour, 2005). It may be more efficient to stay within the boundaries of a discipline where the frameworks are well defined and where the type of maneuvering is well understood. Kuhn (1970) called it doing “normal science” where details are slowly accumulated in accordance with an established broad theory and where there is no need to question or challenge the underlying assumptions of that theory. In this way, a researcher’s energy can be put

¹¹ That said, the goal of the EducMap project (cf §10 *Perspectives for future research*) is to help researchers share such discourse.

towards reaching specific disciplinary scientific objectives. It's difficult to use a framework in an effective way while you are questioning it.

Another drawback of working across disciplines is difficulty in framing research to be published so that it both fits the aims and scope of existing journals and so that it does not fall victim to be found lacking in one way from one disciplinary perspective and in another way from another disciplinary perspective.

4.8 Conclusions and implications for interdisciplinary work

In sum, there are scientific and social reasons for both working towards interdisciplinarity and for staying within mono-disciplinary contexts. Interdisciplinary work may arise naturally, given a particular research question — or set of questions — but it also requires a specific commitment, knowing the challenges involved.

Given that humans are social beings, in the next section, I examine how individual and group learning and the relation between the two have been studied, as paradigms have changed over the past decades. Theoretical assumptions about learning, causality, and reality have evolved, changing the way learning is studied. I propose what we can learn from this historical evolution and suggest where disciplines might join forces for future work, while keeping in mind the challenges of doing so, as described above.

5 A cross disciplinary analysis of the individual versus the group in learning contexts¹²

“Much of what matters about human intelligence is hidden not in the brain, nor in the technology, but in the complex and iterated interactions and collaborations between the two. ... The study of these interaction spaces is not easy, and depends both on new multidisciplinary alliances and new forms of modeling and analysis. The pay-off, however, could be spectacular: nothing less than a new kind of cognitive collaboration involving neuroscience, physiology, and social, cultural, and technological studies” (Clark, 2001, p. 154).

The disciplines that are interested in how groups construct knowledge together explore the tensions occurring in the relationship between the individual and the group in different ways. Much of this research focuses on the context of learning / knowledge construction and it has been carried out within four different major paradigms: behaviorist, cognitivist, sociocognitivist, and sociocultural. The goal of this paper is to analyze how those paradigms shape the assumptions researchers make about their object of study, and to illustrate where those tensions lie, whether they are fruitful or on the contrary, contribute to hindering scientific progress. This analysis helps to understand the difficulties in collaborating across these paradigms, but it also makes it possible to suggest where it would make sense for disciplines to join forces, and given this, to define orientations for further research. This analysis sets the stage for the review of my own collaborative work in §9 *Building a MULTi-theoretical and Interdisciplinary model of GRoup And INDividual (“Multi-grain”) knowledge building.*

This section is organized as follows. First, tensions between accounting for the individual in the group at the frontiers of sociology and language sciences, and within psychology, are framed in terms of a process ontology and in terms of the inseparability hypothesis. Then, a selection of definitions of learning are given for the four main paradigms used to study learning: behaviorist, cognitivist, sociocognitivist, and sociocultural. Particular research studies are described for each paradigm and situated first along a continuum of short-term to long-term studies. I chose to compare short-term and long-term studies because the nature of learning is different according to temporal scale and I wanted to illustrate the extent to which that influenced the methods researchers employ. Second, these same research studies are described, according to the way in which the researcher accounts for level of granularity of learner context (individual, individual within the group, or community). I chose these three levels of granularity because they also illustrate different visions of learning and similarly to timescale, influence the methods researchers employ. Both of these continua are expressed on a three-point scale. In the former, studies are either short-term, medium-term, or long-term. In the latter, studies either focus on the low end on the individual learner, on the high end on the large group/community as an entity that learns, or in the middle on the role of the individual within the group, where this relationship is qualified in a variety of ways.

¹² A version of this section was accepted as a poster to the eleventh annual INGRoup Conference to be held July 14 - 16, 2016 in Helsinki, Finland.

5.1 Sociology helps language sciences and psychology view the individual and group

The tensions I describe below exist because of what are seemingly competing underlying assumptions about how we can understand the place of the individual, interacting in this world. First, there is a tension between doing research to render explicit internalized norms and values that guide behavior and doing research to describe a procedure through which people co-construct their accountability to one another. Second, there is a tension between doing research to describe individual behavior in terms of characteristics of said individuals and doing research to illustrate how a phenomenon is embedded within a process comprised of an individual or individuals in a context. Either the characteristics of individuals (including their internalized norms and values) are deemed sufficiently explanatory for what we want to understand or acceptable explanations need to include how these individuals interact with other individuals within different contextualized processes. The debate around the two foundational theoretical assumptions of socioculturalism: a process ontology of the social world and the inseparability of individual and social levels of analysis (Sawyer, 2002) crystalize these tensions, but also give us a way to reconcile them.

5.1.1 Pre-established rules versus co-constructed experience in sociology and linguistics

Goodwin & Heritage (1990) give a history of how conversation analysis originated that illustrates how the creation of successive disciplinary boundaries can define the assumptions under which specific analytical objects or aspects thereof (and not others) are studied. Under Talcott Parsons at Harvard in the first half of the 20th century, in the disciplinary context of sociology, “mutual understanding and shared communicative meaning were treated as the unproblematic outcome of a preexisting common knowledge of language and cultural symbols” (Goodwin & Heritage *op. cit.*, p. 284) and it followed that the coordination of action was viewed as the product of complying with shared norms of conduct.

Within this vision, if two people had similar socially conditioned dispositions, this sufficed to establish social cooperation and so a conceptual approach to action was favored over detailed empirical analyses of actual social interaction. According to Goodwin & Heritage (*op. cit.*) this meant that the analysis of language and meaning was consigned to linguistics, and was not considered an object of study for sociology. However, they cite the following foundational texts - Bloomfield (1946), Chomsky (1965) and Saussure (1959) - as all contributing to divorcing language from meaning as well, the latter excluding from analysis the “interactive matrix that constitutes the natural home for language (Good & Heritage, *op. cit.* p 285). It followed that at this point in history, talk-in-interaction was deemed relevant for neither sociology nor linguistics.

A first addition to the force of this position was Chomsky’s argument that linguists should ignore actual talk, it being a degenerate version of ideal linguistic competence; linguists should work exclusively on idealized sentences, constructed by the analyst herself. The consequence of viewing empirical analyses of talk-in-interaction as irrelevant to both sociology and linguistics was that action was divorced from interaction and by the mid-60’s, no discipline studied the intersection between language, context, meaning and action (Goodwin & Heritage, *op. cit.*).

Harold Garfinkel (1967) was the first within sociology¹³ to attempt to establish connections between language, context, and meaning and he did so as a critique against the Parsonian treatment of action (Garfinkel, 1952). Parsons chaired his dissertation committee and in his dissertation, Garfinkel criticized the Parsonian framework as not paying sufficient attention to the nature and properties of ordinary experience. In contrast to the Parsonian view that social and communicative order is based on pre-established culture and that this is what determines the meaning of words and rules, Garfinkel sought to show the opposite — that all aspects of a cognitively shared world are sustained through multiple shared methods of reasoning. These methods, according to Garfinkel, are used by individuals to particularize their sense of language, of rules and norms, of common culture and of shared knowledge to local circumstances.

Garfinkel showed that mutual understandings are revisable and instead of context determining the action that enfolds within it, he was able to illustrate a mutually influencing relation: in the same way as a social setting determines the sense of a current action, that action will re-determine the sense of the current context, by sustaining, modifying, updating or transforming it. There is a similar mutual building-upon view in the domain of ergonomics where an artifact becomes an instrument in a first phase, through the demands required of it by the activity being carried out with it — this is instrumentation. In a second phase, personal and collective practices are formed in order to use artifact as an instrument in a knowledge-building activity (Béguin & Rabardel, 2000).

According to Goodwin & Heritage (op. cit.), the type of analysis that takes into consideration mutually influencing aspects of interaction was echoed in a second tradition — labeled “context analysis” by Kendon (1979; 1982); it was less abstract in character and expression than Garfinkel’s work and more concretely focused on the study of social interaction. Its goal was to provide theoretical and empirical analyses of how human interaction is produced and organized and was substantially elaborated in the work of Erving Goffmann (e.g. 1967; 1974).

Sacks and Schegloff were graduate students under Goffmann in the early 1960s and were also in contact with Garfinkel and it was in this context that:

“...the discipline of conversation analysis essentially emerged as a fusion of the interactive and phenomenological/ethnomethodological traditions. Within this fusion, interactional materials would be used to investigate the procedural bases of reasoning and action through which actors recognize, constitute and reproduce the social and phenomenal worlds they inhabit” (Goodwin & Heritage, op. cit, pp. 286-287).

In sum, studying talk-in-interaction from a sociology point of view was not considered worth doing before the birth of conversation analysis. A description of talk-in-interaction could not be proposed as explaining how coordinated action comes about because when people act (or so went the reasoning, then) they are just following norms they have internalized, so any explanatory schema should be based on shared norms. Research should thus involve making these shared norms explicit. In addition, since talk-in-interaction was viewed as a degenerate form of ideal

¹³ Within linguistics, Michael Halliday was the first to consider language as a social semiotic system with the development of Systemic Functional Linguistics (SFL). In SFL, language is a resource for building meaning across the ever changing contexts of human interaction (e.g. Halliday, 1977).

language by the dominant linguistic paradigm of the time, this further discredited studying talk as it occurred in everyday situations.

After conversation analysis was established and participated in rehabilitating talk-in-interaction, the interactional sequence became the “analytic innovation that opened the way for cumulative empirical advance” (Goodwin & Heritage, op. cit, p. 287). When Sacks and Schegloff (e.g. Schegloff, 1968) put forward the concept of *adjacency pair* as an elementary specification of the notion of sequence, the type of explanation that was valued and accepted changed its form. The preferred explanatory schema— at least for conversation analysis — was no longer a specification of an internalized rule that drives behavior — as was the case with the previous view in sociology, but rather a procedure through which participants constrain and hold one another accountable (Heritage, 1984) — a procedure that could be described through the organization of adjacency pairs.

Goodwin & Heritage portray Parsons’ style of research as being the catalyst for other sociologists to begin to do detailed empirical analyses of actual social interaction. Yet, Turner (1974) argues that the substantive visions of reality held by Parsons’ Action Theory and by Symbolic Interactionism (assimilated to Conversation Analysis for the purposes of my argument) are quite similar. He argues that the divergence between them represents more of a strategic difference over how research in sociology should be conducted than a disagreement over the nature of the social world. In other words, it is possible to hold a similar ontological vision of the social world, yet not agree on how to build a theory of and conduct research in this world.

STRATEGIES FOR BUILDING THEORY

In Turner’s view, although different strategies *can* have different ontological implications, they are not so great as critics of Parsonian action theory would contend. Let’s first look at strategies for building theory and then at strategies for conducting research. Both action theorists and interactionists have the objective of formulating concepts that capture the social world, but they differ in the way in which these concepts are constructed. Action theorists advocate a form of deductive reasoning to develop generic theoretical propositions whereas interactionists prefer to generate them inductively. Yet as Turner argues, deduction and induction are not as mutually exclusive as theoretical platitudes would indicate and some authors prefer the term “abduction” in order to emphasize the dual nature of theory building (Willer & Webster, 1970)¹⁴. Therefore, diverging theoretical strategies are not necessarily built upon differing metaphysical assumptions about the nature of the social world. When action theorists build a deductive theoretical edifice, they must also appeal to the facts of the empirical world. And conversely, when interactionists inductively derive their theoretical concepts, they appeal to existing systems of concepts. There is a give and take in both directions.

¹⁴ “Constructs and observables must be connected in the empirical testing and application of a theory (or in its development), but they are *different in type* — constructs are the result of thought; observables, of sensation” (Waller & Webster, 1970, p. 750).

STRATEGIES FOR CONDUCTING RESEARCH

Can a similar argument of complementarity between action theorists and interactionists be made for strategies for conducting research? Both communities focus on the complexity of social processes where a wide variety of actors will orient themselves to situations of varying degrees of stability while attempting to align their respective actions. Both communities have the objective of capturing joint action and institutionalization in terms of generic and abstract concepts. However, interactionists and action theorists have different ways of conceptualizing these activities and then studying them. They disagree on the level at which phenomena should be studied in order to yield the most understanding of events in the social world, as well as the best predictions of these events:

“For the interactionist, the most understanding comes with the study of the interpretative and definitional processes of individual actors, while for Parsons the best strategy is to focus on the emergent patterns of action emerging from such symbolic interaction.

From the interactionist’s perspective, causal theoretical statements cannot ignore the interpretative processes of actors defining and re-defining the situation, since so much of what goes on is accounted for by the fact that actors shift their meanings toward themselves as objects, as well as toward other objects in a particular situation. In light of this fact, it is evident that emergent patterns of joint action, and linkages between these emergent phenomena, are subject to change from the very processes of symbolic interaction from which they emerge” (Turner, 1974, p. 291).

On the other hand, although Parsons — as an action theorist — accepts that emergent phenomena arise out of and are constantly altered by individual actors’ voluntaristic processes, so in this aspect, he seems to agree with the interactionists. But, he nevertheless maintains that structural social reference points must and can still exist without denying the fluidity of social reality. And this is where the two positions differ.

In sum, once presumed metaphysical and ontological differences are reduced to questions of strategy — be they for theory building or for methodological approaches, it makes less sense to argue about assumptions and it becomes clear that both positions have much to offer social theory (Turner, 1974). Each strategy focuses on a different range of phenomena for the same world. Parsons looks toward emergent properties whereas the interactionist studies micro social phenomena¹⁵. Both are concerned with the complexity of social organization, but study it from different perspectives that are in fact complementary.

This short historical analysis of the evolution of sociology and linguistics beginning in the early 1900s and Turner’s comparison of action theory and interactionism allow us to see how the assumptions we hold about human behavior define what we consider to be explanatory. We would do well to consider more closely what we may consider to *not* be explanatory, based on

¹⁵ But see the discussion in section §7.4.3 *Bridging through the definition of properties and laws* where I suggest how emergent properties can be linked to micro social phenomena, thus implying that emergent properties are not in opposition to interactionist micro studies.

how we interpret the epistemological positions of other researchers. Perhaps a closer examination of our opinions on this will reveal possibilities for complementarity, such as in the case above¹⁶.

5.1.2 Significance versus meaning in psychology

In psychology, there is a similar tension between doing research to describe individual behavior in terms of characteristics of said individuals — sometimes termed the Aristotelian approach and doing research to illustrate how a phenomenon is embedded within a process comprised of an individual or individuals in a context — sometimes termed the Galilean approach (Lewin, 1931; Tateo, 2013). An emphasis on frequency and categorization in the Aristotelian approach led psychology to adopt statistical methods to define psychological constructs based on the definition of an average behavior and with a goal towards defining general laws, as in physics. But quantitative methodology in psychology plays a different role than it does in physics. General laws in physics must explain all phenomena whether they occur frequently or only once. In physics, individual cases have the same relevance as frequently observed phenomena (Lewis, 1931; Tateo, 2013). As Tateo (2013) puts it:

“Quantification in psychology is instead used to infer a statistical abstraction from the analysis of inter-individual differences and explain individual behavior in return. The implicit epistemological assumptions in this way of dealing with psychological phenomena are the homology of inter-individual and intra-individual variation and the essentialism of psychological dimensions that are “owned” by average individuals rather than “produced” by the relationship between single concrete persons with actual situations of the world (Tateo, 2013, p. 534)”.

But, what if one does not seek to explain the behavior of an abstractly defined “average child”, but rather the behavior of a certain child at a particular moment? Psychological dynamics in an Aristotelian world only allows the explanation of cases that occur frequently enough to provide a basis for abstracting from the situation, but it does not have the tools for analyzing a particular case, even if it occurs fairly frequently, but not enough to be statistically significant. In a Galilean world, psychological dynamics does not derive all its vectors (e.g. what makes something move or change) from single isolated objects, even if the vectors occur frequently. Rather, explanations are derived from the mutual relations of the factors in the concrete whole situation, both from the momentary condition of the individual and from the structure of the psychological situation (Lewin, 1931).

Although Lewin’s posture is one of arguing against Aristotelian psychology in favor of Galilean psychology, he also gives a way to open up psychology’s epistemological foundations of the early nineteen thirties.

“The accidents of historical processes are not overcome by excluding the changing situations from systematic consideration, but only by taking the fullest account of the individual nature of the concrete case. *It depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antitheses, and that reference to the totality of the concrete whole situation must take the place of*

¹⁶ These are methods for achieving agency, discussed in section §6 *Methodological determinism and researcher agency*.

reference to the largest possible historical collection of frequent repetitions” (Lewin, 1931, p. 65, author’s italics).

This is crucial for psychology from a methodological perspective. It means that the importance of a case study, and its validity as proof, cannot be evaluated by the frequency of its occurrence. Schegloff argues in the following way for the permanence of a single case regarding the study of human interaction:

“And no number of other episodes that developed differently will undo the fact that in these cases it went the way it did, with that exhibited understanding” (Schegloff, 1993, p.101).

Other methods are used to validate the case study, although they have always had the burden of proof from the larger scientific community, as case studies by definition are not assumed to occur frequently and with regularity and this has been the doxa. Lewin argues against lawfulness and regularity being the antithesis of the individual case. He argues this because such a view limits research in that it appears thus hopeless to attempt to understand the real and unique course of an emotion or the actual structure of an individual’s personality. In Aristotelian psychology, such problems are treated only in terms of averages. Psychology in terms of averages requires validation in general, on the average or as a rule. And finally, such “psychology does not regard exceptions as counter-arguments so long as their frequency is not too great” (Lewin, 1931, p. 49). Below I reproduce the summary Lewin gives in order to distinguish between Aristotelian and Galilean views of lawfulness and the differences in their methods.

Table 1. Views of Aristotelian and Galilean lawfulness (reproduced from Lewin, 1931, p. 54)

	For Aristotle	For Galileo
The regular is	lawful	lawful
The frequent is	lawful	lawful
The individual is	chance	lawful
Criteria of lawfulness are	regularity and frequency	not required
That which is common to the historically occurring cases is	an expression of the nature of the thing	an accident, only “historically” conditioned

If the concrete event is to be understood and if the individual is indeed also lawful, then the individual particularities of concrete cases cannot be ignored and the dynamics of whole contextualized situations need to be studied. In this case it doesn’t make sense to try and obtain general laws of processes by excluding the different influences of the situation and only accepting those factors which are observed under all circumstances. It follows that historical regularity is not the way to generalize. The goal is not to abstract from the situation, but rather to find the situations in which the determinative factors of the total dynamic structure are the most clearly discernable. A number of contemporary interdisciplinary researchers have this same objective, for example Levinson (2005) whose work I will present in §7 *Explanations That Compete Across Levels of Analysis*. My own model of the individual’s place within the group during knowledge co-construction also has this objective.

In the next section, I use a foundational sociology debate that addresses the role of the individual within the group to frame the debates just described in language sciences and in psychology.

5.1.3 The assumptions underlying process ontology and the inseparability hypothesis

Briefly, the researchers that subscribe to a process ontology argue that only process is real; events are the only elementary ontological objects¹⁷. In other words, only events and processes exist; entities such as individuals or communities do not exist and should rather be described through processes. The inseparability hypothesis therefore follows from a process ontology, because if only process exists, then the individual and the group cannot be separate entities (Sawyer, 2002). Through an analysis of the literature, Sawyer presents a number of problems with this view:

- Inseparability precludes the examination of the interplay between social structure and individual agency;
- If the self is purely sociological, then action cannot be motivated by internal intentions and reasons for actions are not properties of individuals, but instead are instantiated in the activity being studied. However, in asking whose activity or practice it is we study, it seems difficult to avoid focusing on individual properties.
- The ontological depth of the social world is considerably flattened if properties cannot emerge at a higher level (social) when they do not exist at a lower level (individual);
- Inseparability does not allow accounting for the causal role that social structures play in individual action *or* how processes interact;
- If all dimensions of the phenomena must be taken into account before any one can be examined, then how is it possible to analytically break into the complex cycle?
- Inseparability is foundationally incompatible with developmental science. Theories based on this assumption cannot explain structuring over time, because long-term development is difficult to study through a microsociological study of situated social practice¹⁸.

As Sawyer (2002) then goes on to argue, adopting analytic dualism alleviates these difficulties. Analytic dualism holds that individual properties and group properties of situated practice can indeed be analytically distinguished. A theory that accepts analytic dualism must also include postulates about the two-way causal relationship between individual and social properties. If achieved, socioculturalism could better connect with individual psychology on the one hand, and macrosociology (e.g. social class, social networks, educational level, geographic regions, race and ethnicity, gender, social power, etc.) on the other. This would in turn allow

¹⁷ A provocative example arguing for the importance of process, but that accepts events as real states is "that it is wrong to look for boundaries between preexisting social entities. Rather we should start with boundaries and investigate how people create entities by linking those boundaries into units. We should not look for boundaries of things but for things of boundaries" (Abbott, 1995, p. 857).

¹⁸ Some of the collaborative work that I review for this HDR proposes a way to do this (cf. §9.4 Young children's language development).

socioculturalism to participate in the major theoretical questions of contemporary social science (Sawyer, 2002):

- What is the best theory of processes, individuals and groups?
- What is the nature of the regularities holding between individuals and groups?
- To what extent does this relationship require psychology to incorporate theoretical models from sociology?
- To what extent does this relationship require sociology to incorporate psychological models of individuals?

This view of analytic dualism helps us to see how the aforementioned tensions could exist in better harmony. Doing research to render explicit internalized norms and values that guide behavior can be accomplished both from a psychological point of view focused on the individual and from a more macrosociological view focused on the social sources of such norms and values¹⁹. Whereas doing research to describe a procedure through which people co-construct their accountability to one another is done from a microsociological point of view. Similarly doing research to describe individual behavior in terms of characteristics of said individuals is a traditional goal in psychology whereas doing research to illustrate how a phenomenon is embedded within a process comprised of an individual or individuals in a context frames the question in a more sociocultural way²⁰. These forms of questioning are not incompatible with one another if researchers agree they support analytic dualism. Additionally, if they pursue the above questions in a collective manner, remaining open to other disciplines, a better and more complete understanding should be reached.

In the next section, I review research on learning situated within behaviorism, cognitivism, sociocognitivism, or socioculturalism and along the way present a selection of emblematic definitions of learning. Although it would be impossible to claim an exhaustive coverage of research across these paradigms and across the different disciplines studied (i.e. cognitive psychology, artificial intelligence, organizational behavior, organizational economics, the learning sciences, computer supported collaborative learning, developmental psychology, social psychology, sociology, evolutionary anthropology, business, management, and cognitive science), I will argue with a selection of emblematic examples, that although the study of learning began with looking at how an individual learned, the historical trend shows work that takes the group as the unit of study as well as work that accounts for both the individual and group perspectives in the study of learning. The approaches in this latter area are quite diverse and here I suggest that attention should be paid to their complementary nature. Given the tensions in linguistics, psychology, and sociology between the individual and the group that I mentioned in the previous section, this road is fraught with challenges. However, I also argue that taking this road will allow us to obtain the most understanding of the dynamics of learning, that is, if an open, interdisciplinary approach is embraced.

¹⁹ Other collaborative work I review here works toward this objective (cf. § 9.5 Understanding how emotion relates the cognitive and the social during debate).

²⁰ This is an approach akin to both §9.4 and §9.3 Conceptual change during group lab work in the junior high physics classroom.

5.2 Behaviorism

The following definition of learning takes two flavors: behaviorist and cognitivist. I begin with the first.

“Learning is the permanent modification, due to interactions with the environment, of the disposition of an individual to carry out a behavior or perform a mental activity” (Le Ny & Sabah, 2002, p. 30, my translation from the French).

Within the behaviorist view, a well-known example is Pavlov’s classical conditioning where a stimulus elicits a predicted physical response in canines because of it having been paired with a previously occurring reflex. Behaviorist psychology is a purely objective experimental branch of natural science (Watson, 1913). It is therefore aligned with the positivist ontological and epistemological stance.

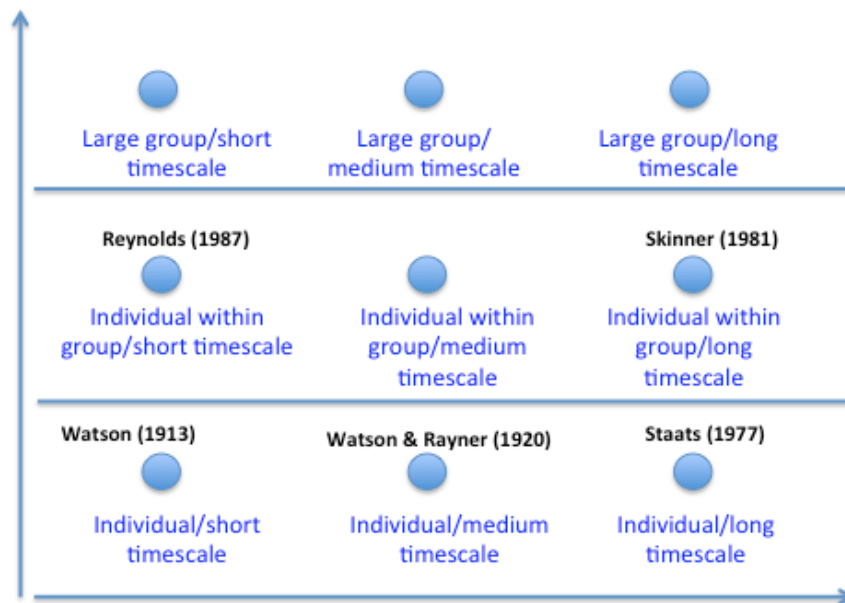


Figure 1. A selection of research on learning in the behaviorist paradigm

I argue that Figure 1 illustrates where the behaviorist paradigm has contributed according to a selection of emblematic studies. Watson & Ryner (1920) performed an experiment where they showed that Pavlov’s classical conditioning worked just as well on humans. They trained a 9-month old child known as Little Albert to be afraid of a white rat by associating a loud noise with the rat’s appearance that had already made Little Albert cry before the association was established. The training took two months. This work was carried out on a medium timescale, and concerns the individual. A behavioral view has also been applied to the study of groups, on a short time scale. Reynolds (1987) did his study in artificial intelligence and simulated individual birds²¹ within a flock where each bird navigated independently according to its local perception

²¹ Interestingly, the form of bird flocks is also used as a canonical example of emergence, a concept from studies of complexity, only just beginning as a science in the 1980s when this article was written. In the beginning of this century, Sawyer explains the phenomenon in the following way: “The V shape of the bird flock does not result from one bird being selected as the leader, and the other birds lining up behind the leader. Instead, each bird’s behavior is

of the dynamic environment, according to laws of simulated physics that rule its motion, and according to a set of behaviors programmed into it by the animator (i.e. the programmer who designs behavior). A behavioral model underlies the explanation that the aggregate motion of the simulated flock is the result of the dense interaction of the relatively simple behaviors of the individual simulated birds. This model could be considered behaviorist in that only birds' behavior is simulated; they have no significant mental states. However, the authors mention more sophisticated models that would take into account hunger, finding food, fear of predators, and a periodic need to sleep. Such considerations push them towards a more cognitivist view (see next section).

The behaviorist view can also extend to longer time scales for individuals, but this is more rare. For example, Staats (1977) trained his own infant child and then developed a child learning behavioristic technique based on rewards (food and/or toys) where “behavior-technicians” trained 4-year old “culturally deprived” children in a classroom equipped with a behavior modification apparatus that distributed a reward upon task completion. Training took place over a period of several months for a few minutes each day. Finally, the behaviorist view can also be extended to the social group within a long time scale, as illustrated by some of the lesser-known work of Skinner (1981). According to Skinner, the evolution of social environments or cultures is a kind of “selection by consequences”. A culture evolves when practices contribute to the success of the group in solving its problems. For example, a better way of making a tool, growing food, or teaching a child is reinforced by the result that this better way gives: the tool or food itself or the child who has become a useful helper. Although this reinforcing consequence for individual members is based on operant conditioning (i.e. reinforcements or punishment for behavior), it is the effect on the group that is responsible for the evolution of the culture.

In the work of both Reynolds and Skinner then, separate phenomena at the individual level and at the group level are related. Reynolds describes behavioral rules that the individual follows, but also argues that the group behavior emerges because the individuals follow the rules. Skinner argues that an individual bootstraps change, but that this change is visible on a societal level only because sufficient numbers of individuals carry them out.

5.3 Cognitivism

The cognitivist flavor of this first definition of learning — like behaviorism — understands learning as resulting from experience within a stable, objective world. However, instead of focusing on measurable stimuli and responses, the cognitivist focuses on rich descriptions of mental processes (Kirschner & Whitson, 1997), most commonly from within the field of cognitive psychology. In either case — behaviorist or cognitivist — these theoretical orientations lead naturally to methods that quantify relationships between environmental stimuli or conditions and measurable aspects of behaviors on relatively moderate time scales. The causality question asked is more often whether x caused y (e.g. comparison of experimental and control conditions), rather than how or why it did so, more present in qualitatively focused work.

based on its position relative to nearby birds. The V shape is not planned or centrally determined; it emerges out of simple pair-interaction rules. The bird flock demonstrates one of the most striking features of emergent phenomena: higher-level regularities are often the result of quite simple rules and local interactions at the lower level” (Sawyer, 2001, p. 555).

Figure 2 gives a sampling of where the cognitivist paradigm has contributed according to a selection of emblematic studies. Like behaviorism, research spans from short to medium/long time scales²² in paradigms where reality is stable and objective. Cognitive studies of individuals extend to the mid-term (8 weeks). For example, Coltheart (2012) proposes two competing cognitive level explanations for how it is we change from one phoneme to the next when reading. In other example, mindfulness training appears to alter sensitivity to respiratory sensations as measured by brain activity, which is in turn interpreted as information processing (Farb, Segal & Anderson, 2013). And Morrison & Chein (2011) trained individual subjects' working memory during medium to long time scales (up to 14 weeks) in order to enhance performance on individual's cognitive tasks.

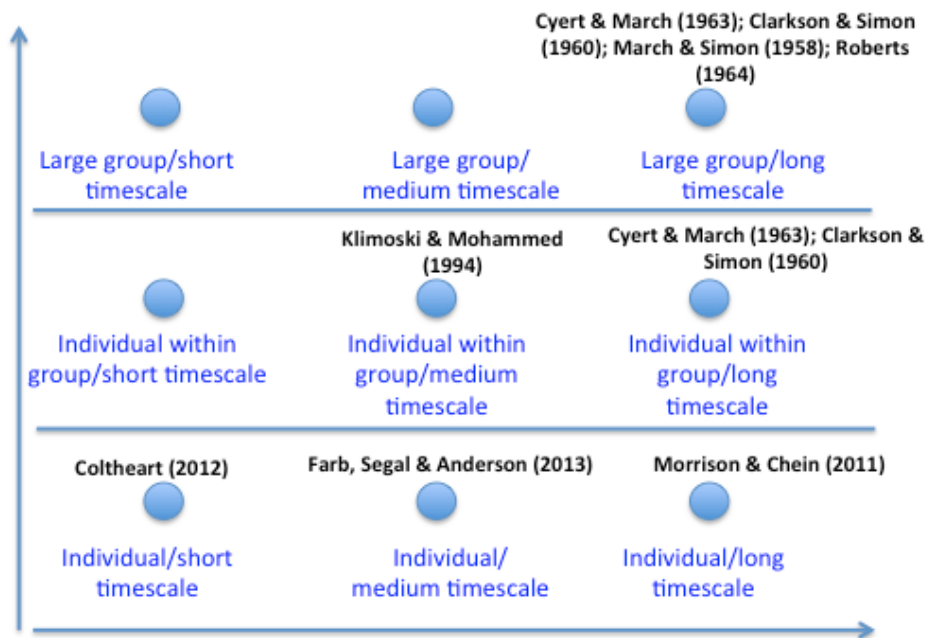


Figure 2. A selection of research on learning in the cognitivist paradigm

Concerning group learning at short to medium timescales, some cognitive science researchers extend the cognitivist mental models framework to teams (e.g. Klimoski & Mohammed, 1994; Langan-Fox, Code & Langfield-Smith, 2000) where mental representations are shared at the team level. Although the measurement techniques used to index team mental models are at the individual level, the phenomenon studied is at the level of the group (i.e. team mental models). Moving from the individual to the group employs a method of aggregation and whether there is emergence of an identity at the group level greater than the sum of its parts depends on the maturity and quality of group functioning (Klimoski & Mohammed, 1994).

²² The behaviorist and cognitivist paradigms would seem more adapted to research carried out on short time scales in that in the experimental approaches often used, variables are isolated in order to determine their effect and the longer time goes on, the more potential there is for an effect to be possibly attributable to some other cause. That said, these paradigms are perhaps also more adapted to the individual. Depending on the subject, individuals take a shorter time to learn than organizations and it may be difficult to monitor shorter-term changes in larger organizations.

In this information processing view within organizational psychology, an organization's learning began to be studied over the long term, in the early 1960s (Cyert & March (1963). They emphasized the processes of organizational decision-making in order to build their behavioral theory of the firm and built four sub-theories around organizational goals, expectations, choice, and control. As not all of these processes are directly observable (in the way that behavior is observable in the behaviorist paradigm), this approach fit nicely into the cognitivist paradigm where processes of an organization are related to the cognitive processes in the human brain. Therefore, once we consider the group, the cognitivist space differs from the behaviorist, in that instead of just a collective behavior, there is a notion of a collective mind. One way of looking at this is that individual and group learning become institutionalized in some way (e.g. Hedberg, 1981), for example as an organization's adaptation to its environment over time. This collective mind contains representations of the environment in which it operates, just as the brain contains representations of the outside world (Cyert & March, 1963).

In the heyday of artificial intelligence, Clarkson & Simon (1960) used simulations to reproduce part or all of the output of a behaving system where the system was an aggregate of units, each with a behavior. They argued that since computer programs could now modify themselves in an adaptive direction on the basis of experience (e.g. learn), then the phrase "A computer can do only what you program it to do" had lost its meaning. It becomes similar to the phrase "A human being can do only what his genes program him to do" (Clarkson & Simon, 1960, p. 925).

It seems that within the cognitivist paradigm then, the move from the individual to the group can be summarized as the aggregation of individual units, but as the following definition attests, aggregation is a flexible concept.

"The essence of aggregation is that the output value computed by the aggregation function should represent or synthesize "in some sense" all individual inputs, where quotes are put to emphasize the fact that the precise meaning of this expression is highly dependent on the context" (Grabisch, Marichal, Mesiar, & Pap, 2011).

For example, the behaving system can be an entire economy aggregating over group behavior or a particular unit (e.g. a human decision maker aggregating over cognitive processes). The output of a simulation can also be one aggregated element (e.g. an interest rate) or a set of thoughts, associations and actions of a problem solver (Clarkson & Simon, 1960). The group adapts to the environment through the technique of aggregating individuals together to represent group behavior. Therefore organizations can be understood as adaptive cognitive systems (March & Simon, 1958) or social organization could be read as a sort of architecture of cognition at the community level (Roberts²³, 1964). As Singer (1968) argues,

"Even though no social group can be properly thought of as having a personality, an attitude or an opinion, we may nevertheless attribute certain properties to a group on the basis of the distribution and configuration of these psychological properties. In other words, I would hold that the aggregation of individual psychological properties provides a quite sufficient base for describing the cultural properties of the larger

²³ Roberts, (1964) is cited in Hutchins (2000, p. 2): "such social organization could be seen as a sort of architecture of cognition at the community level".

social entity which is comprised of those individuals.” (Singer, 1968, p. 140, author’s italics).

Roberts (1964) and March & Simon (1958) appear at the level of large group learning in Figure 2 since their focus is on the group as a cognitive system, per se. However, the authors Cyert & March (1963) and Clarkson & Simon (1960) appear both at the level of large group learning and at the level of the individual as part of the group. Cyert & March (1963) focus mostly on the organizational level, and although there is mention of individuals, as part of coalitions, for example, individuals and groups are considered as equals at least on a functional level (e.g. having interests, specifying goals). Although I place them also at the middle level, the conceptualization of the relation of the individual to the group — as separate, yet mutually influencing, is not very elaborate. Clarkson & Simon (1960) appear at this middle level through their use of the aggregate, a specification of how the individual is related to the group. This technique of aggregation, however, will prove problematic in the following two paradigms: sociocognitivism and socioculturalism.

5.4 Sociocognitivism

The sociocognitive approach, as understood generally in social psychology, views the individual and the social as two separate units that establish relationships and interact without losing their distinctiveness (Glaveanu, 2011). An individual processes information coming from the environment where the environment is seen as a set of variables that stimulate the individual in different ways. Some are material, some are social, and they condition the activities and outcomes of the individual. Learning is thus primarily viewed at an individual level and localized within individual cognitive processes. True to a typically positivistic stance, researchers that objectify learning as a type of product or behavior agree that it can be measured experimentally and learning outcomes are often measured with pre and post-tests (Stegmann & Fischer, 2011). These authors are positioned more toward the individual end of the axis. Their goal is to test various theoretical assumptions between specific qualities of text-based knowledge building processes in Computer Supported Collaborative Learning and successful knowledge construction. They take individual learners as the unit of analysis because their main point of interest is individual knowledge acquisition (Stegmann et al. 2007). Their assumption is that such acquisition is a consequence of individual cognitive processes. Interestingly, they do admit that learners in a group cannot be regarded as mutually independent²⁴.

But as this interdependence violates the random sample prerequisite of their statistical procedures, they needed to find a solution so they can use their intended methodology. So they randomly select one learner from each of their groups in order to represent all the learners of that

²⁴ This illustrates an epistemological-methodological dilemma for socio-cognitivists and it has a flip-side for socioculturalists. The former do not subscribe to the inseparability hypothesis. In other words, they hold that the individual and the group are separate entities. Yet, they must find methods that account for and analyze the interaction’s interplay between individuals in meaningful ways. Conversely, many socioculturalists *do* subscribe to the inseparability hypothesis and argue that the individual cannot be separated from the social and cultural context. Yet, many of them also implicitly accept independent units (e.g. a ‘member’ of a ‘community’ for Lave and Wenger, 1991 indicates the existence of two entities and Rogoff’s (1997) examination of individuals’ roles in the context of their participation requires an analytic focus on specific individuals, on relationships between distinguishable individuals, and on specific individuals in distinguishable contexts, none of which are accepted by inseparability (Sawyer, 2002)).

group. Although this solves their methodological problem, it is not very meaningful, even within their own framework. This is where the concept of aggregation begins to be problematic in the sociocognitive paradigm. How can one learner represent all the learners, given that each learner is an individual that is interacting with others in meaningful ways that change outcomes (Lund, 2011)? Cress (2008) proposes a methodological approach that allows one to escape this contradiction of having only one individual represent the group, on the basis that individuals are mutually interdependent. Here again, we see a glimmer of the sociocultural view involving inseparability of individuals in interaction, but within an experimental approach that must be based on separability as it assumes individuals can be studied as units.

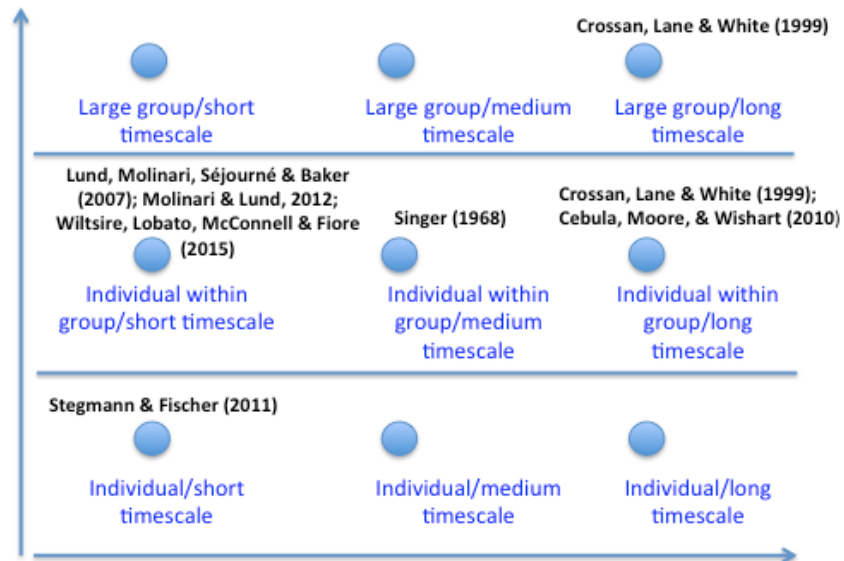


Figure 3. A selection of research on learning in the sociocognitive paradigm

Alternatively, authors can focus more on the quality of the interaction, for example, based on what mechanisms (e.g. argumentation, explanation) are known to favor learning (Lund, Molinari, Séjourné, & Baker, 2007). In this work, even though an experimental condition was built in order to both affect learning as it is measured individually and to affect interactional processes such as argumentation, the place of the individual in the group is considered by studying the meaning-making going on in the interaction while evaluating both the individual and the group as sociocognitive units, per se. In this study, the influence of two types of instruction for using an argumentation diagram during on-line pedagogical debates was the focus. In particular, how did using an argumentation diagram as a medium of debate (“Graph for debating”) compare to using an argumentation diagram as a way of representing a debate (“Graph for representing chat debate”)? In this case, a potentially causal link was searched for in terms of variables that produced outcomes, but these outcomes were explained both in terms of types of interactions that favored learning and quality of argumentative texts produced after the debate, as a result of the interaction.

The underlying epistemological assumption in this study is indeed that the individual and the social are seen as two separate units that establish relationships and interact without losing their distinctiveness, but at the same time this position regards human interaction as a process that is

mutually influencing. I argue that this position is similar to one of the sociocultural position described in the next section — but one combined with acceptance of analytic dualism.

In the context of understanding the sociocognitive development of individual children with Down's syndrome, Cebula, Moore, & Wishart (2010) have called for larger-scale, finer-grained, longitudinal work which recognizes the within-individual and within-group variability that characterizes this population. Clearly then, from a causal standpoint, this work is situated within the sociocognitive paradigm. Interestingly however, these authors suggest adapting for Down's syndrome a model on autism that distinguishes between levels of explanation. That alone is not suspect if one allows that these levels can be treated as separate units. However, the model builds on Morton's linear causal approach to explaining developmental change (Morton & Frith 1995; Morton 2004) by making a crucial modification. Instead of the causal chain being built unidirectionally from biology to behavior by passing through and being mediated by a cognitive level (Morton & Frith 1995), Cebula, et. al.'s model allows for bidirectional transactions among two levels of explanation. In their model (covering birth to 5-8 years), the causal chain essentially goes from neurobiology, to cognition, to social behavior, and finally to the social environment. But the social environment is also understood to influence social behavior, thus also adding the reverse direction of the causal chain and fundamentally changing its nature. For example, a Down's syndrome child's different patterns of social attention (social behavior) can influence the child's mother to show higher warmth to maintain interaction (social environment) which in turn can inspire a greater focus on people versus objects on the part of the Down's syndrome child (social behavior). This bidirectional action has potential epistemological consequences that the authors do not explicitly address.

Notably, if bi or multidirectional transactions imply reciprocal determinism²⁵ (Bandura, 1989), then there is a potential conflict between the proposed model and the sociocognitive paradigm that underwrites it. Indeed, how can one simultaneously separate the individual from the social (and this is what the sociocognitive paradigm requires) and yet also support a reciprocally, deterministic process ontology?

This doesn't necessarily have to be a problem. Sawyer (2002) relates how Archer (1995) makes a crucial distinction between interplay and interpenetration. Analyzing the interplay between the individual and the social allows for an emergentist type of social realism where emergent properties at the collective and the individual level are both distinct from each other and cannot be reduced to each other²⁶. These strata are separable from each other first because each one has properties and powers that belong only to one strata and secondly, the emergence of one from the other justifies their differentiation as strata (Archer, 1995). On the other hand,

²⁵ “Social cognitive theory favors a model of causation involving triadic reciprocal determinism. In this model of reciprocal causation, behavior, cognition and other personal factors, and environmental influences all operate as interacting determinants that influence each other bidirectionally” (Bandura, 1989, p. 2). This said, Sawyer argues that Bandura accepts analytic dualism, which is a kind of reciprocal determinism with causal links (Sawyer, personal communication, 2015).

²⁶ As Sawyer (2001) points out, contemporary sociological uses of the term “emergence” are contradictory and unstable. Some argue that collective phenomena are collaboratively created by individuals but are not reducible to explanations in terms of individuals. But for others, emergent social properties can exist even if such properties can be reduced to explanations in terms of individuals and their relationships. It's a matter of carefully specifying the relationship between the individual and the collective.

researchers who argue in favor of interpenetration between the individual and the social in a way that makes them inseparable are precluded from participating in a sociocognitive educational paradigm that necessitates separation of these units. Such researchers will work in the sociocultural education paradigm, (see next section), even if that paradigm has similar contradictions. If Cebula, Moore, & Wishart (2010) respect the constraints of their sociocognitive paradigm, they must formulate their study of the relationship between the individual and the group as causal interplay. They cannot formulate it as interpenetration, where causality is rejected between the individual and the group.

Organizational learning brings us research done on group learning in a longitudinal setting, carried out in the field of business and management (Crossan, Lane and White, 1999). Their take on such learning centers on strategic renewal of companies (March, 1991), defined as striving for a balance and recognizing the tensions between exploration (assimilating new learning) and exploitation (using what has already been learned):

“Organizational learning is a dynamic process. Not only does learning occur over time and across levels, but it also creates a tension between assimilating new learning (feed forward) and exploiting or using what has already been learned (feedback). Through feed-forward processes, new ideas and actions flow from the individual to the group to the organization levels. At the same time, what has already been learned feeds back from the organization to group and individual levels, affecting how people act and think. The concurrent nature of the feed-forward and feedback processes creates a tension, which can be understood by arraying the levels against one another” (Crossan, Lane and White, 1999, p. 532).

These authors conceive of learning as a “dynamic flow” (p. 533) that moves on the one hand, between intuiting, interpreting, integrating, and institutionalizing (i.e. the 4I model) while feeding forward from the individual to the group and finally to the organization. On the other hand, this dynamic flow is simultaneously also moving in the other direction, between institutionalizing, integrating, interpreting, and intuiting while feeding back from the organization, to the group, and finally to the individual.

Unsurprisingly perhaps, these authors — working in the field of management — liken the dynamic flow of learning to the dynamic flow of production. They briefly discuss how constraining a production flow can inform us of how a learning flow is similarly constrained. For example, bottlenecks may interrupt smooth flow if the organization cannot absorb the feed forward of learning from the individual to the group and organization. Investment in individual learning can thus become stockpiled if the organization has a limited capacity to absorb the learning. Products that stockpile up do not have any feelings as a result, but people that are being hindered in the learning process and in moving their ideas forward can and do become frustrated (Crossan, Lane & White, 1999).

But although they call for researchers and managers to change their approach to organizational learning by considering how the different parts of an organizational system impact one another, there is no underlying epistemological discussion, such as the one I mentioned previously between separating or not separating entities. Entities are assumed to exist apart from one another (e.g. individual, group, organization), yet flow is continual and simultaneous in both directions of granularity. In sum, it may seem that the authors are describing a complex system, but in fact, the relations they evoke are linear and so are therefore presumably traceable in terms

of cause and effect. But, in a complex system, there can be multiple, simultaneous, non-linear interactions taking place between components and then, it becomes impossible to keep track of causal relationships (Cilliers, 2001). Crossan, Lane, & White (1999) make reference to such a non-linear complex interaction — but without theorizing it as such — when they note that although the 4I's have been presented in a linear fashion for ease of explanation, the iterative nature of the processes at each level is critical. Notably, the dynamic nature of the learning process itself creates a tension between the feed forward and the feedback of learning within their model. The authors note the questions below and plead for looking at how different parts of the organizational learning system impact one another, although no specific methodological approaches are proposed:

- Is there a satisfactory level of intuitive, innovative insights in the organization?
- Do individuals have the motivation, understanding, capability and opportunity to interpret their environment?
- How do individual and group experiences help to develop shared understanding?
- How well do individual insights become shared, integrated and institutionalized in the organization?
- What impediments are there to integrating individual perspectives?
- How much of the organization's intellectual capital resides in individual heads?
- Is there enough institutionalized learning?
- How does institutionalized learning facilitate or impede intuiting, interpreting, and integrating?
- What is the nature of the interplay between the feed-forward and feedback processes?

A similar model for social cognition is proposed by Wiltshire, Lobato, McConnell & Fiore (2015). It is similar in that it stretches from the individual to the environment, yet its focus is more on the individual's perception, action & cognition in interaction with another individual whereas Crossan, Lane, & White's focus is on the back and forth between the individual, group, and organization. Although Wiltshire et al.'s model deals with pairs of individuals in social interaction during very short time scales, and we have already discussed this for the educational paradigm of sociocognitivism, I choose to present their work here, as it is fruitful to compare it to Crossan, Lane, & White's work in terms of causal relationships that cross levels of granularity, even if this latter research focuses on longer time scales.

Wiltshire et al.'s model refers to three levels of description and of explanation: 1) supra-individual (e.g. characteristics of the social situation such as temporal dynamics, degrees of coordination between participants, the nature of the relation between participants, environmental and contextual factors, etc.), 2) personal (e.g. direct perceptions of other's mental state or theoretical and/or simulative inferences on the reasons of the other's mental state), and finally 3) the relations between what goes on sub-personally including both functional explanations of sensory motor processes that provide direct perception of another's mental states or functional explanations of cognitive mechanisms that provide the inferences about another's mental state). This latter level is described by neuroscientific data that on the one hand can show the activation of particular areas in our brains when we either accomplish sensory-motor actions or when we perform cognitive processes, both being done in order to attribute mental states to others (Theory of Mind). They base this model on work from Bohl, & van den Bos (2012).

In the view of social cognition as illustrated in Bohl & van den Bos's model, the environment influences the sub-personal, but this influence does not move in the opposite direction. In other words, there is no arrow showing that the activation of a person's sensory motor data can influence the characteristics of a social situation in which the person finds him/herself. Yet, research shows that the biological differences in atypically developing individuals cause them to experience the world differently, and often cause the world to treat them differently (Diamond, 2009). It follows that there should be an arrow going from the sub-personal level to the supra-individual level.

And, in the new model proposed by Wiltshire, Lobato, McConnell & Fiore (2015), of two individuals in interaction), they do indeed choose to represent this bi-directional arrow between environment and personal level experiences. Individuals' sub-personal processes contain both processes enabled by direct perception and processes enabled by inference. These relate reciprocally to one another, and are at the personal level, to which they also relate reciprocally. In turn, the personal level reciprocally relates to the body level and the body reciprocally relates to the environment.

The transfer from the Wiltshire, Lobato, McConnell & Fiore model from the Bohl & van den Bos model is similar to the transfer from the Cebula, Moore, & Wishart model from the Morton & Frith (1995) model. In both cases, the latter version of the model turned a unidirectional causal link into a bi-directional causal link. Wiltshire et. al. made the body also act upon the environment instead of just the environment acting upon the body. And Cebula et al. transformed the unidirectional causal chain going from neurobiology, to cognition, to social behavior, and finally to the social environment by stipulating that the social environment also acted upon social behavior. However, in both cases, neither Wiltshire, et. al. nor Cebula, et al. explicitly discussed the epistemological consequences of making the arrow bi-directional. In other words, how can the interplay between multiple units in a complex system be studied and described when these units are mutually constitutive, as illustrated by the bi-directional arrow? That said, Cebula et al. call for the co-ordination of many different levels of explanations of behavioral outcomes in order to understand Down's syndrome and Wiltshire, et al. refer to Richardson, Dale & Marsh's work (2014) on dynamical systems as a way to examine social cognitive processes in an integrative manner.

5.5 Socioculturalism

In the sociocultural approach, there is interdependence between self and other (person, group, community, society) and the focus is on a symbolic mediation through cultural artifacts. Both knowledge and self are co-constructed and take place through social interaction (Glaveanu, 2011). Human activities are thus analyzed as part of cultural contexts that are mediated by language and other symbol systems. They can be best understood when investigated in their historical development (John-Steiner & Mahn, 1996). Most research here accounts for the role of the individual within the group, as illustrated by Figure 4. This is done in different ways that I will examine.

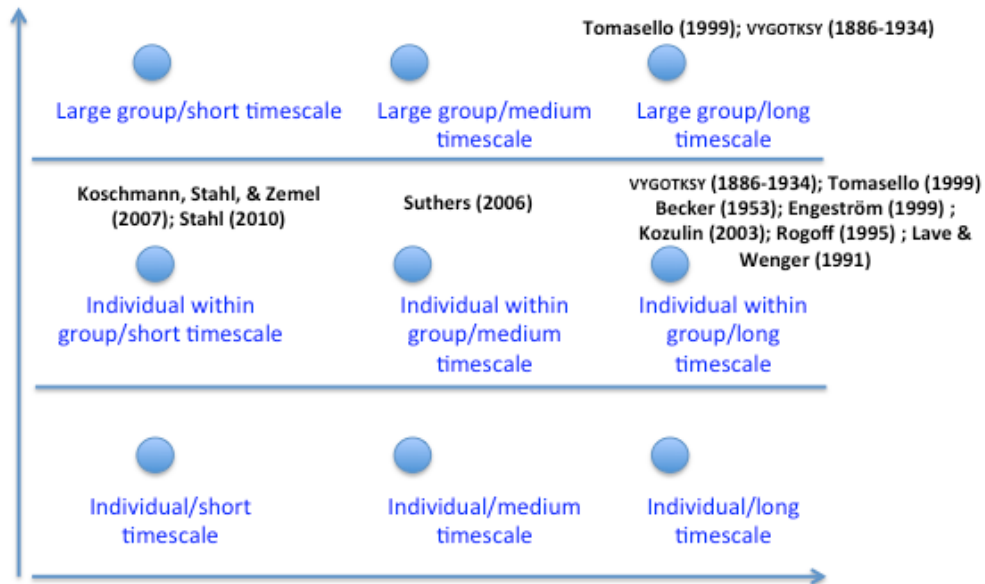


Figure 4. A selection of research on learning in the sociocultural paradigm

Some views of learning in this paradigm still consider the individual as the agent of learning. However, they move yet further in the direction of apprehending learning in the context of social interaction, with other individuals, groups or communities. Socioculturalism makes a strong ontological claim: process is not only a guiding orientation, it is also the fundamental nature of reality (Sawyer, 2002). It follows that the unit of analysis here is situated social practice rather than the bounded individual. Rogoff, in her work at the frontiers of psychology and anthropology, proposes this type of sociocultural approach, involving:

...observation of development in three planes of analysis corresponding to personal, interpersonal, and community processes. I refer to developmental processes corresponding with these three planes of analysis as apprenticeship, guided participation, and participatory appropriation, in turn. These are inseparable, mutually constituting planes comprising activities that can become the focus of analysis at different times, but with the others necessarily remaining in the background of the analysis” (Rogoff, 1995, p. 139).

Rogoff’s inseparable levels of analysis place her work as conceptualizing the individual within the group, but expose her to the problems pointed out by Sawyer (cf. § 5.1.3 *The assumptions underlying both a process ontology and the inseparability hypothesis*).

The sociologist Becker, writing in the early fifties, gave a definition of learning compatible with socioculturalism in his study of how a person becomes a marijuana smoker that uses for pleasure (as opposed to for “status”). As Hammersely (2011) explains, Becker’s definition went against dominant drug studies of the time, where drug use was explained in terms of individual attributes, indicative of a cognitivist paradigm. Instead, Becker (1953) aimed at an explanation that showed how:

“...the motivation or disposition to engage in the activity is built up in the course of learning to engage in it and does not antedate this learning process. For such a view it is not necessary to identify those “traits” which “cause” the behavior. Instead the

problem becomes one of describing the set of changes in the person's conception of the activity and of the experience it provides for him" (Becker, 1953, p. 235).

In this way, Becker rejects what he terms pre-dispositional theories (e.g. behaviorist, cognitivist) and he gives two main reasons. First, some users do not have the individual traits that are claimed to cause the behavior. Second, marijuana use can be quite variable over time, and pre-dispositional theories suggest that those who are pre-disposed will use marijuana regularly. Although the physiological effects of the drug are recognized, learning sustained marijuana use depends on "the meanings that users learn to ascribe to their experiences, the actions they base on these, and their interpretation of the results—in a continuing, interactive process" (Hammersely, 2011, p. 539). This is akin to the argument I put forth in section §5.1.1 *Significance versus meaning in psychology*. Becker then concludes the following about learning:

"This suggests that behavior of any kind might fruitfully be studied developmentally, in terms of changes in meanings and concepts, their organization and reorganization, and the way they channel behavior, making some acts possible while excluding others" (Becker, 1953, p. 242).

Becker's work also clearly gives a view of the individual within the group as meanings change within a social process. And not unlike Becker's thinking, the Vygotskian approach, within psychology, radically reoriented learning theory from an individualistic to a sociocultural perspective. In Vygotsky's perspective, "social" refers to both an interaction between two people (e.g. adult-child) and to wider interactions within culturally defined structures (Kozulin, 2003):

"Vygotsky strongly believed in the close relationship between learning and development and in the sociocultural nature of both. He proposed that a child's development depends on the interaction between a child's individual maturation and a system of symbolic tools and activities that the child appropriates from his or her sociocultural environment" (Kozulin, Gindis, Ageyev, & Miller, 2003 p. 5).

This view is aligned with distributed cognition in that more weight is given to the interactions with the environment that become part of the cognitive process. Cognitive processes are not only happening in the brain of the individual, while being supported by knowledge present in objects outside of the body. In addition, they are carried out first with the help of a more capable other (peer or adult) and are thus distributed socially between the peer and the learner or the adult and the learner. It is only then that what is learned is "internalized" into the individual. Further, the learner's interaction with a more capable other takes place within larger socially organized interactions. For example, in traditional societies²⁷, children are often directly involved in everyday work where in more industrial societies, adults create environments, tasks, and activities that are attuned to what the community considers to be a child's age appropriate needs

²⁷ "Tradition is oriented towards a legitimate reference to the past, while modernization is oriented towards the mastery of the future, the discovery of the new. Second, tradition and modernization are marked by a strongly different manner of intervening in the world, of producing goods and services; an empirical manner of learning over time and transmission across social relations that are embedded within the family or small communities, and a scientific manner that asserts itself as the result of research and calculation, embedded within complex social networks and formal organizations." (Langlois, 2001, p. 15830).

(Kozulin, 2003; Rogoff, 1995). In traditional societies, child-adult interactions are more contextual, usually less verbal, and aimed at the successful integration of the child into the activities. In industrial societies, the interactions are more child-centered, more verbal, and aimed at fostering skills in the child that may not have immediate practical value, but that are perceived as prerequisites for the child's future integration into a rapidly changing technological society.

In a similar socially oriented view, Tomasello (1999) — considered to be both a psychologist and an evolutionary anthropologist — argues that human cultural learning is possible because as individuals, we have the ability to understand others as beings like us, who have intentional and mental lives like our own:

“The human understanding of conspecifics [belonging to the same species] as intentional agents is thus a cognitive ability that emanates both from humans' identification with conspecifics, emerging very early in infancy and unique to the species, and from the intentional organization of their own sensory-motor actions, shared with other primates and emerging at around eight to nine months of age. Both of these skills are biologically inherited in the sense that their normal developmental pathways occur in a variety of different environments within the normal range (all of which include, of course, conspecifics).

[...] Children who understand that other persons have intentional relations to the world, similar to their own intentional relations to the world, may attempt to take advantage of the ways other individuals have devised for meeting their goals. Children are also at this point able to tune into the intentional dimensions of artifacts that people have created to mediate their behavioral and attentional strategies in specific goal-directed situations” (Tomasello, 1999, p 77-78).

Becker's, Vygotsky's, Roggof's and Tomosello's sociocultural views on learning do not fit into the positivistic stance, long the dominant view in science. Tongue in cheek, Ageyev (2003) notes the difficulties for Vygotsky: his “samples are small, data are unclear and/or ambiguous, advanced statistics are absent, and it is not clear how he controlled the independent variables” (p. 435). But since we can safely infer that these are not measures for success in Vygotsky's ontological and epistemological view, nor in Tomasello's, Roggof's or Becker's, it doesn't matter. Ageyev's joking illustrates that what we expect of one paradigm will not necessarily be required of another paradigm. This is often the source of misunderstandings in collaborative work, but the unreflective researcher can also be inadvertently influenced individually by work in another paradigm in a negative way. He or she may want to integrate a new approach without adequately examining whether the view expressed in this new approach regarding causality, for example, is compatible with his or her own view (e.g. replacing a uni-directional arrow with a bi-directional mutually influencing one without perhaps making completely clear how methods of investigation will be modified as a result).

Indeed, the question being asked in the sociocultural paradigm cannot be answered by describing the consequences attributable to deliberately varying a treatment (the goal of experiments). Rather it is “clarifying the mechanisms through which and the conditions under which the causal relationship holds (Shadish, Cook, & Campbell, 2002, p. 9)”. That said, Vygotsky performed developmental experiments (the translation chosen by Ageyev op. cit. p. 436), but these experiments always had a complex and authentic phenomenon as the object of his

research, not a laboratory surrogate and their view on causality was not variable-oriented, but process-oriented (cf. §8.3.1 Variance theory versus process theory). Vygotsky's goal was to describe a developmental path of a given phenomenon (e.g. mediated memory, scientific concepts, play) and in his experiments he investigated the developmental phases of the phenomenon in question. Ageyev (2003) argues that five phases can be discerned in most of Vygotsky's research:

- 1) the phase in which the given phenomenon does not manifest itself yet;
- 2) the phase in which its initial traces seem to appear for the first time, always with corresponding analyses of the psychological tools and social forces that bring this phenomenon to life;
- 3) the phase in which the phenomenon reaches its climax, always linked to social interaction and usage of tools;
- 4) the phase of its gradual "interiorization";
- 5) the phase in which it appears that the phenomenon in question has always been there, quite naturally, in our heads, resembling inherited individual property that was just waiting its time to be actualized.

At this level of descriptive detail, the differences between viewing learning from behaviorist and cognitivist perspectives as compared to viewing learning from sociocognitive and sociocultural perspective with an emphasis on cognition become clear. In the first two, learning is viewed as a change in an individual's disposition to carry out a behavior or perform a mental act. These changes are hypothetically due to the individual interacting with the environment where the objects or events considered as the environment are typically highly engineered treatments designed to provoke learning and the goal is to verify this hypothesis, often through the comparison of pre and post-tests that measure the changes. Such an approach also occurs in the sociocognitive perspective, but researchers have also begun to evaluate the quality of the human interaction by focusing on the process that leads to the production, evaluating both for indicators of learning. In a sociocultural perspective, learning as change is documented and described so the focus is entirely on how and with what series of phases the learning came about rather than on what specific factor provoked it. Although "change" is the product in a sense, it is not this product that is the focus, but rather the process that led to it.

Going back to Tomasello (1999) and his evolutionary perspective, much can be accomplished — culturally — in a quarter of a million years of learning tool and symbol use. Young children have countless learning experiences by actively engaging with their cultural environments over the course of several years, days or even hours. Understanding how these interactions define human cognition places the "social" at a yet higher level of granularity:

"...my focus is only on the species-unique aspects of human condition. Of course, human cognition is in large measure constituted by the kinds of things that appear as chapter heads in traditional Cognitive Psychology text-books: perception, memory attention, categorization, and so on. But these are all cognitive processes that human begins share with other primates (Tomasello and Call, 1997; Tomasello, 1998). My account here simply presupposes them, and then focuses in Vygotskian fashion on the kinds of evolutionary, historical, and ontogenetic processes that might have transformed these fundamental skills into the special version of primate cognition that is human cognition" (Tomasello, 1999, p. 10-11).

As Tomasello aligns with Vygotsky in working to understand how culture is propagated, the approach to causality is also more descriptive, where the “how” of learning is more important than the “what” that may form the focus of an intervention during an experiment. Thus, these theoretical perspectives lead to methods that examine a much broader range of time scales and relevant objects (e.g., the role of cultural histories and artifacts). Vygotsky and Tomasello alike contribute to both research on long timescale group learning on the level of the culture (labeled as “large group” on Figure 4), while also conceptualizing the relation of the individual with the group, so they appear in two places.

Other researchers in this paradigm concentrate on the place of the individual within group learning rather than on the evolving larger group (society, culture) as influenced by the socially exposed individual. For example, through learner articulation:

“...articulation involves not only putting ideas into words but also the bringing together and fitting together of words (and hence ideas) in the process. Thus, the term *learner articulation*, as we use it here, accommodates the notion that learners may achieve new understandings, through the process of combining ideas, in the course of expressing them” (Koschmann & LeBaron, 2002, p. 250).

Koschmann’s work is particularly interesting in that it allows researchers to consider learning as something other than as he puts it, “a purely occult mental process amenable only to indirect study” (Koschmann & LeBaron, 2002, p. 251). In Koschmann’s view collaborative problem solving provides the context to study learning directly as an interactional phenomenon, rather than as a mental one. And he describes his method as one of documenting how learners do articulation²⁸. This is observable as learners actively use their hands and bodies as well as aspects of the material environment while displaying and co-elaborating their understandings.

Also in the Computer Supported Collaborative Learning context, Suthers (2006) and colleagues have been inspired by ethnomethodology and conversation analysis in order to also argue that not only the meanings of utterances are contextual and negotiated in order to support action, but also the same is true for nonlinguistic representations that support action. They use the term “uptake” instead of “adjacency pair” (e.g. Schegloff & Sacks, 1973) as a generalized building block of interaction that can be constructed of relations between nonadjacent events and found in diverse media. They create a time-ordered representation of individual contributions and their characteristics such as actor and linguistic content and form a relational graph showing how words, phrases and ideas are echoed across contributions, how actors address each other, etc. and this can be converted into a summary representation of uptake evidenced by such relations, which in turn is converted to a sociogram of who uptakes from whom with what frequency. This method carefully documents the process of individual contributions leading to a collaborative result, and how those contributions played off each other.

Stahl (2010), again in Computer Supported Collaborative Learning, argues that there are distinct phenomena and processes at the individual and community levels, but also at the small

²⁸ Here Koschmann is making reference to the work by John Langshaw Austin’s *How to do Things with Words: The William James Lectures delivered at Harvard University in 1955, 1962* (eds. J. O. Urmson and Marina Sbisa), Oxford: Clarendon Press.

group level and that analyses at each level reveal different insights. Although he does not frame it in this way (but see Stahl, 2015), this is a view that fits well the concept of emergence, one of the central tenets of complexity theory, where the claim is that particular kinds of systems are capable of giving rise to radically new properties not present in the components of the system (Bechtel & Richardson, 2010):

“H0 (collaborative learning hypothesis): A small online group of learners can – on occasion and under favourable conditions – build group knowledge and shared meaning that exceeds the knowledge of the group’s individual members” (Stahl, 2005, p. 87).

Stahl (2010) gives an alternative both to sciences of the individual and to sciences of the community (e.g. Engeström, 2008; Lave & Wenger, 1991) that instead focuses on the collaborative small group, a middle element of analysis that is important for understanding the other two. He also argues that a reductionist stance may not hold between the group and individual levels of analysis:

“Processes at the small-group level are not necessarily reducible to processes of individual minds, nor do they imply the existence of some sort of group mind. Rather, they may take place through the weaving of semantic and indexical references within a group discourse” (Stahl, 2010, p. 26).

But to what extent and exactly how is the individual taken up within the group for Stahl? Stahl proposes to study “interpersonal trains of thought, shared understandings of diagrams, joint problem conceptualizations, common references, coordination of problem-solving efforts, planning, deducing, designing, describing, problem solving, explaining, defining, generalizing, representing, remembering and reflecting as a group” (Stahl, 2010, p. 29-30). In this way, he conceives of the small group as a distinct level, in between the individual and the community and he develops an account of the relationships between the individual, group, and community.

5.6 Conclusions and implications for modeling the individual within the group

This was a limited review of an interdisciplinary literature— and will have necessarily missed relevant articles, but it was undertaken for two objectives. First the analysis reveals how theoretical assumptions about learning, causality, and reality have all changed as new paradigms have evolved. Similarly, research questions have changed. Second, a reflection on this evolution suggests areas where epistemological and ontological difficulties are located, but also where disciplines could collaborate on elaborating the role of the individual within the small group, while relating that to larger communities. Table 2 illustrates only some examples from the review. The educational paradigms reviewed appear as column headings and the rows express five elements that change across these paradigms.

	Educational paradigm				
	Behaviorism	Cognitivism	Sociocognitivism	Socioculturalism	
Scientific paradigm Salomon, 1991	Analytic		Analytic, Systemic		
View of reality	Objective: independent of human experience		Objective or subjective: individual and context are separate units between which causal relations exist <i>or</i> they are interdependent, inseparable and constituted through shared meanings		
Theoretical assumptions about causality	Since it is possible to isolate causes as variables: did x really cause y? (e.g. comparison of experimental and control conditions)		Causes can be isolated as variables, but processes can also be causes	Causality is rejected OR Causality itself is observable through a study of process that clarifies the mechanisms through which and the conditions under which a causal relationship holds	
Example theoretical assumptions about learning occurring in a particular paradigm	Learning is a physical response to a stimulus (the brain is a black box)	Learning is a mental process in the brain (or due to interactions with the environment)	Learning is <i>primarily</i> viewed at an individual level and localized within individual cognitive processes, yet influenced by social processes New ideas and actions feed forward from individual, through, group, to organization and back again	Learning is an interaction between a child's individual maturation and a system of symbolic tools and activities that the child appropriates from his or her sociocultural environment	Learning is a set of processes at the small group level that take place through the weaving of semantic and indexical references within a group discourse
Example roles of the individual within the group occurring in a particular paradigm	Group behavior emerges because individuals follow rules or because sufficient numbers of them perform an action	The aggregation of individual psychological properties provides a sufficient base for describing the cultural properties of the larger social entity which is comprised of those individuals	The individual and the social are seen as two separate units that establish relationships and interact without losing their distinctiveness	Only events and processes exist; entities such as individuals or communities do not exist and should rather be described through processes OR Analytic dualism is possible and examines the two-way causal relationship between individual and social properties, including individual development and the evolution of social structure	

Table 2. A selection of characteristics from the four educational paradigms

First, the type of scientific paradigm present in the research conducted is either analytic or systemic:

“The analytic approach mainly assumes that discrete elements of complex educational phenomena can be isolated for study, leaving all else unchanged. The systemic approach mainly assumes that elements are interdependent, inseparable, and even define each other in a transactional manner so that a change in one changes everything else and thus requires the study of patterns, not of single variables” (Salomon, 1991, p. 10).

In sociocognitivism and socioculturalism, both are present. Second, the view of reality most commonly held is either an objective view, where reality is independent of human experience (present in behaviorism, cognitivism, and sociocognitivism) or a subjective view, where reality is socially constructed (present mainly in socioculturalism). Whether research is analytic or systemic and the perceived view of reality (objective or subjective) are both closely correlated to theoretical assumptions about causality — the third row. In an analytic, objective reality, it is possible to isolate causes as variables and researchers focus on whether x caused y through controlled experimental settings. In an objective reality, it’s also possible to treat processes as causes. In a subjective reality, either causality is not admitted because of the inseparability hypothesis or causality can be observed (contrary to Hume), and in that case either bi-directional causal relationships can be identified or other methods (such as decomposition and localization — Bechtel & Richardson, 2010) need to be mobilized for dealing with complex systems where multiple, simultaneous, non-linear interactions can take place between components. Unsurprisingly, the ways in which learning is studied across the paradigms — the fourth row — also vary with these dimensions, as does the role of the individual within the small group (fifth row) and how it can therefore relate to a larger community. The questions surrounding aggregation in the sociocognitive paradigm and the tension concerning a process ontology and its accompanying inseparability hypothesis within the sociocultural paradigm may illustrate that these paradigms are evolving (Kuhn, 1970). Interdisciplinary work may be useful, for example between conversation analysis and psychology in the sociocultural paradigm in order to combine analyses of interaction-in-context with characteristics of individuals²⁹. In the sociocognitive paradigm, work in organizational learning could be combined with microsociological studies of situated social practice that zoom in at different time periods. In sum, given this review, multi theoretic modeling of complex systems that crosses levels of analysis (individual, small group, organization/community/culture) is where the understanding of learning is headed. This may be an obvious conclusion, but given this, the goal of this HDR is to describe some of the ways collaborators and I have achieved this and how we may now move forward.

In the next section, I explore the concept of “methodological determinism”, defined as the proposition by which methods incarnate theoretical assumptions and cannot escape them (Yanchar & Williams, 2006). It is this proposition that underlies the “incompatibility thesis” which states that methods from different traditions are immutably based on theoretical assumptions that are incompatible and cannot be combined without resulting in incoherence (Yanchar & Williams, 2006).

²⁹ This is work I am currently pursuing in the context of the JEN.lab project with post-doc Vicky Markaki and David Shaffer and his colleagues from the University of Wisconsin in Madison.

Arguing against methodological determinism is relevant for this HDR because if researchers show agency — in this case, if they choose the theoretical assumptions that underlie their methods — they can fruitfully combine perspectives. In §9 for each collaborative article I review, I describe the choices we made about theoretical assumptions, but also about definitions, analytical constructions, and views on learning.

6 Methodological determinism and researcher agency

“Whether or not it is clear to you, no doubt the universe is unfolding as it should.” Max Ehrmann, Desiderata: A Poem for a Way of Life

If we consider that methodological determinism is true, there is a research practice that can be dangerous called “naïve methodological eclecticism”. This is when researchers combine methods that “work” without worrying about whether the underlying theoretical assumptions are compatible (Yanchar & Williams, 2006). If the theoretical assumptions are anchored in the methods, the researcher cannot act on them and if (s)he combines methods with contradictory assumptions, then the resulting research is incoherent. On the other hand, if we consider that methodological determinism is true only in certain cases, and that the researcher has agency by choosing which theoretical assumptions underlie the method used, then methodological eclecticism becomes a conscious process and the researcher does not risk performing incoherent work.

This section is organized as follows. In order to uncover the conditions under which methodological determinism is true and how naïve methodological eclecticism can bring researchers into an impasse, I will first review two examples of methods — Social Network Analysis (SNA) and the transcription of human interactions. My intent is to explore to what extent each of these methods incarnate theoretical assumptions and to what extent a researcher’s voluntary action is fundamental for deciding what epistemological orientation a particular application of a method may take. Second, I present the Productive Multivocality project (Suthers, Lund, Rosé, Teplovs & Law, 2013). The objective of this project was to help researchers reflect on how their epistemologies may be different, given their theoretical and methodological frameworks and we accomplished this by having them compare their respective analyses on shared corpora. I present this project in order to give an example of a participating researcher who was able to invent a mixed method by using conscious methodological eclecticism after being introduced to researchers using different methods. Finally, I will make some concluding remarks while discussing the consequences of this type of reflection on the relation between learning and teaching. As for the previous section, this analysis sets the stage for the review of my own collaborative work in §9 *Building a MULTi-theoretical and Interdisciplinary model of GRoup And INdividual (“Multi-grain”) knowledge building*.

6.1 Fostering dialogue between researchers using diverse methods

Varied analytical approaches are used in multidisciplinary communities of research. Communities focusing on research in education are not an exception; the diversity of methods is anchored in disciplines as varied as educational sciences, psychology, cognitive science, computer science, linguistics, anthropology or sociology and each of these disciplines also contains multiple approaches. This diversity can have as a consequence that the different traditions involved do not communicate directly, each working in their own sub-communities in parallel (Lund, Rosé, Suthers, Baker, 2013). Worse, certain traditions can reject the research in other traditions as not being interesting, or even being invalid, under the pretext that their theoretical assumptions are not “correct” or that their methods do not respect a collection of a priori constraints, held to be necessary for the tradition doing the criticizing. My position is that it is not desirable to produce a community of research in education that would be theoretically and methodologically homogenous. It’s beneficial for researchers whose epistemologies differ to have dialogues about their analytical constructs (Abend, 2008) and so such dialogue should be

encouraged and supported between research traditions. But how can this be done? If we examine first how methods may or may not actually carry assumptions and second how these assumptions can be bypassed by a researcher's deliberate practice — while a researcher remains cognizant of his epistemological framework and that of others — then fruitful exchanges are possible. Such exchanges should be deliberately put into place and organized by the research communities in education.

6.2 What assumptions does Social Network Analysis carry?

Let's first look at Social Network Analysis in order to appreciate how an assumption can or cannot be anchored within a method. SNA (e.g. Wasserman & Faust, 1994) has the objective of modeling and analyzing the relations within a domain. This works by 1) identifying social actors and other entities of interest within a network, modeled by nodes in a mathematical graph, 2) describing these entities in terms of the relations between them, modeled by arcs of a graph, and possibly including arc attributes and 3) analyzing the structural properties of the graph in order to draw conclusions on the social system represented by the graph at multiple levels (e.g. actors' properties represented by nodes, properties of local structures as coherent sub-groups and properties of global structures as the degree of connectivity and the distribution of graph parameters).

In this kind of approach, where could the theoretical assumptions be located? Marin & Wellman (2010) argue that SNA is not just a method. Rather, it is a perspective on causality to the extent that the method takes a position on the relation between cause and effect. In their view, the researchers who use SNA hold a particular point of view on the reasons why people act in the ways they do. This vision of causality is that people have similar behavior because they have similar positions within the social network. Incidentally, it is with this vision that SNA was built at its origin. However, other researchers (e.g. Barabási & Albert, 1999) now use SNA with another view of causality. These researchers think that people do what they do because they have similar attributes (e.g. personality). In the first view, the position of people in the network creates similar constraints, opportunities or perceptions and in consequence, their behavior is similar whereas in the second view, people do similar things because they are similar by nature.

In the literature on the SNA method itself, there are research strands that use this method as a tool to compare these two competing visions of causality. For example, let's consider homophily — the phenomenon in which nodes representing similar attributes would be more often connected together than heterogeneous nodes. A first hypothesis (consistent with the network-centric view) is that homophily is due to diffusion where nodes that are already connected become more and more similar to each other (Rogers, 1964). In other words, the nodes that are already in contact take on similar characteristics by simple association, because they are in contact. But there is a second hypothesis that claims it is more probably that nodes connect to others if these other nodes have similar attributes to the nodes doing the connecting. This second hypothesis is a form of preferential attachment (Barabási & Albert, 1999) that privileges the influence of local attributes. In the first hypothesis, the nodes become similar thanks to their association and in the second hypothesis, the nodes are already similar and it is for this reason that they become associated. I thus argue that SNA is not deterministic in that it does not require a researcher to adhere to one or the other view of causality, even if it is with the network-centric view of causality that SNA was originally developed.

6.2.1 What are the consequences for the incompatibility thesis?

If either one of these two views of causality can underlie the usage of SNA, then what are the consequences for the incompatibility thesis? I remind the reader that this is the thesis that methods from diverse traditions are based on incompatible theoretical assumptions and cannot be combined without incoherence. So far, we have seen that a method is not always restricted to a unique theoretical assumption and can be used in a completely coherent way with two different assumptions. These same two competing theoretical assumptions are found in attribution theory (e.g. Mandel, Hilton & Catellani, 2005) where behavior is either attributed to internal factors (i.e. personality) or external (i.e. situation); it is thus a well-known tension.

What should we pay attention to as researchers then in terms of the incompatibility thesis if more than one theoretical assumption is possible? There are, indeed, assumptions inherent in the usage of a method, but these are methodological assumptions and not theoretical ones. In order to use SNA effectively — whatever one's theoretical position on causality — the analyst must agree that 1) it is possible and useful to identify actors or entities in the data and model this data as nodes in a network and 2) that the binary relations between these actors and entities be pertinent and useful for understanding the phenomena of interest. In addition, the researcher who combines SNA with another method should make sure that the theoretical assumptions chosen to work with SNA are compatible with the theoretical assumptions of the other method in order to not produce incoherent results. If the researcher is conscious of his own theoretical assumptions and the methodological assumptions carried by the technique, then he can analyze his data in a coherent manner.

6.3 What assumptions are carried by the transcription of human interaction?

Transcripts (e.g. a text that mirrors oral statements of participants) are analytic representations created to represent the original data (e.g. an audio recording), which themselves represent the phenomenon itself. Ochs (1979) provides a detailed discussion of how the notational format of the transcript may have biases that can be derived from theoretical assumptions. For example, consider the transcript of the interaction between an adult and a child. When the transcript is written in sequential order of the utterances (cf. left in Figure 5) - which is usual for Conversation Analysis, the reader reads the statements of the interlocutors from top to bottom as a dialogue where the utterances follow chronologically. The overlapping speech is designed by square brackets “[“ and elongated vowels by “:…””. In this case, the reader interprets a statement of a speaker followed by a statement of another party as a logical pair of the type "question-answer" or "greeting-greeting," where the reader expects a response if a question was asked and where the reader expects a greeting in return for a first greeting. These concepts of "adjacency pairs" (e.g. question-answer, greeting-greeting) and "preferential selection" (e.g. prefer a certain type of response to some type of statement) are fundamental to the theory underlying Conversation Analysis (Sacks, Schegloff & Jefferson, 1974; Sacks & Schegloff, 1979). One of the goals in this tradition is to describe how these adjacency pairs occur in conversation and when there is not the preferred response, describe how this happened, showing why it is an exception to the rule. Conversational Analysis is built on the theoretical assumption that participants co-construct their own interaction by self-organizing in real time, and the researcher's work is to describe how this occurs (cf. §5.1.1 *Pre-established rules versus co-constructed experience in sociology and linguistics* for a discussion on the theoretical assumptions of interactionists and those who study action theory) In addition, the representation on the left in Figure 5 makes it easy to locate the

voice of one speaker that overlaps with the voice of another (denoted by brackets and often aligned vertically). This is especially important for understanding co-constructed utterances (e.g. lines 4 and 5).

<i>N°</i>	<i>Part.</i>	<i>Time</i>	<i>Utterance</i>
1	Child	01:23	i was at school [and
2	Adult	01:24	[what did you do at school
3	Child	01:24	and then and then there was this lady
4	Adulte	01:24	the lady wa::[:s
5	Enfant	01:25	[she was my teacher's friend

<i>N°</i>	<i>Time</i>	<i>Child</i>	<i>Adult</i>
1	01:23	i was at school [and	
2	01:24		[what did you do at school
3	01:24	and then and then there was this lady	
4	01:24		the lady wa::[:s
5	01:25	[she was my teacher's friend	

Figure 5. Two transcription formats, each renders salient different phenomena.

On the other hand, if researchers are interested in language production of young children, this form of transcription will risk masking the nature of their speech or at least make it less recognizable. Young children do not necessarily attempt to make their contributions relevant to a previous contribution of a partner. They can engage in narratives where their contributions are relevant to their own previous contributions (see right of Figure 5, lines 1 and 3). This example is designed to illustrate the concept and gives only an overview; here a child continues his narrative without paying attention to the adult intervention, but the phenomenon may be even more marked with much longer child narratives - almost monologues - punctuated by several adult interventions not taken into account by the child. In order to more easily identify the nature of child speech, Ochs suggests using a more appropriate form of transcription, designed to highlight this feature: place each participant in their own column, aligned horizontally with respect to time. This representation makes the individual narrative of each participant more prominent, and shows how the child's speech can be built on his own stated history (see right of Figure 5, lines 1 and 3). The inconvenience is that speech overlap becomes less prominent, but this just illustrates the choices to be made. What is the phenomenon that the researcher wants to emphasize, in its analytical objectives and theoretical orientations? The transcription conventions that the researcher selects are precisely designed to help meet objectives and are a function of researcher orientations.

6.3.1 What are the consequences for the incompatibility thesis?

Here we ask the same question as with SNA. What can we conclude from the previous discussion on the transcripts? Can the use of a transcript trap the researcher in the thesis of

incommensurability that claims that methods based on incompatible theoretical presuppositions cannot be combined without inconsistency? As with SNA, there are methodological assumptions inherent in transcribing. For example, if a researcher uses a transcription, he assumes that it is useful to view an interaction as a discrete or overlapping sequence of utterances where this sequence is ordered in time. We do not have as clear an example of two competing theoretical assumptions for transcripts, as we did for SNA, but the transcript is an elastic material with which to build other representations that restrict and select narratives about how the world was at one time through a combination of symbolic signs, iconic and indexical (Duranti, 2006). As Duranti stated (opt. cit.), both the evolution of transcription and the changing interpretations of a transcript can give us a record of our theoretical and epistemological changes. We therefore come to the same conclusions as we did with SNA - theoretical assumptions are not rooted in the method of transcription, but rather in how transcription is put to use. And what happens if a researcher combines transcribing with other methods? As with SNA, the analyst must ensure that the choice of theoretical assumptions made for using a transcript is consistent with those of the other methods so as not to be in contradiction. If the researcher is aware of his theoretical and methodological assumptions, he can act accordingly and consistently.

6.4 How the study of SNA and transcriptions criticize methodological determinism

In summary, how do the examples on SNA and transcripts help us criticize methodological determinism, that is to say, the proposition that diverse methods are based on theoretical assumptions immutably anchored in the methods themselves? How do these two examples show that researchers are able to use the methods in different settings from those with which the methods were developed without creating inconsistencies? Let's take the last question first. The SNA example showed that some methodological assumptions were indeed rooted in the method itself (modeling entities as nodes and assuming relationships between entities) while other assumptions (theoretical this time) were not rooted in the method itself (i.e. two views on causality were possible with the same representation). The example of the method of the transcripts showed that the practice of shaping transcripts could render more salient specific characteristics of the interaction, according to the researcher's interest. The chronological representation of utterances (left of Figure 5) facilitates the location of adjacency pairs (e.g. a response following a question) and that same representation allows easy location of overlapping speech by vertical alignment of the square brackets. On the other hand, the researcher who is interested in other phenomena can shape the transcription in order to make *them* salient. In this case, there is a change in practice and not a submission to the theoretical assumptions inherent in a method. The right side of Figure 5 favors the perception of other types of expected phenomena that are being studied (e.g. the child who builds his story on his own statements). But we must remember that making one phenomenon salient (e.g. the monologue of the child) can result in making another phenomenon less prominent (e.g. the precise location of overlapping speech). Unlike the SNA example, there are no alternative views of theoretical assumptions since the concepts of adjacency pairs and preferred answers are still relevant and overlapping speech can still be taken into account. Rather, it is a change of focus; the gaze of the researcher is on adult-child interactions, and specifically on the nature of child in relation to itself as opposed to in relation to other participants' speech. The researcher reorganized the transcript format to make salient specific phenomena of interest. In each case, theoretical assumptions are projected onto the model of the interaction, whether in the case of SNA (i.e. causality) or in the case of transcripts (i.e. type of interaction).

In both examples (SNA and transcription), the chosen representation includes methodological assumptions. In SNA, a researcher with agency chooses a theoretical assumption and when transcribing, a researcher with agency reflects on the events he wants to highlight and selects a representation to make salient phenomena of interest, without committing to the theoretical assumptions on which he can rely during analysis.

6.5 The Productive Multivocality project

In this section, I present a five-year collaboration involving researchers who explored the possibilities of productive dialogue between different traditions that analyze group interactions, focusing on educational contexts. My goal for this section is to show how a researcher located within this project has put in place a so-called "mixed method" using conscious (rather than naïve) methodological eclecticism. He therefore does not fall prey to the incompatibility thesis.

The Productive Multivocality project was motivated by the fact that researchers from different traditions in multidisciplinary areas such as Learning Sciences, could benefit from working more closely, but with an approach that respects the diversity of existing traditions. Teams of analysts analyzed five corpora where each analyst represented a particular theoretical and methodological tradition. The project was called the Productive Multivocality Project (Suthers, Lund, Rose, Teplovs, & Law, 2013) because the main objective was to engage the "voices" of the multiple traditions interested by group interactions in a productive dialogue. The project objectives were defined at several levels. In addition to the research objectives of individual participants, the collective aim was firstly to get different academic traditions working on the problem of understanding group interactions in educational contexts. Secondly, it was to reflect on and change the participating researcher practices and learn how multiple traditions could "talk" to transcend their differences while mobilizing them. In other words, the multivocal efforts on interaction analysis not only produced research results regarding the learners studied, but also served as a context for a research program on the interactions between the participating researchers. There was an additional overarching goal of sharing this experience with other groups of researchers who are working in multidisciplinary contexts.

Researchers studying group interactions come from a wide variety of disciplines, use different epistemological frameworks, and do not necessarily interact in the same communities. It makes sense therefore to develop a forum in which these researchers could examine the epistemological frameworks that attempt to take into account similar empirical data or handle similar concepts. The premise of the Productive Multivocality project is that it is productive to compare epistemologies through multiple analyses of shared corpus, without necessarily having the same research objective. Doing such comparisons generates fundamental questions for the research communities involved, for example, how did the theoretical assumptions lead the research, (the central query of this article)? Also, attempting to combine theoretical perspectives has the potential benefit of showing why it would be interesting for two isolated traditions to work together.

One problem that can happen in these multidisciplinary contexts is the one pointed to at the beginning of this section. When researchers begin to use methods of researchers from other traditions, they are subject to naïve methodological eclecticism if they do not reflect on the associated methodological and theoretical assumptions. There is also another danger. When a researcher is in tradition A, but borrows a method from tradition B, the latter method may simply be used in the service of tradition A to the detriment of its fair value (Dourish & Button, 1998). It

is not negative in itself if the research objectives are achieved, but an opportunity may be missed to question the assumptions of the dominant tradition or establish a dialectical confrontation between the two traditions.

6.5.1 Changing theoretical assumptions in order to coherently apply a new method

In this penultimate section, I showcase a researcher from the Productive Multivocality project who borrowed a method from another research tradition, thus setting up a so-called "mixed method" approach. He managed this in full awareness of the theoretical and methodological assumptions of the methods involved.

Chiu is the author of a method called Statistical Analysis Discourse (SDA). The objective of his method is to statistically model the features of speech turns and how they affect the occurrence of further speech turns with other features. For example, he often applied his method in order to study micro-creativity (Chiu, 2008). Chiu argues that when students are faced with a problem for which they do not know the solution, they try to create new ideas and evaluate these ideas through explanations and justifications. If these ideas are also correct, Chiu calls this process micro-creativity. While new ideas are needed to solve the problem, justifications support or refute the usefulness of an idea by linking it with data. Based on the literature on metacognitive and social processes, Chiu wondered whether certain utterances (e.g. questions, disagreements, correct evaluations, repetitions) resulted in more new ideas, correct ideas and / or justifications. To do this, he uses the speech turn as the unit of analysis (a speech turn corresponds to a # utterance in Figure 6). Chiu (2013) analyzed the corpus furnished by Shirouzu (2013a) and uses the speech turn to define units of interactions, which are sequences of one type of action (within a speaking turn) followed by another. For example, in # 547, the participant Y has a "correct idea" and then participants N, F, K and O respond positively in chorus. The unit of interaction here corresponds to a correct idea plus a question (in speaking turn # 547), plus an agreement (# 548 in turn).

# Utterance	Participant	Speech and actions
547	Y:	I thought that all –all the answers – were ½ of the whole. What do you think?
548	N, F, K, O:	Ok

Figure 6. Translated from Shirouzu's corpus (2013a) and analyzed by Chiu (2013)

There may be interaction units with various combinations of utterances (e.g. a correct evaluation followed by a justification). Unfortunately, I don't have the space to go into Chiu's analysis in detail (see Chiu, 2013), but his method allows him to predict the likelihood that a type of utterance would be followed by another utterance type. For example, after a correct evaluation, one can expect a justification with a probability of 5% rather than another type of utterance.

Chiu also defined what he called a breakpoint - a pivotal moment - that would be a single utterance that defines the border between a collection of utterances of a first type and a collection of utterances of a second type. For example, a breakpoint is a division of the interaction between time periods distinguishing between few new ideas and many new ideas. Another breakpoint corresponds to a division of the interaction between time periods distinguishing between more justifications and fewer justifications. Thus the breakpoint indicates a rise or fall of statements of a certain type (between few new ideas and many new ideas, or between more justifications and

fewer). Chiu needs breakpoints (or pivotal moments) for his method to function and these take place in a speech turn. However, we will see that this constraint is not true for other analytical approaches, which also have the objective of locating pivotal moments, in other words, moments illustrating a change to qualify. Being confronted with this difference forced Chiu to change his thinking.

As stated earlier, Shirouzu provided the corpus Chiu analyzed within the Productive Multivocality project. The corpus was recorded and also analyzed by Shirouzu (2013a, 2013b) during a 6th grade course Shirouzu taught on fractions in Japan. Children had to fold sheets of origami, comparing their solutions in order to solve the problem $\frac{3}{4}$ multiplied by $\frac{2}{3} = ?$ Faced with the analyses of two other researchers: Trausan-Matu (2013) and Shirouzu (2013b), who used descriptive and qualitative approaches, Chiu further developed his own quantitative method (cf. Lund, 2013 for a comparison of the three analytical methods, made by Shirouzu, Chiu and Trausan-Matu).

To understand how Chiu introduced his hybrid approach without succumbing to inconsistencies, we must not only understand the assumptions involved, but we must also distinguish between the data actually taken into account by the two methods, destined to be mixed. In fact, each method examines a subset of data, theoretically seen as more valid than other data that are not considered. If two methods value and use different subsets of data, they can come to different conclusions, which would be a source of incommensurability. It is true that the quantitative method SDA and a qualitative interaction analysis both examine different data, although initially they are both acting on the same human interaction, represented by the same corpus. For example, the SDA method assumes that all statements coded as "justification" can be treated equally. Although we know that all justifications are not equal, we accept this simplifying assumption, necessary for the method to work. Similarly, as noted above, the SDA method requires that the pivotal moment or breakpoint occurs during a single speech turn. In the Productive Multivocality project, we deliberately left the definition of pivotal point flexible so that each researcher could adapt the concept to his or her context while respecting the element of change being present. This "flexible constraint" produced pivotal moments with different characteristics. For example, Shirouzu and Trausan Matu-defined pivotal moments longer than a speech turn. We will see how Shirouzu and Trausan-Matu's differing views on the pivotal moment allowed Chiu to change his vision and adapt his method.

Chiu put the following two ideas together: 1) two methods do not look at the same data and 2) there are useful simplifying assumptions. He noted that there are several possible outcomes to describe what appears to be the same change in human interaction. First, the SDA method statistically identifies a pivotal moment (breakpoint), signaling a change in frequency of correct ideas (by using coded utterances). Second, a qualitative analysis around this pivotal moment helps qualify conceptual change in progress. Table 3 details the logical space of possible outcomes.

Table 3. Results obtained with SDA and with qualitative analyses of the transcription

Four possible ways of comparing pivotal moments, identified by both methods	The conclusion to draw
1. The pivotal moment identified by qualitative analysis of the transcript is the same as the one identified by SDA	➤ The two methods and the two datasets achieve the same result (triangulation)

2. The pivotal moment identified by qualitative analysis of the transcript completely encompasses the moment identified by SDA, but exceeds it	➤ The simplifying assumption of placing a pivotal moment within a speech turn is productive but incomplete
3. The pivotal moment identified by qualitative analysis of the transcript is close, but totally exterior to the pivotal moment identified by SDA	➤ The simplifying assumption of placing a pivotal moment within a speech turn is productive but incorrect (e.g. there is an error that consistently places the pivotal moment at one turn too early)
4. There is no pivotal moment identified by qualitative analysis of the transcript close to the pivotal moment identified SDA (not in the range of 10 turns of speech)	➤ The simplifying assumption of placing a pivotal moment within a speech turn is both non productive and incorrect. We must seek more data to understand why SDA finds a pivotal moment

The corpus analysis on fractions gave rise to possibility #2 in the above table. The pivotal moment identified by SDA was contained with the pivotal moments identified by Shirouzu and Trausan-Matu’s methods of descriptive and qualitative analysis, but these latter pivotal moments encompassed more time. Therefore, Chiu acknowledged the limitations of SDA constraining a pivotal moment to one speech turn, saying it was useful to look at the context of the statement to understand in what ways the moment was pivotal. Chiu then modified (extended) the unit of analysis to include the context of a speech turn. This expansion had two consequences. First, Chiu developed his understanding of why his breakpoints were pivotal moments (with the help of qualitative analysis). Second, he changed his method by incorporating a qualitative analysis into his quantitative approach. Chiu saw how a detailed description of the interaction around the pivotal moment could reveal mechanisms that altered the interaction. These mechanisms could be sought out in other corpora to determine their strength and they could then be specified through operationalized variables and be tested statistically. The example shows how Chiu developed a mixed method that helped him to better understand the phenomena he studied, through the deployment of a conscious methodological eclecticism and in a coherent way, thus escaping the consequences of the incompatibility thesis.

6.6 Conclusions and implications for interdisciplinarity and research-action

In this article, I showed that although methods have biases, researchers are not deterministically bound to the theoretical assumptions underlying the traditions that founded these methods. The methods are based on data, on analytic representations, and on the manipulation of such representations in order to derive new ones. Methods are used as tools to that effect. But these methods are also used by researchers *in practice* and it is these practices that enable the researcher to act, to select the questions that need to be asked, to choose the situations that are worth studying, to knowingly link situations to particular representations of data and finally to interpret analytical representations.

At the beginning of this article, it was postulated that theoretical assumptions were immutably rooted in methods. On the contrary, I showed that the methods described had methodological assumptions embedded in them, but that the researcher had agency regarding the theoretical assumptions associated with the methods and could choose those that were consistent with her worldview. Successfully building a mixed method, without succumbing to the incompatibility thesis is not easy. One must first understand the assumptions that a method intrinsically brings

with it and also understand what epistemological and theoretical commitments the practices of this method may imply. Such a reflexion will be deeper if done collectively, not individually, such as in a project like Productive Multivocality. Indeed, there is a difference between the standard mixed methods approach and multivocal analysis. If they are developed in a coherent manner, mixed methods will benefit research, but multivocal analysis also benefits the academic community, forging new connections in terms of relationships between researchers, publications and their respective traditions, an important issue for the community of Learning Sciences and for any multidisciplinary community.

6.6.1 Multivocality influences teaching and research directions

There was a second benefit of the Productive Multivocality project concerning the relationship between learning and teaching. Shirouzu's corpus was recorded during a class he had taught himself. When he found out where Chiu located his pivotal moments (breakpoints) on the frequency of new ideas, Shirouzu recognized them as markers that could show him whether or not his own intentions as a teacher had been carried out, and this allowed him to evaluate his teaching sequence. He was able to check whether his course plan effectively allowed students to reach the successive levels of conceptual understanding in the ways that he intended.

Shirouzu also benefited from Trausan-Matu's analyses regarding pedagogical design, for two reasons. First, according to Trausan-Matu, the reaction of a first learner to what a second learner has said could be generated by the first learner's internal dialogue. Specifically, Trausan-Matu mobilized the concept of adjacency pair in a completely unorthodox manner. The first element of the adjacency pair is what the first learner said and the second element of the pair is what the second learner would by hypothesis think. The utterance of the second student would allow the analyst to infer the existence of the mediating (yet unobservable) thought of the second learner. If a dissonance results between the first learner's words and the second learner's beliefs (thus provoking what the second learner uttered), then conceptual change could be triggered. Although he was not necessarily ready to accept that "thought" could be part of an adjacency pair, Shirouzu changed his vision of conceptual change, characterized until then more by convergence than caused by divergence.

Second, while Shirouzu based his interpretations on what was observable in the video (e.g. such and such a student focuses on such and such fractions phenomenon) Trausan Matu-interpreted discussions between students by assigning personal characteristics to them (e.g. student "X" is a divergent thinker and therefore differs in his views in relation to other students). Although Shirouzu did not accept such an approach for a corpus taken from a single class session, he still built a direction of future research where he intended to analyze how the change of personnel foci through several class sessions could participate in the formation of personal attributes, such as "divergent thinker."

If we sum up this last reflection on the relationship between learning and teaching, we see that the researchers who compared analyses on a shared corpus and who were interested in teaching and learning, were able to do three things. First, they assessed how learning actually took place compared to what had been expected. Second, they refined analytical concepts, such as conceptual change. Third, they questioned the scientific method used to measure individual participation in the group. Finally, as noted by Law & Laferrière (2013), multivocal analytical methods can be used to support the training of teachers' reflection both on the impact of different designs on the educational process and on the facilitation of collaborative learning outcomes.

In the next section, I review two principal types of scientific explanations that have been given for learning in different disciplinary contexts — mechanistic and functional explanations — and second, I consider the extent to which these two types of explanations can co-exist from a philosophy of science perspective. Third, I review three different ways in which interdisciplinary work has proposed explanations that cross between levels of analysis. Finally, given that I position myself in favor of pluralistic explanation, I present its principal danger, one that researchers particularly risk in interdisciplinary contexts. This danger can be viewed as another consequence of naïve methodological eclecticism. Examining the extent to which different explanatory frameworks can be combined is crucial for any interdisciplinary work. Like the two previous ones, this next section sets the stage for §9 A MULTI-theoretical and interdisciplinary model of GRoup And Individual – “Multi-grain” knowledge building.

7 Explanations that compete across levels of analysis

“The human condition is the most important frontier of the natural sciences. Conversely, the material world exposed by the natural sciences is the most important frontier of the social sciences and humanities. The consilience argument can be distilled as follows: The two frontiers are the same” (Wilson, 1998, p. 267).

Recall the biology example from the beginning of §4 *Integrating Across Disciplinary Boundaries: Interests & Dangers*. It related two types of explanatory schema for the phenomenon of bird migration. The first explanation was mechanistic in nature, relating climatic or daylight changes to modifications in the bird’s physiology. The second explanation was functional in nature, making survival and reproduction capacities a function of bird migration to warmer climates where more food is available. These explanations are not about collaborative knowledge construction, but do involve different levels of explanation. When taken together, they are not contradictory in terms of their underlying assumptions and both are interesting when seeking to understand the phenomenon of bird migration more fully.

Recall also the discussion from §5.1. *Sociology helps language sciences and psychology view the individual and group*. The action theorists and the interactionists focus on a different range of phenomena for the same world. The former maintain that structural social reference points exist even if emergent phenomena arise from what individuals do in interaction. The latter prefer to study micro social phenomena because in their view it is at this level that one gains the most understanding of events in the social world, given that participants co-construct their experience in different ways, as well as the meaning they give it, depending on the contextual elements they have at their disposal. But here again, Turner showed that these views were not incompatible and in this section, I will discuss Levinson’s (2005) work where he specifically works toward connecting the social and interactional levels.

Couched in a discussion concerning the epistemological foundations of psychology, Section §5.1 also pinpointed the tension between understanding the individual as an abstraction and understanding him or her as a set of different situations in which the determinative factors are the most distinct. Although it not framed in these terms, I argue that the distinction made here is also one between mechanistic and functional explanations. In the Aristotelian view, the essence or essential nature of an object determined its behavior, both positively and negatively. So there is an idea that behavior is explained by reducing the object to its essence. Indeed, what determines an object’s movement is completely due to the object’s characteristics. In the Galilean view, context matters and its characteristics coupled with the individual’s characteristics determine the individual’s dynamics. This is akin to saying that the individual has a function within a context. This is also a debate between levels of analysis. On the one hand, the individual alone is the focus; on the other, it is the individual within the situation.

The presumption that physical phenomena are fundamentally determinate seems to have defined modern behavioral science (Glimcher, 2005) and according to Sperber (1997), the core of cognitive science is constituted by its naturalist and mechanistic program geared to explain mental phenomena. This means that mechanistic explanations are at an advantage from the perspective of how science in general is set up, as well as how particular disciplines are oriented.

For Sperber, an explanation is mechanistic when it analyzes a complex process as an articulation of more elementary processes and it is naturalist if there is good reason to think that

these more elementary processes could be in turn analyzed in a mechanistic manner up until the point at which the level of description of their character would be evident (e.g. neuronal processes). On the other hand, many researchers in human and social science disciplines contest that elementary, cognitive mechanisms furnish a causal explanation of social behavior (Ogien, 2011). These researchers propose a cognitive model of processing in the relevant cognitive domain and performing a functional decomposition of the domain in question allows for the generation of such models (Coltheart, 2012). This disagreement over whether mechanistic or functionalist explanations have more explanatory value is what I next will unpack in the first subsection.

7.1 Mechanistic explanations

In psychology, where one of the goals is to explain human behavior, some of the literature shows that particular sub-disciplines exhibit a preference for different explanation types. Researchers in neuropsychology are often partial to reductionist, mechanistic types of explanation where a complex phenomenon can be reduced into simpler components and this is due to their assumption that the human brain functions in a similar way to a computer:

“...Mechanism schemata explain not by fitting a phenomenon into a web of inferential relationships but by characterizing the mechanism by which the phenomenon is produced or realized.” (Craver, 2008, p. 71).

For example, cognition should be understood by reducing it into the underlying biological mechanisms of the brain (Reilly & Munakata, 2000)³⁰. More specifically, a mechanistic explanation for understanding people’s ability to read would consist of “being able to explain the neural mechanisms the operation of which explain the exercise of the power to read (Bennet & Hacker, 2006, p. 35).” Ilari (2013) puts it this way: “...finding mechanistic explanations involves finding and describing the phenomenon, and finding and describing the entities and activities, and their organization, by which the phenomenon is produced (p. 3).”

But does showing the parts of which some thing is constituted (ontological reduction) or stating that a complex system is best investigated at the lowest possible level (methodological reduction) or embracing the idea that knowledge about one scientific domain (typically about higher level processes) can be reduced to another body of scientific knowledge (typically concerning a lower and more fundamental level) — epistemic reduction — give explanations for what that thing is (Brigandt & Love, 2014)? The reductionists who favor mechanistic explanation say “yes”:

“The term ‘reduction’ as used in philosophy expresses the idea that if an entity x reduces to an entity y then y is in a sense prior to x, is more basic than x, is such that x fully depends upon it or is constituted by it. Saying that x reduces to y typically implies that x is nothing more than y or nothing over and above y” (van Riel & van Gulick, 2014).

³⁰ This is a highly controversial statement and Carandini (2012) evaluates the extent to which neuroscience has revealed how neural circuits lead to human behavior: “The general public might think that this goal has already been achieved; when they read that a behavior is associated with some part of the brain, they take that statement as an explanation. But most neuroscientists would agree that, with a few notable exceptions, the relationship between neural circuits and behavior has yet to be established“ (Carandini, 2012, p. 507).

In this view, being able to describe the mechanism itself is what does the explaining. In mechanistic explanation, the reduction to the most basic parts gives the most explanatory value, as there can be no more reduction³¹. However, according to Brigandt & Love (2014), who write about biology, reductionism versus anti-reductionism has created a false dichotomy between two extreme positions. On the one hand, a reductionist stance argues that molecular biology can in principle fully explain all biological facts and in that case, higher-level biology theories are no longer useful. On the other hand, an anti-reductionist stance holds that higher-level biological fields of study possess explanatory principles of their own and do not need molecular biology in order to produce explanations that have value to the larger field of biology. I will argue that these approaches are complementary.

This same reductionist approach is found when neuroscientists explain learning. For example, Burke, Tobler, Baddeley, & Schultz (2010) have studied observational learning, where individuals learn through the observed actions and outcomes of others. Learning increased when participants were able to see what a confederate chose to do during a task that involved rewards and punishment. Participants' learning increased even more when they were able to see the consequences of the confederate's actions. They showed that brain activity in the two prefrontal cortex regions corresponded to observational learning signals. But even a reductionist explanation needs a higher order interpretation. The reason why the activation of these brain regions is potentially interesting is that the same regions have been shown to be involved in a mirror system that allows us to understand the intentions of others. Understanding the intentions of others allows an individual to extract more reward from the doing a task than would normally be possible relying only on his or her own individual learning. It is the possibility of observing the mistakes or success of others that makes the difference. This is a functionalist explanation, and although it is not holistic as it remains on the level of the individual, it still explains why a particular brain region being activated could be interesting for the studied task. That said, there is debate amongst neuroscientists as whether or not brain plasticity is so high that regional definition of brain activity according to task type is not very informative (Rakic, 2002).

In what follows I present a detailed example of a functionalist who defends his type of explanation against reductionist mechanistic attackers and who argues that there is room for both types of explanation.

7.2 Functionalist explanations

I have given an idea of what mechanistic explanation is and how it can be reductive, but how are the higher-level explanations characterized, to which mechanistic explanations may stand in opposition? These higher-level explanations are called functional explanations. The following

³¹ If this view can be likened to a version of Occam's razor (i.e. as a kind of preference for simplicity), it is not clear what exactly should be simple. Baker (2013) distinguishes between two fundamentally distinct senses of simplicity: syntactic simplicity (i.e. the number and complexity of hypotheses), and ontological simplicity (i.e. the number and complexity of things postulated). Recall that a hypothesis is a supposition to be confirmed or infirmed whereas a postulate is a hypothesis used without any intention to verify it (e.g. used for sentimental, political, or religious reasons) (<https://fr.wikipedia.org/wiki/Hypothèse>). In the case of mechanistic reductionism, it is possible to further reduce ontologically to more "simple" chemical processes that make up neuronal activities. In addition, perhaps the number and complexity of hypotheses could be reduced at the functionalist level (as opposed to the mechanistic level) – see next section.

definition is written from the viewpoint of studying the mind using cognitive psychology and artificial intelligence:

“Functionalism is the doctrine that what makes something a thought, desire, pain (or any other type of mental state) depends not on its internal constitution, but solely on its function, or the role it plays, in the cognitive system of which it is a part. More precisely, functionalist theories take the identity of a mental state to be determined by its causal relations to sensory stimulations, other mental states, and behavior” (Levin, 2013, on-line).

Let’s take an example from Coltheart (2012) that concerns reading and that will illustrate how an explanation can be satisfactory at a functional and causal level without specifying any of the underlying mechanisms.

Arguing from the domain of language positioned as a subdomain of cognition, Coltheart takes issue with Bennett and Hacker (2006), who claim that attempts at cognitive-level explanations of behavioral phenomena are only mere re-descriptions of these phenomena, and are therefore never of any explanatory value. Bennett and Hacker assert that terms typically used in cognitive-level explanations of language are empty, that there is no such thing as a ‘mental lexicon’, ‘retrieval’ or ‘lexical access’ and that postulating such entities does not explain anything at all — it merely re-describes the interlinked abilities in a different way. As a case in point, these authors criticize Levelt’s (1999) way of describing his model of fluent speech where he talks of the ‘process’ of accessing and selecting words when we speak. For these neuroscientists, there is no process of retrieval of words:

“The only *processes* [authors’ emphasis] are neural, and they are not processes by which we do anything, but processes that enable us to do things we do. Furthermore, we do not *access* [authors’ emphasis] words. We use words that we have learnt. That is not to access anything. When I walk home, I know the route I use, but that does not imply that I have access to a map” (Bennett and Hacker, 2006, p. 36).

“Levelt’s suggestion that words are ‘processed’ at 1.6–2.6 years is problematic. We can explain what it means for our machines – word-processors – to process words. But until an acceptable explanation is offered, we do not know what it means for the brain or its parts to process words” (Bennett and Hacker, 2006, p. 38).

The heart of the argument between these two researcher camps consists of deciding what phenomena have explanatory power for their purported theories. Unsurprisingly, Bennett and Hacker do not contest that there are neural changes that correspond to the acquisition of the abilities to pronounce a word and to recognize a spoken word (they just don’t yet know what they are), but “talk of phonological and phonetic encodings adds nothing to that truism” (Bennett and Hacker, 2006, p. 38). It is interesting to note that this argument between cognitive and neural levels of explanation somewhat mimics the argument the behaviorist Watson (1913) made against functional psychology insofar as it tried to distinguish itself from structuralist psychology. Essentially, Watson remarked that a change in terminology in and of itself does not obtain extra explanatory value:

“The terms sensation, perception, affection, emotion, volition are used as much by the functionalist as by the structuralist. The addition of the word ‘process’ (‘mental act as a whole,’ and like terms are frequently met) after each serves in some way to

remove the corpse of 'content' and to leave 'function' in its stead. Surely if these concepts are elusive when looked at from a content standpoint, they are still more deceptive when viewed from the angle of function, and especially so when function is obtained by the introspection method. It is rather interesting that no functional psychologist has carefully distinguished between 'perception' (and this is true of the other psychological terms as well) as employed by the systematist, and 'perceptual process' as used in functional psychology. It seems illogical and hardly fair to criticize the psychology which the systematist gives us, and then to utilize his terms without carefully showing the changes in meaning which are to be attached to them” (Watson, 1913, p. 165).

Bennett and Hacker as well as Watson accuse cognitive-level explanations of just re-describing interlinked abilities in a different way but in 1913 Watson proposed behavioral level explanations as an alternative while almost one hundred years later in 2006, Bennett and Hacker propose neural-level explanations. Both are mechanistic type explanations.

In contrast to this, Coltheart (2012) clearly shows how cognitive level functionalist explanations can be satisfactory:

“Example 1. Your task is to read single pronounceable non-words aloud as rapidly as you can. The non-words always have five letters. Half of these have five phonemes³² too, because with these non-words, each letter maps onto a separate phoneme (e.g., FREPS). The other half has three phonemes, because these non-words (e.g., FEECH) possess phonemes which are represented as pairs of letters.

What difference in reading times will be seen between these two types of non-words?
Answer: The non-words with fewer phonemes produce slower reading-aloud responses than the non-words with more phonemes: this is the ‘whammy effect’ (Rastle & Coltheart, 1998).

How might this difference be explained?

Answer: The cognitive procedure we use for reading aloud non-words or unfamiliar words works by translating, serially and left-to-right, letters into phonemes. When it has reached the second letter in FEECH, it is translating the string FE, and so is generating the phoneme /e/ (as in ‘fed’) as the second phoneme for the response. But when this procedure gets to the third letter and is translating the string FEE, it generates the phoneme /i:/ as the second phoneme in response to the letters EE. So now, there are two competing candidates for the second phoneme position, /e/ and /i:/. There is mutual inhibition between phonemes competing for the same position, and the inhibition exerted by the wrong phoneme /e/ on the correct phoneme /i:/ slows the rate at which activation of the correct phoneme rises: The correct phoneme suffers from a whammy effect. Then, when the fourth letter is reached, the string being translated is FEEC, and so the phoneme /k/ is generated as the third phoneme.

³² A phoneme is the smallest unit of speech that can be used to make one word different from another word (Merriam Webster).

This will exert inhibition on the correct phoneme /tʃ/ that begins to be activated when the fifth letter of the non-word is reached. Hence, two of the three phonemes of FEECH will be affected by competition and hence inhibition: there will be a double whammy. In comparison, none of the phonemes of FREPS will be whammied. That is why non-words like FREPS are read more quickly than non-words like FEECH. We know that this cognitive-level explanation works because the whammy effect is present in the responding of a computational model of reading, the DRC model (Coltheart, Rastle, Perry, Langdon, & Ziegler, 2001)” (Coltheart, 2012, p. 15-16).

As Coltheart points out, there is currently no explanation of these experimental findings at the neural level (e.g. reductive and mechanistic) whereas the above interpretation gives a functional and causal explanation at the cognitive level for reading behavior. He also argues that these cognitive level explanations are non-vacuous in that they are falsifiable by experiment. There are two competing hypotheses that could explain the cognitive procedure we presumably use for reading aloud non-words or unfamiliar words; this procedure is that we translate letters into phonemes, serially and from left to right. If this is the case, what is the mechanism that controls this left-to-right process? If the leftmost N letter of the input are currently being translated to sound, then at some later point in time, the system must move on to the next letter so that now the leftmost (N+1) letters are being translated to sound.

So, what are the two competing hypotheses for explaining how we move from one letter to the next? The first is a hypothesis centered on time. Hypothesis T (for Time) states that after the leftmost N letters of the input have been processed for a fixed time T, the system moves on to include the next letter, i.e., begins to process the leftmost (N + 1) letters of the input. The second is a hypothesis centered on phoneme activation. Hypothesis A (for Activation) states that after the rightmost letter of the N letters currently being processed has activated a phoneme to some critical activation level A, the system moves on to include the next letter, i.e., begins to process the leftmost (N + 1) letters of the input. In the first hypothesis, the change to the next letter is triggered by time spent on the previous letter whereas in the second hypothesis, the change to the next letter is triggered by a recognition and activation of the smallest part of the word that makes it different from another word (i.e. phoneme).

What is important to note is that if we want to refine the above cognitive-level functional and causal explanation by adopting one or the other of these two hypotheses, then the only way to do that is to obtain evidence by performing a behavioral experiment (see Coltheart, 2012 for details). Nothing we currently know about the brain can help us to decide between the two hypotheses. And indeed, if ever a new cognitive neuroimaging study was able to localize these information-processing activities in specific regions of the brain, in Coltheart’s opinion, “they would not be offering a new explanation of anything; they would just be fleshing out an existing explanation” (p. 17). This can be taken as an argument for combining both mechanistic and functional explanations as aspects of a more complete explanation. In other words, a cognitive-level functional explanation gives us the reasons we would move from one letter to the next while reading whereas a mechanistic level explanation tells us the neuronal processes that are going on when we move from one letter to the next. The first distinguishes between two different possibilities of *why* we change letters and the second tells us what is happening physically in our brains when we change letters so that we *can* read.

Despite this making sense on the level of the disciplines involved, the philosophy of science does not refrain from giving an opinion on whether or not these two explanations can exist on metaphysical grounds.

7.3 The causal exclusion principle

If the researchers involved can agree that an explanation type that is different from their own can also have value (as long as they do not hold irreconcilable assumptions about the way the world works), is it then possible for both a mechanistic and a functional explanation to exist for the same *explanandum* (phenomenon to explain)? And if it is possible, what is the relation between these two explanations? Can both give explanatory value to the same phenomenon and exist in a compatible fashion? I will first examine this from a philosophy of science point of view and then from the point of view of what both may contribute to explaining the role of the individual within the group during learning.

There is a metaphysical argument that argues against both mechanistic and functional explanations existing together, for example, in the case of the cognitive psychologist versus and neuroscientist, just previously described. The argument is called the causal exclusion principle (Kim, 1998; Looren de Jong, 2003). It states that a single event cannot have both a physical and a mental cause. If both causes were contributing to the effect, then that implies *causal overdetermination* in that we cannot distinguish between the causes and thus determine which one is doing the work.

Kim uses pain and the firing of nerve cells to illustrate this. If stabbing causes the firing of nerve cells, and the firing of nerve cells is what makes us experience pain, then it is not the mental process of feeling pain that causes us to actually feel the pain. The mental process is thus called *epiphenomenal*, meaning that it occurs at the same time as the physical, but that it just accompanies the physical, without embodying any causal action. Following this logic, Coltheart's functional and causal explanation at the cognitive level for reading behavior (i.e. cognitive processes regarding how we change focus from one letter to the next and why a phoneme is activated when it is) would be epiphenomenal, whereas the real causal work would be done by the mechanistic explanation, given by the neuroscientists: neural processes are the only processes that exist and they *enable* us to read.

However, others argue that causal accounts given at different levels of description *can* co-exist. According to York (2013), who writes in the field of Cognitive Science:

“A holistic or non-mechanistic account given at one level of description can coexist with—and complement—a mechanistic account given at another (lower) level” (p. 2).

This involves reconciling between what York calls the personal and the sub-personal levels, defined differently than the personal and sub-personal levels of Wiltshire, et al. (2015). For York, the relevant entities in the development of learning to appreciate a new musical genre (e.g. experimental trip hop) is not just at the sub-personal level of inner mental processes, but at the personal level where entities include cultural artifacts (e.g., recordings, books about music, etc.), institutions (record stores, concert halls), and other people with whom we might listen to the new musical genre. So York is claiming that mechanistic explanations have their limits in terms of explanatory value. He argues that if the “system” under consideration includes autonomous agents (i.e. human beings as opposed to processes that occur inside human beings), then purely mechanistic accounts will not give the explanatory value that would contribute to such an

understanding. He quotes (p. 3) Bechtel and Abrahamsen (2007) who argue the following: “Mechanisms, insofar as they involve purely causal processes, are fully determined in their responses and so lack the requisites for [personal] agency” (p. 96). Understanding the development of musical aesthetic sensibility then, requires coming up with the kind of explanation that can take into account interactions with other people that show “agency” which Jeannerod (2003) defines as the “way by which the self builds as an entity independent from the external world” (op. cit., p. 1) or more largely as initiating, executing, and controlling one's own volitional actions in the world. Here we find a parallel with the researcher who uses agency in the way he or she adapts a method in order to avoid entrapment within undesirable assumptions. In this case, the person's newly acquired music appreciation did not arise from pre-determined causal processes, but through his or her agency in the process of listening to the music.

That said, it's one thing to show that mechanistic explanations do not take into account all of the phenomena needed to explain human behavior, but this is not sufficient to argue against the causal exclusion principle. If there are both mechanistic and functional causes for a given phenomenon, there is still a problem in that we can't know which one is deciding the outcome. In other words, the outcome is *overdetermined* (Robb & Heil, 2014). There are a certain number of responses to the causal exclusion principle and this branch of the philosophy of science is currently very active. In what follows, I give a brief summary of the debate. In the first part, I argue from the philosophy of science that there are configurations in which reductionism cannot exist. Accepting this opens the way to functionalism and it allows me to introduce the concepts that are used in responding to the causal exclusion principle. In the second part, I argue how both types of explanation can co-exist by giving two of the typical responses to the causal exclusion principle, again from the philosophy of science. In the third part, I review how a philosopher, researchers in sociology and in linguistic anthropology, and a physicist propose different ways of bridging across levels of analysis: by explaining different aspects of a phenomenon through multiple disciplinary perspectives, through the definition of intermediate variables, and through the definition of properties and laws. These ways of bridging will be taken up in my own proposal in §9 *A MULTI-theoretical and interdisciplinary model of GRoup And Individual – “Multi-grain” knowledge building*.

7.3.1 Arguments against reductionism

There are three philosophical concepts that, when taken together, argue that a law or a concept from psychology cannot be reduced to a neurobiological law or concept, even if, as Sawyer (2001) puts it, there is nothing in the universe except physical matter. These three concepts are 1) supervenience, 2) multiple realizability otherwise known as multiple realization, and 3) wild disjunction.

Supervenience states that if two events are identical with respect to their descriptions at the lower level, then they cannot differ at the higher level. Supervenience has two consequences. The first is that if a collection of lower-level components with a given set of relations causes a higher-level property to emerge at a given time, then whenever this collection with the same set of relations occurs again, then the higher-level property will again emerge. The second consequence is that if an entity changes at the higher level, it must also change at the lower levels. Supervenience alone is still consistent with reducing elements at a higher level to a lower level. But if one adds multiple realizability and wild disjunction, then reductionism is no longer possible.

Multiple realizability states that even if each mental state must be supervenient on some physical state, each token instance of that mental state can perhaps be implemented by a different physical state. This is the fundamental thesis of functionalism — states of mind are multiply realizable (Robb & Heil, 2014). For example, a human and an octopus can both be hungry, but that hunger may be implemented with different neural circuitry. However, if there were only a few different realizing states or if those states displayed some common features (similar neural circuitry between octopi and humans), then reductionism could still be possible. It only becomes impossible with the addition of wildly disjunctive lower level phenomena.

Wildly disjunctive realizations were coined by Fodor (Sawyer, 2001). I have drawn Figure 7 in order to illustrate the concept in two cases, adding the case of psychology and neuroscience. Sawyer drew a similar figure involving just sociology and psychology, but his argument also touched upon psychology in terms of neurobiology. For example, if a neurobiological equivalent of a psychological term (cited within a law) was an otherwise unrelated combination of many neurobiological concepts and terms (neural phenomenon 1 or 2 or 3) or if a psychological equivalent of a social term (cited within a law) was an otherwise unrelated combination of many psychological concepts and terms (psychological phenomenon 4 or 5), it is difficult to see how one could coherently reduce in both cases to the lower-level (cf. Figure 7). A social law involving two social phenomena could be comprised on the lower level of disjunctive properties of psychological phenomena. And a psychological law involving two psychological phenomena could be comprised on the lower level to disjunctive properties of neural phenomena. In this case, reductionism is not possible.

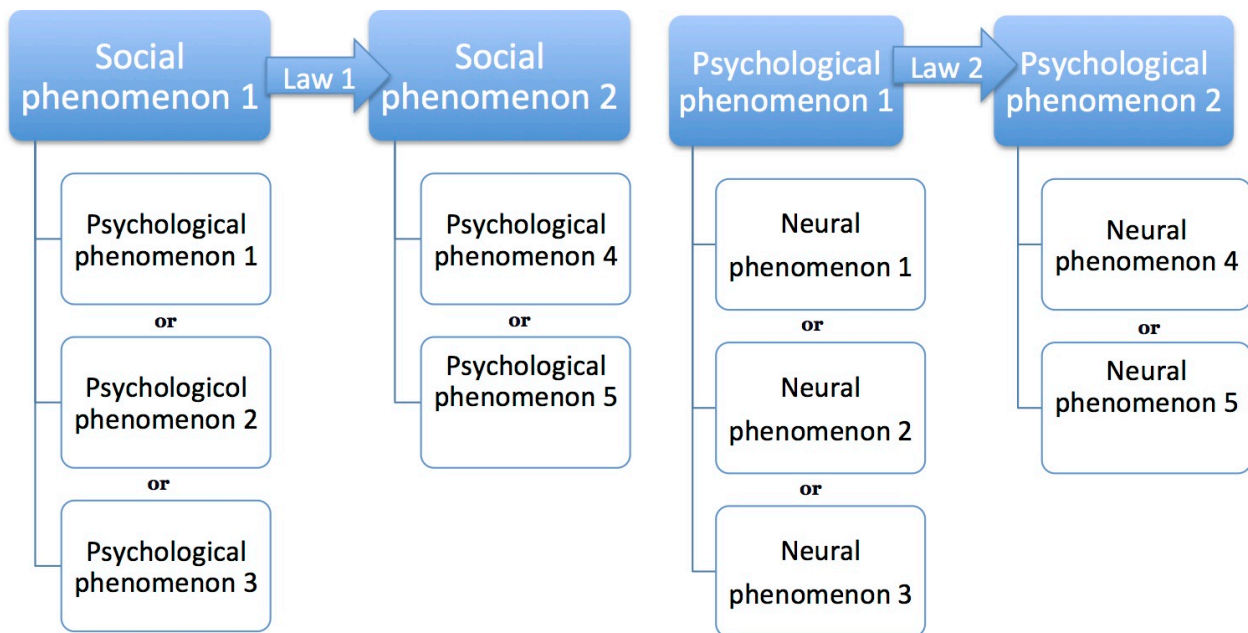


Figure 7. Illustration of wild disjunction instantiated with two types of attempted reduction from higher-level laws.

According to Sawyer, Fodor argued that a true scientific law could not have wildly disjunctive components. This is because wild disjunction implies that there could be lawful relations among events on a higher level of description, but that there are cases where the events at a lower level do not have lawful relations amongst themselves. If the lower level events are

wildly disjunctive, they cannot predict future events (as laws must) as they only apply to a specific token instance, even if the higher-level law is more generally applicable.

The co-existence of all these concepts (supervenience, multiple realizability, and wildly disjunctive realizations) for a particular phenomenon of study therefore argues why certain social properties and laws and certain psychological properties and laws cannot be reducible. Sawyer (2001) gives the example of the collective entity that has the social property of “being a church”. This entity has a collection of individual properties associated with each of its component members. For example each individual may hold properties about beliefs or intentions where the sum total of these beliefs and intentions can be said to make up the social property of “being a church”. Yet, “being a church” can be made up of many different individual properties. Interestingly for this HDR, Sawyer remarks that microsocial properties are no less multiply realizable (e.g. “being an argument”, “being a conversation”). Another way of saying this is that microsocial properties are *indeterminate*, that which cannot be determined, for which the outcome is uncertain (Bryson, Lowe, & Stein, 2001) or that which is indistinguishable between two options such as an electron being both a wave and a particle or a picture being either a duck or a rabbit, depending on perceptual focus (Gershenson, 2007).

If a system exhibits indeterminacy, it also being reductionist seems impossible, but is this only a seeming indeterminacy and therefore just a problem of measurement or is it a real indeterminacy (Glimcher, 2005)? One of the recent frameworks for studying indeterminacy is the science of complexity, and some definitions of complexity oppose complexity to reductionism:

“What is complexity? Let us go back to the Latin root *complexus*, which means “entwined” or “embraced”. This can be interpreted in the following way: in order to have a complex you need: 1) two or more distinct parts, 2) that are joined in such a way that it is difficult to separate them. Here we find the basic duality between parts which are at the same time *distinct* and *connected*. Therefore, the analytical method alone won’t allow us to understand a complex, as by taking apart the components it will destroy their connections” (Gershenson & Heylighen, 2005, p. 48).

In such a view, the system components are mutually entangled so that changing one component will set off a propagation of other changes in other components, potentially including the component that began the process (Gershenson & Heylighen, 2005), exhibiting a kind of *feedback* loop. Causal behavior is difficult to track in such systems, but these kinds of systems are more the rule than the exception, as these authors argue, giving examples such as a living cell, a society, an economy, an ecosystem, the Internet, the weather, a brain, and a city. All of these systems consist of numerous elements whose interactions produce a global behavior that cannot be reduced to the behavior of the separate component. In this HDR, I argue that given the outcome of §5 *A cross-disciplinary analysis of the individual versus the group in learning contexts*, the fact that models are becoming more complex requires us to develop new methods to take this complexity into account. I will address the relationship of indeterminacy (in the form of *emergence*) to reductionism in a §7.4.3 *Bridging through the definition of properties and laws*, but for now, I give arguments on how the causal exclusion principle does not hold in order to argue for researchers to be able to provide both functional and reductionist explanations for the phenomena that interest them.

7.3.2 Arguments against the causal exclusion principle

ONE VERSION OF THE AUTONOMY SOLUTION

Recall that the exclusion principle presents a mental, functional property and its physical realizer as competing to be causally relevant to the same behavior; both cannot be the cause. But the mental property may not be threatened with exclusion if the physical realizer is causally relevant to a different property of the effect. This version of the autonomy solution means that the mental and physical causes are autonomous of each other — they cannot compete if each causes a different property of the effect (Robb & Heil, 2014).

INHERITANCE SOLUTIONS

Whereas autonomy solutions separate mental properties from their physical realizers, inheritance solutions propose that mental properties are so closely related to their realizers that the former inherit the causality of the latter. It is not a question of the physical excluding the mental as they work together. There are different ways that this inheritance is expressed, each of which has their critics (Robb & Heil, 2014). Take the example of the property of being water giving the power to dissolve salt or the property of being sharp giving the power to cut. If we take a given mental property M and one of its physical realizers P, M's power's could be included in P's. Both properties cause a certain kind of behavior, but P just has a greater specificity. The example given by Robb & Heil is a person stepping on another's foot. The stepping on causes the pain, but we say the person was responsible. Linking this back to properties, if M's causal powers are included in those that are given by P, then P's causing of the behavior includes M (Robb & Heil, 2014).

7.3.3 Conclusions and implications for the co-existence of competing explanations

In this section, I introduced the causal exclusion principle as a way to argue against having both functionalist and mechanistic explanations for the same phenomenon. This principle states that a single phenomenon cannot have both a physical (mechanistic) and a mental (functional) cause in that the effect would then be over-determined which means we would not know which cause was doing the actual work. Before furnishing two common responses to this principle, I needed to introduce the concepts that these responses mobilized and so I presented the situation in which reductionist explanations cannot hold, but where functional explanations are possible. I was then in a position to give two of the common responses to the causal exclusion principle: autonomy solutions and inheritance solutions. If I have been convincing, then both mechanistic and functional explanations will be allowed to co-exist for aspects of the same phenomenon. In the next section, it will become clear how distinguishing between aspects of the phenomenon in question as being part of different explanations is crucial.

7.4 Bridging between levels of analysis

I argued in the last section that autonomy and inheritance solutions gave reasons to doubt the causal exclusion principle, so if the reader has been convinced, it could be possible for researchers to combine mechanistic and functional explanations for aspects of the same phenomena. We can now turn to the arguments given by four researchers, one in philosophy (Thagard, 1994), one in linguistic anthropology (Levinson, 2005), one who reviews conceptions of emergentism in sociology (Sawyer, 2001) coupled with researchers in complexity theory (Bechtel & Richardson (2010), and one in physics (Anderson, 1972). They all suggest ways of bridging between levels of analysis. Thagard argues that cognitive processes help understand

social structures, the reverse also being true. Levinson suggests developing variables that mediate between levels of social structures and the micro processes of human interaction. Sawyer along with Bechetal and Richardson give a view of emergentism that is compatible with reductionism, in that one level of analysis can explain another through linking laws and properties. Finally, Anderson argues that lower-level laws *govern* higher-level sciences, but this does not mean that other laws *not* linked to lower levels cannot emerge at higher levels.

7.4.1 Bridging by explaining different aspects of a phenomenon

Thagard (1994) considers cognitive and social explanation schemas of different aspects of a phenomenon as complementary and shows how they can be brought together in order to form an integrated explanation schema. He therefore does *not* subscribe to the inseparability hypothesis, nor then to a process ontology. In the context of explaining how philosophers, historians, psychologists, and sociologists of science accomplish the doing of science, Thagard (1994) argues that researchers in these disciplines all produce explanations of different natures. Philosophers of science offer logical explanations in which new scientific knowledge is derived inductively, deductively, or abductively from previous knowledge. Cognitive scientists as well as psychologists, computer scientists, and some philosophers propose cognitive explanations in which knowledge change is derived from scientists' mental structures and from the procedures they carry out. Finally, sociologists of science propose social explanations in which scientific change is explained by the social interests of scientists and by the ways in which academic institutions are organized. Thagard favors the cognitive schema as the one to be integrated with the social over the logical in that the cognitive schema is more general since the representations and procedures that it invokes need not be those found in formal logic.

Thagard is a philosopher of science and so his focus is on describing the relationship of social and psychological explanations of science. He presents four different models for this (cf. Figure 8; the arrow signifies "explains" (reproduced from Thagard, 1991, p. 642)).

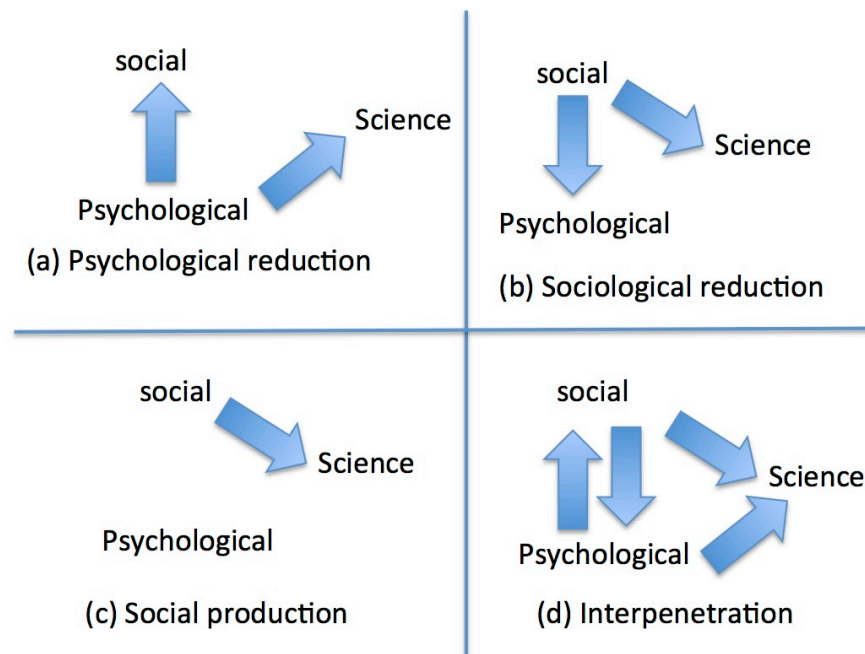


Figure 8. Four models of the relation of social and psychological explanations of science.

He criticizes the first three:

- (a) Psychological reduction (top left) only admits psychological explanations of science, even its social aspects;
- (b) Sociological reduction (top right), is the view that science and the psychological have purely social explanations;
- (c) Social production (bottom left) simply ignores the psychological while giving only social explanations of science;
- (d) In interpenetration (bottom right), the development of science is explained both socially and psychologically, with the social and the psychological interpenetrating rather than reducing one to the other. Thagard clearly argues for this complementary position:

“Sometimes we need the social to help explain the psychological, as when the development of Lavoisier's beliefs³³ is explained in part by his circle of friends. And sometimes we need the psychological to explain the social, as when cognitive processes of analogy thinking are used to help understand how social structures can suggest scientific theories” (Thagard, 1991, p. 642).

This reflection is at the heart of what interdisciplinarity can bring us as researchers. In the next section, this thinking is continued in more detail in that the disciplines that furnish explanations are linked together through intermediate variables. The disciplines in which these intermediate variables are being mobilized and the analytical goals for which they are manipulated have the consequence that different aspects of a given intermediate variable are emphasized.

7.4.2 Bridging through intermediate variables

Levinson's is also an argument for keeping levels separated, yet linking between them. He suggests studying subsystems separately with the view of building up a theory about a complex system while having to hypothesize about the relations between separate subsystems. This is very similar to what Greno (1998) suggests in educational psychology where the system includes the context in which an individual's cognition and behavior is studied. In Levinson's words:

“The model suggested is one of three distinct levels of analysis, or three different kinds of systems — sociocultural systems, interaction systems, and language systems — interlocked in various ways. One doesn't have to be a realist about these entities — one can treat them as analytical fictions, whereby one gets a better model of the whole shebang by finding relatively differentiated subsystems which seem to have organizing principles of their own” (Levinson, 2005, p. 449).

And like Thagard, Levinson (2005) also argues against reductionism, but the form he fights against is interactional reductionism — defined by Schegloff (1987) and stating that social order

³³ Thagard gives both a cognitive *and* a social account of the chemical revolution in which Lavoisier's oxygen theory of combustion overthrew the phlogiston theory of Stahl.

is nothing but the local product of interaction. Levinson paraphrases Schegloff's point of view in the following way:

“...instead of thinking of social institutions as organizing and creating the interaction that takes place within them, we should rather think of interaction patterns as engendering the very social institutions themselves. The example he [Schegloff] works through is persuasive: he shows how various US presidents have tinkered with the structure of presidential press conferences, only to find that the new arrangement has a quite unintended life of its own” (Levinson, 2005, p. 432).

That said, Levinson is not wholly convinced by Schegloff's example. Even though changes in the conduct of social and verbal interaction will engender changes in institutions and in the rules of language, he argues that it is not the case that interaction determines every other level of phenomena, including social systems and linguistic systems. In order to explain why this is so, Levinson distinguishes between interactional reductionism and interactional constructivism. The first only refers to interactional principles when it comes to reconstructing systems, while rejecting any talk of social systems or grammatical systems themselves. But like any other field's radical reductionism, this approach throws out other levels of analysis at which principle and order may be better captured. Interestingly, in relation to the debate between functionalist and mechanistic explanations, he takes the example of reducing English grammar to the state of neurons in English speaker's heads. Without those neurons, there would be no English grammar. However, no amount of neuroscience is going to help to extract the rules of English grammar out of brain tissue and as Levinson (2005) jokes, “and anyway, whose head should we dissect?” (p. 452).

On the other hand, interactional constructivism simply holds that interaction constructs social relations, which in turn construct social institutions. It also argues that uses of language can construct new rules of usage and these in turn can construct new rules of grammar. Both types of systems — social systems as well as languages — are influenced by changes in interaction, but even if those systems are the outcome of interactions over the centuries, both social systems and grammar will have a constraining influence on what people can do in interaction and on how they construct meaning.

Levinson would prefer treating the sociocultural system, the linguistic system, and the interaction system as separate, but equal. In his view, no system should be accorded ontological priority and one does not get a better model by claiming reductionism in any direction. Rather, he argues for building models that involve independent systems (linguistic, sociocultural, and interactional) that are linked together in what he calls the Durkheimian manner, that is, by focusing on intermediate variables. The intermediate variables that he has found in his own work include:

- Types of social relationship (these link linguistic systems to social systems);
- Inferential heuristics (these link cultural systems to linguistic systems);
- Cognitive styles (these link interaction systems, cultural systems, and linguistics systems).

He attributes these intermediate variables to “Mind” where cognitive styles are mental parameters, where social relationships are mental templates and where inferential heuristics are mental habits. What happens in the mind then, allows him to link between the three independent systems: cultural, social, and linguistic.

Levinson deploys two versions of these systems — kinship theory and interactional systematics to illustrate how the nature of verbal interaction is tied to culture and to social institutions. Kinship theory predicts the character of kinship joking for his example population — the 4000 inhabitants of Rossel Island, the last island off the eastern tip of Papua New Guinea. A kinship joke will only occur in the right ecological niche. But, it takes interactional systematics to explain why a particular joke occurs at a particular time in the conversation and how it is recognized and received as such. The intermediate variable that played a role here was social relationships. The kinship system of the inhabitants of Rossel Island gives n types of social relationships. Each type carries interactional expectations and constraints, such as a linguistic etiquette or why one should avoid speaking about particular relationships. The social relationship can also be contextualized in the interactional system as how a person is referenced during talk. When the inhabitants of Rossel Island suppress reference by name of particular relatives in certain cases, there are thus two systems that can give explanations for that behavior.

In the next section, I look at bridging between levels of analysis through laws. The previous discussion on bridging between levels of analysis through intermediate variables did not focus on this particularly, but kinship theory can predict properties of interactions, like joking behavior, in-law avoidance and name-taboos. Similarly, general principles governing what happens at the interactional level can have a deep impact on language structure (Levinson, 2005). But such “laws” for the most part, have yet to be proposed.

7.4.3 Bridging through the definition of properties and laws

Sawyer (2001) defines two types of emergence, one of which allows bridging through laws by accepting reductionism, and one that does not. Although philosophical arguments about emergence and reductionism have typically focused on the mind-brain relation, they can be generalized to apply to any hierarchically ordered set of properties (Sawyer, 2001) and can thus be applied to the analysis of the co-construction of knowledge involving individuals, groups and communities.

Sawyer evaluates these two competing theories of emergence, concluding that neither adequately addresses all of the implications of what philosophers agree on and he identifies several unresolved issues that face sociological theories of emergence. Unfortunately, I don’t have the space to consider those as in this section I concentrate on how one can bridge between levels of analysis through the definition of laws.

Methodological individualists accept the existence of emergent social properties, yet they claim that such properties can be reduced to explanations in terms of individuals and their relationships. Such individualist emergentists believe that macrosocial properties and laws can be explained in terms of properties and laws about individuals and their relations. In this view, lower-level properties or laws explain higher-level properties and laws and higher-level entities are composed of the lower-level entities. According to Bechtel & Richardson (2010), the traditional requirement of reductionism is to connect systems by deriving one set of laws at a higher level from another set of laws at a lower level where laws are universally quantified statements. In addition to this, they write, philosophical models of reductionism require bridge laws that connect the terms of the higher-level theory with the terms of the lower-level theory. They mention the controversies over whether Mendelian genetics has actually been reduced to molecular genetics or whether psychology might reduce to neuroscience. Part of the difficulty is

figuring out which statements represent the “laws” at each level and if laws are indeed agreed upon, often they are too specific to serve as parts of a general theory.

Methodological collectivists on the other hand, argue that collective phenomena are collaboratively created by individuals yet are not reducible to individual action. Most of these accounts argue that although only individuals exist, collectives possess emergent properties that cannot be reduced to individual properties. Such collectivist emergentists believe that emergence of some phenomenon at the macrosocial level cannot be explained in terms of properties and laws about individuals and their relations. And correspondingly, in this view lower-level properties or laws cannot explain higher-level properties and laws, even if there may be laws that exist at the level of individuals and their relations.

Sawyer’s opinion is that whether or not a social property is reducible to individual properties or whether or not a social law is reducible to individual laws is an empirical question. I agree with him since *some* higher-level properties *are* predictable and derivable from the system of lower-level properties³⁴, it remains an empirical question to determine for whichever phenomenon interests us whether or not the relation between the higher-level and lower-level properties is wildly disjunctive and indeterminate and therefore not reducible.

In a very well known and highly debated article published in the journal *Science*, Anderson (1972) argues how academic disciplines are related to one another in terms of laws. Anderson defines the reductionist hypothesis as arguing that the workings of our minds and bodies as well as all of the animate or inanimate matter of which we have any detailed knowledge, are assumed to be controlled by the same set of fundamental laws. However, he is not considered to be a reductionist by other physicists — or at least not a constructivist reductionist — because he argues that being able to reduce everything to simple fundamental laws does *not* mean that we can start from those laws and reconstruct the universe (P. Jensen, personal communication, February 4, 2016).

Anderson’s view of reductionism arranges sciences in a linear hierarchy where the disciplines reflect increasing scale and complexity and where the elementary entities of science X obey the laws of science Y (cf. Figure 9). Each of the arrows represents the claim that Science X obeys the laws of science Y. For example, elementary particle physics³⁵ is at the most fundamental level and is the science with the lowest complexity, on the right. Many-body physics obeys the laws of elementary particle physics (hence the arrow), chemistry obeys the laws of many-body physics (hence the arrow), and the disciplines increase in scale and complexity up until the social sciences. I suggest that it is quite telling that Anderson makes no mention of any difference that could come about between crossing the boundary from the “purely” physical and biological to the cognitive and social, at least in terms of following the laws of the next lower level science. Is

³⁴ For example, Cerf, Einhäuser, Harel, & Koch (2007) predicted human gaze using a preference for face fixation and Martin, Jager, Nisbet, Preuss, & Grimm (2013) predicted population dynamics from the properties of individuals.

³⁵ Many body physics provides the framework for understanding the collective behavior of large assemblies of interacting particles (Mahan, 1991).

there no qualitative difference between the exact sciences and the human and social sciences in terms of scientific laws³⁶?

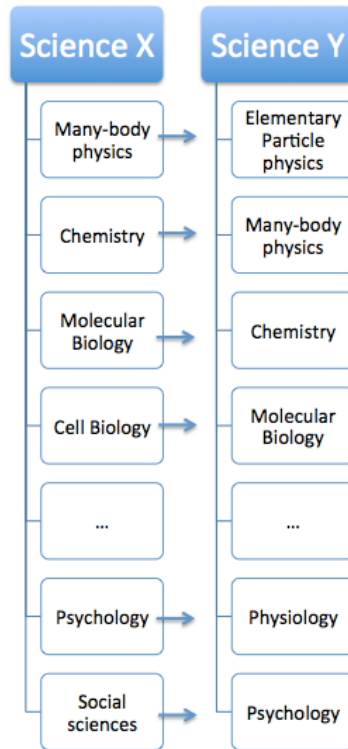


Figure 9. The hierarchy of sciences (adapted from Anderson, 1972, p. 393)

Anderson takes an example from physics to illustrate what we now call emergence and it is for this reason that he is not considered a reductionist by some. The properties of elementary particles do not explain the behavior of large and complex aggregates of such particles. In fact, new properties appear at each level of complexity within the systems of connected disciplines. In addition, understanding these new properties and the behavior that accompanies them requires research that is just as “fundamental” as any other; in this way Anderson responds to those scientists who would give more value to the lowest-level sciences, argued by them to be more “fundamental”. As I have stated, emergence is one of the central tenets of complexity theory, and represents the claim that particular kinds of systems are capable of giving rise to radically new properties not present in the components of the system (Bechtel & Richardson, 2010).

Yet, even though Anderson accepts emergence, he maintains that each higher-level science obeys the laws of the lower level science. On the one hand, if I dehumanize Sawyer’s terms and make them relative to the exact sciences, Andersen is a collectivist emergentist since he believes that emergence of some phenomenon at the macro level cannot be explained in terms of

³⁶ Some researchers have argued that thoughts and behaviors do not follow any laws at all, let alone the laws of physics. Indeed, as Roediger argues, “no general empirical law [involving learning or memory] withstands manipulation across the four sets of factors that Jenkins (1979) identified as critical to memory experiments: types of subjects, kinds of events to be remembered, manipulation of encoding conditions, and variations in test conditions” (Roediger, 2008, p. 225).

properties and laws about entities and their relations at the micro level. Yet, in parallel, each higher-level science must obey the laws of the sciences at the lower levels and less complicated pieces must fit into more complicated systems as we move towards higher-level sciences. One can only conclude that the lower-level laws that Anderson sees higher-level sciences following are not the same laws that govern (if any laws do) the emergent behavior at higher levels. Another way to put this is that for Anderson, the distinction between "in practice" and "in principle" is crucial. Higher-level sciences are indeed governed, in principle, by particle physics, but in practice, we have to find the effective laws of the level in question, that are fundamental for that level.

We have seen the difficulties that pursuing pluralistic explanation can produce, both from the perspective of philosophy of science and from the perspective of attempting a dialogue between disciplines. Neither difficulty was insurmountable. In the next section, I will present the principal danger that confronts the researcher who strives for wide pluralistic explanations; it is also surmountable for the careful researcher.

7.5 The principal danger of pluralistic explanation

It may seem obvious that one will want to generate the most understanding for a given phenomenon, but we have already seen a number of difficulties blocking us from fully capturing the heterogeneity of empirical and theoretical work in disciplines that work on learning or knowledge construction. First, differing worldviews may be an obstacle. Strategies for building theory and for conducting research may differ. People may not want to make the effort and there may be institutional roadblocks. But there is a more dangerous challenge and that is of creating conceptual chaos and I will give two versions of what that can mean. The first deals with not being able to untangle interactions within a system and the second has to do with making errors during agglomeration of results in that what may seem to be the same category is in fact not, because researchers have put differently defined elements into it. If we are to attempt pluralistic explanation, we need to take measures so that we do not fall into these traps.

Singer (1968) formulates this first version of conceptual chaos in terms of the opposition between the aggregative-reductionist view and the organismic-emergent view and proposes a solution that allows for the former to account for the latter:

“...it requires that we define levels of analysis along only one dimension, or at least, only one at a time. If we want to treat the economic, the political and the social sectors of society, and the cultural and the structural attributes of society as different “levels of analysis” [...] there may be no serious harm, but to shift from a horizontal back to a vertical axis and to also include the physiological and the psychological in the scheme [...] is to court conceptual chaos” (Singer, 1968, p. 141).

He further argues that there is a difference between the procedures we use in order to describe a complex system and the procedures we use in order to explain how the system came to be. In his view, we may observe the distribution of subsystem attributes, the relationships among the subsystems, and the interactions among them, and on this basis offer a relatively complete description of that system. However, we cannot explain and account for the properties, behavior, or relationships of the system itself until we can observe and demonstrate links between subsystem attributes, relationships, and interactions. This HDR will propose a method for doing that.

Longino (2013) brings us the second version of conceptual chaos in her examination of a wide variety of research programs devoted to studying human behavior where she specifically looked at aggression and at sexuality³⁷. This second form of conceptual chaos occurs when researchers cut up the problem space differently. More specifically, it occurs when researchers approach the nature of the *explanandum* differently. In other words, when they describe what it is they want to explain, they measure it differently. For example, she looked at behavioral genetics approaches, neurobiological approaches, and social environmental approaches and found they measured the same behavior differently. The first took genetic factors, the second neurological factors and the third social factors as the important explaining factors. It is clear that they are all partial correlations, but where does the incommensurability come in?

Longino describes the incommensurability between these approaches in terms of the causal spaces in which they operate. Each approach uses a different set of factors to describe the behavior and this is the causal space. They won't focus on other factors (i.e. on different causal spaces because they don't have the methodological tools to do so). But why then can't we just combine them? Can't we obtain a kind of additive account? Therein, lies the rub. Having different measurement strategies means that a given factor will fall into a different part of the causal space and its effects will be measured differently, depending on the methods of the approach. Longino gives two examples of how this happens.

She calls the first example of this "the uterine effect". In the behavior genetic approach, this includes the factors that affect a fetus in the prenatal stage (e.g. diet). This is independent of rearing, independent of environment after birth, and independent of neurological factors. But for behavior geneticists, the uterine effect falls into the environmental category because these researchers view that as a context within the body. The foreground and background of the object of focus (Duranti & Goodwin, 1992) for them is on a more micro level. And, for the social environmental approach, the uterine effect will not fall into environmental category, but rather into the biological-environmental category.

A second example is parental divorce. Quantitative behavior geneticists parse the causal space into genetics or heritable factors on the one hand, and environmental factors on the other. They use phenotypic³⁸ differences, and birth, and rearing differences as methodological tools for separating out environment and heritable factors. In their view then, parental divorce is treated as a matter of shared affective environment if the siblings are similar from a phenotypic standpoint. If the siblings are phenotypically different, parental divorce is treated as part of the non-shared environment. On the other hand, a social environmental approach does not parse the environment in terms of the phenotypic features of the individuals. Rather, it investigates the environment directly. In this view, parental divorce is part of the shared environment regardless of the siblings being similar or different.

³⁷ Part of what is written here was inspired by an online interview done by Carrie Figdor on May 15, 2013 and can be found here : <http://newbooksinphilosophy.com/2013/05/15/helen-longino-studying-human-behavior-how-scientists-investigate-aggression-sexuality-university-of-chicago-press-2013/>

³⁸ A phenotype is the observable properties of an organism that are produced by the interaction of the genotype and the environment (<http://www.merriam-webster.com/dictionary/phenotype>). And a genotype is all or part of the genetic constitution of an individual or group.

These two examples — the uterine effect and parental divorce — show us that the same phenomenon is going to get assigned to a different category (i.e. a different causal space), depending on what approach a researcher takes and so its effect is going to be measured differently. This means that what's being measured and the kind of affect it might have will fall into different categories. So the outcomes of the investigations are not going to be commensurable; they cannot simply be just added up.

7.6 Conclusions and implications for combining explanatory frameworks

In §7, understanding the value we attribute to explanations and comparing them in terms of that value guided my analysis of mechanistic and functionalist explanations, two types of explanation that are sometimes put into opposition in the literature, the former being preferred by neuroscientists and the latter by some cognitive psychologists.

I looked to the philosophy of science to analyze the arguments given there and found arguments confirming the possibility of explanatory pluralism on philosophical grounds, but in particular conditions.

In addition, I reviewed three ways of bridging across levels of analysis where the levels were mostly represented by the individual and by the group. These three ways of bridging were 1) by explaining different aspects of a phenomenon through multiple disciplinary perspectives, 2) through the definition of intermediate variables, and 3) through the definition of properties and laws.

One writer in the sciences of complexity gives us vocabulary for talking about linking levels of analysis when interested in the place of the individual within a group during knowledge construction, even if we may not agree with the author's epistemological assumptions about reality. In Gershenson's positivist framework — where there is only one true description of reality — emergence leads to a contradiction because a system cannot at the same time be a set of elements and a whole with emergent properties. In this HDR, I have argued that a system *can* be both decomposable with separate components having a function, but it can nevertheless give rise to emergent properties at a higher level. However, the vocabulary Gershenson introduces is useful for distinguishing between disciplinary views. He makes an ontological distinction between “absolute being” (what something really *is* in reality) and “relative being” (what it is in a particular context). An *abs*-being can be observed from an infinity of perspectives and describe an infinity of properties or aspects. On the other hand, because a *rel*-being is tied to a particular context; it is like the same *abs*-being, but viewed by a set of observers with a similar point of view. *Rel*-beings are different models or metaphors for describing the same thing. If researchers do not agree about a phenomenon, it is often because different nuances in the different *rel*-beings and contexts are being given emphasis. This is a way to describe what happened between action theorists and interactionists, with Turner suggesting a way out, between Aristotelian psychology and Galilean psychology, with Levin suggesting a way out, and between Schegloff and Levinson with Levinson suggesting a way out.

Another way of framing the result of this section is that one must distinguish between aspects of the phenomenon one is studying. Each aspect can be part of an explanation that occurs at a given level. And there are different ways in which these aspects can be related.

Concerning explanatory pluralism, I argued that both reducing to lower levels of analysis and taking account of new phenomena that emerge at a higher level give explanatory power. I argued

that we don't have to choose one or the other if we claim that whether or not a social property is reducible to individual properties or whether or not a social law is reducible to individual laws is an empirical question.

It makes sense to vary the levels at which one looks at a phenomenon. As Bechtel & Richardson, (2010) argue, when researchers operate at too high a level, they tend to cluster together too many activities into one entity. The result is that it becomes mysterious as to how the entity is able to carry out the functions assigned to it. And on the contrary, when researchers operate at too low a level, they may miss some of the complexity in the organization and in the behavior of the system, both critical to understanding how the system actually accomplishes its activity. These authors also argue that once researchers step back and use decomposition and localization in a more global approach that may or may not lead to mechanistic explanations, a researcher's conception of what needs explaining no longer has epistemic priority. Instead, what needs explaining emerges through the explanations and the models we develop. This harkens back to one of the results of my PhD in Cognitive Science in 2003: when people collaboratively elaborate an explanation, they need first to agree on the *explanandum*. What exactly are they going to explain? In the beginning of their interaction, they may set out to explain different things and this only becomes clear as explanations are produced.

Finally, at both the point where a researcher chooses to segregate a system from its environment and attribute an activity to it, and at the point where he or she tries to decompose its function, it is possible to seek out alternatives to mechanistic explanations. These alternatives should focus on understanding how multiple changes can occur simultaneously (Bechtel & Richardson, 2010). Assuming that the system can be decomposed into components, how does the organization matter? What is the nature of the connections? And if we measure human behavior in different ways, we need to make sure that our measurement methods can indeed be unified (Longino, 2013). Managing the multiple approaches to generating explanations of different types, presumes respecting the complexity of local causality as it plays out over time, and successfully combining analyses that focus on variables with ones that focus on process.

8 Methods of investigation for connecting levels of analysis

“We are not students of some subject matter but students of problems. And problems may cut across the borders of any subject matter or discipline” (Popper, 1963).

In this section, I will discuss a selection of methodological approaches for connecting levels of analysis. Causality and generalization function differently according to the paradigm in question and this has consequences for understanding the role of the individual within the group. In this section, I examine the extent to which it is possible to combine multiple methods for connecting levels of analysis. As in other previous sections, this review sets the stage for §9 *A MULTI-theoretical and interdisciplinary model of GRoup And Individual – “Multi-grain” knowledge building.*

A causal relationship is precisely what the word ‘because’ must denote and since the *summum bonum* of science is explanation, science attempts — at least in a positivist paradigm — to describe something beyond the observable phenomena, namely causal relationships and processes (Fraassen, 1993). How then, does the relationship between explanation on the one hand, and causality on the other work when researchers attempt to connect levels of analysis?

8.1 Discovering a law-like relationship between variables: causal description

In the oldest and most widely accepted view for much of the twentieth century, causality was defined as a matter of regularities in data (Maxwell, 2004a). As Maxwell tells it, this view comes from David Hume’s analysis of causality in which it is not possible to directly perceive causal relationships. We can thus have no knowledge of causality beyond the observed regularities in associations of events. This means that causal inference requires some sort of systematic comparison of situations in which the presumed causal factor is present or absent, or varies in strength. In addition, one must implement controls on other possible explanatory factors. In other words, the demonstration of causation is being able to show that there is a regular law-like relationship between two variables, so that a change in the first (“independent”) variable results in a change in the second (“dependent”) variable (Maxwell, 2012). And crucially, we must make the distinction between spurious correlation and real causation.

Hempel and Oppenheim (1948) developed a first version of this position in their proposition of the “deductive-nomological” model of scientific explanation. In this view, scientific explanation consists of deducing particular events from the initial laws and from a set of conditions that govern relationships between relevant variables. The question being addressed is *why* something is the case and not one of ‘making clear’ or ‘making intelligible’ (Antaki, 1994), which is a different kind of explanation.³⁹ According to Hempel, there is a symmetrical relationship between explanation as “why” and prediction. If the event in the argument’s

³⁹ My Ph.D. thesis *Analyse de l’activité explicative en interaction : Étude de dialogues d’enseignants de physique en formation interprétant les interactions entre élèves* [Analysis of explanatory activity in interaction : study of pre-service physics teachers dialogues as they interpret student interactions during labwork] contains an extensive literature review on what explanation may entail.

conclusion has already occurred, it's an explanation and if it has not, it's a prediction (Ruben, 1993).

However, this kind of variable-based causality in order to discover laws, around which experiments are made and predictions rendered possible is not all-powerful and indeed is only a first step in the process. Consider how Shadish, Cook, & Campbell put it:

“The unique strength of experimentation is in describing the consequence attributable to deliberately varying a treatment. We call this causal description. In contrast, experiments do less well in clarifying the mechanisms through which and the conditions under which that causal relationship holds — what we call causal explanation. For example, most children very quickly learn the descriptive causal relationship between flicking a light switch and obtaining illumination in a room. However, few children (or even adults) can fully explain why that light goes on” Shadish, Cook, & Campbell (2002, p. 9).

In my view, such causal description contains a superficial answer to the “why” question. Why did the light turn on? The answer is because someone flipped the switch. But this answer does not get to the full explanation of why the light goes on. As Shadish and colleagues note, a full explanation would require decomposing the treatment (the act of flicking a light switch) into its causally efficacious features (e.g. closing an insulated circuit). And depending on what disciplinary view one takes, the causal explanation — that indeed gives a full explanation of the “why” — will be of different natures (e.g. a physicist’s explanation versus an electrician’s). Recall also the example from the beginning of this manuscript that compared the physicist’s and the biologist’s explanations on the bubbles in beer where rival explanations were proposed from different viewpoints.

8.2 Discovering how it is that the causal relationship holds: causal explanation

Some philosophers argue that speaking about a “cause” requires identifying all the variables that necessarily, inevitably and infallibly result in the effect (Beauchamp, 1974). Shadish et al. argue that it is impractical for the social sciences to achieve such complete explanations, yet also argue why it is important to attempt building them. For example, when the switch does not turn on the light and when replacing the light bulb does not solve the problem, explanatory knowledge can give clues about what to do next (e.g. test for a short circuit). Explanatory knowledge can also help us illuminate a room with no lights. First, there must be a source of electricity, but this source could take many forms (e.g. battery, generator, windmill, solar array). Second, there must be a switch mechanism to close a circuit but this also could take many forms (e.g. touching two bare wires, installing a motion detector that trips a switch when someone enters the room). In addition, this example illustrates how causal explanation is the way to generalize causal descriptions because it tells us which features of the causal relationship are essential to transfer to other situations.

“This benefit of causal explanations helps elucidate its priority and prestige in all sciences and helps explain why once a novel and important causal relationship is discovered, scientific effort turns toward explaining why and how it happens. This can involve decomposing the cause into its causally effective parts, decomposing the effects into its causally affected parts, and identifying the processes through which

the effective causal parts influence the causally affected outcome parts” (Shadish, Cook, & Campbell, 2002, p. 10).

The difficulty, however, lies in the process of decomposition. These authors use the term molar to mean something taken as a whole rather than in parts and in their view, descriptive causation usually concerns simple bivariate relationships between molar treatments and molar outcomes. For example arguing that psychotherapy decreases depression is a simple descriptive causal relationship between a molar treatment package and a molar outcome. Psychotherapy has many different components, such as verbal interactions, placebo-generating procedures, contextual characteristics, time constraints, and payment given for sessions. And measures of depression refer to physiological, cognitive and affective aspects. Explanatory causation breaks these molar packages down – both in terms of causes and effects so as to understand which may specifically be connected to the others. For example, verbal interactions during therapy may cause changes in the cognitive systems of depression but payment for services does not — although it may be important for legitimizing the interaction, so that the verbal interactions can have their effect.

Experiments are not as effective in providing explanatory causal knowledge, yet they are central to science. Shadish, Cook, & Campbell, (2002) give four reasons for this:

- 1) Many causal explanations consist of chains of descriptive causal links in which one event causes the next. Experiments help to test the links in each chain;
- 2) Experiments help distinguish between the validity of competing explanatory theories, for example, by testing competing mediating links proposed by those theories;
- 3) Some experiments test whether a descriptive causal relationship varies in strength or direction under condition A versus condition B. In this case, the condition is a moderator variable that explains the conditions under which the effect holds.
- 4) Some experiments add quantitative or qualitative observations of the links in the explanatory chain (mediator variables) to generate and study explanations for the descriptive causal effect.
- 5) Identification of practical solutions to social problems may have greater priority than explanations of those solutions⁴⁰

But how can a researcher grasp causality outside of a positivistic paradigm and in a way that is not specifically tied to relationships between isolated variables? I argued in this section that experiments are good for pinpointing causal relationships, but less effective in elaborating causal explanations of how those relationships came about. In addition, taking apart a complex system may destroy the connections or at least make it impossible to see them. The next section gives a

⁴⁰ “What is involved here is the difference between explanation and intervention. Many disorders can be *explained* by the failure of the organism to make a normal protein, a failure that is the consequence of a gene mutation. But *intervention* requires that the normal protein be provided at the right place in the right cells, at the right time and in the right amount, or else that an alternative way be found to provide normal cellular function. What is worse, it might even be necessary to keep the abnormal protein away from the cells at critical moments. None of these objectives is serviced by knowing the DNA sequence of the defective gene” (Lewontin, 1997, p. 29), cited by Shadish, Cook, & Campbell, 2002, p. 11).

different view of causality where a system is *not* viewed as a set of independent and dependent variables and the goal is *not* to establish causal links between them.

8.3 Causality within a realist view of the world where causality is visible

Becker's words are particularly enlightening as to the negative effect of Hume's theory on research in sociology, where sociologists end up "hinting at what we would like, but don't dare, to say" (Becker, 1986, p. 8). The view of the necessity of discovering a law like relationship between variables for doing good science led to qualitative analyses based on description as being taken as less impactful. If researchers are not able to fully justify the application of their method, and indeed are almost ashamed of it, then it is not surprising that their results will be less believable.

Maxwell (2012) makes a number of claims when he presents how realism allows for a different view of causality. According to Hesse (2004):

"[...]...realism, as a philosophical stance, is a necessary foundation for any empirical claim to be able to reconstruct facts from evidence and to claim that language (and more broadly any system of signification — visual, textual or aural) has a denotative as well as a connotative function. That language is at some level referential (that it refers to something outside of itself, albeit contingently) is critical, moreover, if one is to be able to make sustainable general claims — about culture, or about any other aspect of human existence. Finally, the realist stance is necessary to anyone attempting to address [...] the question of agency ('Whose discourse?') and the question of causality ('How does meaning arise?')" (Hesse, 2004, p. 202).

Arguing from a realist stance, Maxwell first and foremost claims that qualitative researchers are allowed to draw and support real causal conclusions that are not just speculative. This view is possible if one holds that, contrary to Hume, causality does consist of real causal mechanisms and processes that are visible. In addition, realism does *not* require that these mechanisms necessarily produce regularities in order to be considered as causal. In affirming this, realism goes against the dominant view. Finally, the realist view develops a distinction between variable-oriented and processes-oriented approaches to explanation, such as the distinction between causal description and causal explanation – as presented just above by Shadish and colleagues.

In a causal view based on regularities (Maxwell, 2004a), the question is whether x caused y, or whether there is a systematic relation between inputs and outputs. The goal is to define general laws into which facts can fit and in terms of which particular events can be explained. In this view, "cause" is used mainly to refer to the systematic relation between variables, rather than to causal processes, per se.

In a realist view, positioned as an alternative approach to the regularity view of causality, the question is how x caused y (Maxwell, 2004a; 2004b), and it is not required that x causes y repeatedly for "how" it does so to be a worthwhile question. Causality refers to the actual causal mechanisms and processes involved in particular events and situations, so these are specific to context. How do the things we want to explain come about?

There are two sides to the context coin. The first is expressed within a positivist view of causality. It is the difficulty of finding regularities across contexts in education research in order to achieve generalizability. The second gains more attention within a realist view of causality. It

concerns the fundamental role of context in causal explanations of educational phenomena, *per se*. Developing adequate explanations of educational phenomena and also understanding the operation of educational interventions requires using methods that can investigate the involvement of particular contexts in processes that generate these phenomena and outcomes (Maxwell, 2004a).

8.3.1 Variance theory versus process theory

The distinction between variance theory and process theory can help illuminate the tensions between establishing law-like relations between variables on the one hand and fully understanding the processes by which an event or situation occurs. The goal of this section is to examine the extent to which both views of causation⁴¹ can co-exist in a research program.

“Variance theory deals with variables and the correlations among them; it is based on an analysis of the contribution of differences in values of particular variables to differences in other variables. The comparison of conditions or groups in which the presumed causal factor takes different values, while other factors are held constant or statistically controlled, is central to this approach to causation, and is affirmed as “a fundamental scientific concept in making causal claims” (Maxwell, 2004a, p. 6).

Experimental research that uses quantitative measurement and statistical analysis is associated with variance theory. Process theory, on the other hand is concerned with events and the processes that connect them. The latter analyzes the causal processes by which some events influence other events and is indicative of a realist approach (Maxwell, 2004a):

“When realists say that the constant conjunction view of one event producing another is inadequate, they are not attempting to bring further “intervening” variables into the picture.... The idea is that the mechanism is responsible for the relationship itself. A mechanism is ... not a variable but an account of the makeup, behaviour and interrelationship of those processes which are responsible for the regularity” (Pawson & Tilley (1997), p. 67–68).

Context cannot be reduced to extraneous variables without misrepresenting the nature of the causal process and the context stretches from the immediate physical and social situation to broader social and cultural contexts (Anderson and Scott, 2012). Rather, in much the same way that a detailed, chronological description of a physical process can reveal the causal mechanisms at work (e.g. waves eroding a beach), a similar description of a social setting can reveal many of the ongoing causal processes. This is not yet a distinction between causality being a question of regularity or not, only whether or not causality is observable. Morrow & Brown (1994) gives Sayer’s definition of explication:

“[Explication is] ...the research logic of empirically lifting into view the underlying semantic, sociocultural, and structural relations that are constitutive of historically

⁴¹ Hulswit (2002) makes an initial basic ontological distinction between “causation – or the production of an effect by its cause” and “causality, which is the relation between cause and effect.” For example, the breaking of a glass is described as causation because it is what is produced whereas causality is the relationship between the ‘cause’ of the shattered glass and the shattered glass. In causation, the product is referred to whereas in causality, it is the relation.

unique actors, mediations, and systems, respectively” (Sayer, 1999) cited by Morrow & Brown, 1994, p. 236).

In the realist view of causation, some causal processes can indeed be directly observed, rather than only inferred through measurement of co-variation of presumed causes and effects. In such a view, comparing situations where the presumed cause is present or absent and where one must infer correlations is not required. In the next section I discuss how the process of generalization differs in variance-theory as opposed to process-theory approaches.

8.4 Ways of approaching generalization

Variation theory views generalization as compatible with Humean causality and a search for regularities that is indicative only of relationships established between variables. This view naturally leads to (or stems from) experimental paradigms. On the other hand, forms of process theory view generalization as compatible with realism where causality is directly observable. This latter view naturally leads to (or stems from) paradigms that study naturally occurring interactions. I briefly present the two ways of conceptualizing generalization below. It is in these views of generalization where we see that researchers do not agree whether regularity should be a necessary characteristic of causality.

8.4.1 The two causal generalization problems of the experimental paradigm

Within an experimental paradigm, (Shadish, Cook, & Campell, 2002) point out two causal generalization problems. The first concerns how to go from the particular units, treatments, observations, and settings on which data are collected to the higher order constructs these instances represent. In order to see what this means, consider the following. Nurses in different hospitals administered an educational course to future patients that included a tour of their hospital and covered some basic facts about surgery. The patients were all to be operated on in a single hospital and ten specific outcomes were measured such as their daily activities and the type of analgesics they used after surgery. Results should then be related to likely target constructs such as patient education (the target cause), promotes physical recovery (the target effect), among surgical patients (the target population of units), in hospitals (the target universe of settings). For example, if the hospital tour was not causally related to recovery, but the information about surgery was, then the construct *patient education* could be re-specified as *surgical informational patient education*. This is an example of evaluating construct validity: the data did not really represent the concepts as they were initially specified, but re-defining the boundaries of the concept allowed the results to be generalizable within a more limited framework.

The second problem of causal generalization is to infer whether a causal relationship holds over variations in persons, settings, treatment, and outcomes. Here, Shadish, Cook, & Campell, (2002) remind us that generalization is not a synonym for broader application. In other words, it is not generalization to wonder if an educational program with partially overlapping social and developmental goals that worked in one type of classrooms in one city would work in another type of classrooms in another city. However, the results from a randomly sampled set of participants in a larger population could generalize to all the other unstudied members of that same population. Generalization can also go from broad to narrow (e.g. when the differences between performances of students at private versus public schools could inform a parent’s choice of where to send their children). These are questions of external validity and they share the need

to infer the extent to which the effect holds over variations in persons, settings, treatments, or outcomes (Trochim, 2006).

Shadish, Cook, & Campell, (2002) propose a grounded theory of causal generalization that has five related principles that scientists follow:

1. *Surface Similarity*. They assess the apparent similarities between study operations and the prototypical characteristics of the target of generalization⁴²;
2. *Ruling Out Irrelevancies*. They identify those things that are irrelevant because they do not change a generalization;
3. *Making Discriminations*. They clarify key discriminations that limit generalization;
4. *Interpolation and Extrapolation*. They make interpolations to unsampled values within the range of the sampled instances and, much more difficult, they explore extrapolations beyond the sampled range;
5. *Causal Explanation*. They develop and test explanatory theories about the pattern of effects, causes, and mediational processes that are essential to the transfer of a causal relationship.

In the causal descriptions and causal explanations I develop, I would like not only to ascertain whether a relationship between variables exists and be able to generalize it to higher order constructs and to variations in persons, settings, treatment, and outcomes, but I would also like to focus on process-oriented causal explanations that do not necessarily focus on regularities. And although I do not rule out discovering laws, the pursuit of a set of evolving matrix of chaining models seems more feasible. The next section examines how generalization is viewed from this second perspective.

8.4.2 The pertinence of the singular to the general

In the article where Maxwell (2004a) evaluates the 2002 National Research Council (NRC) report entitled *Scientific Research in Education*, he criticizes the authors' commitment to viewing causal explanations as necessarily general. Recall that a regularity model of causation implies that causality can never be identified in single events or cases, only through repeated observations of a relationship between two variables or events. Maxwell points out that the NRC report presents causality as *whether* x caused y, not as *how* it did so. The report presents three types of research questions: (a) description—What is happening? (b) cause—Is there a systematic effect?, and (c) process or mechanism—Why or how is it happening? The term “cause” is mainly used for the systematic relationship between variables, rather than for causal processes. Any interrogations related to causality (e.g. causal questions, causal studies) refer primarily to the investigation of causal effects rather than the search for causal mechanisms (Maxwell, 2004a). In fact, realism replaces the regularity model with one in which objects and social relations have

⁴² Shadish and colleagues remark that most experimental work probably relies on surface similarity, which is the weakest of these alternatives, but “It requires resources to sample irrelevancies so that they are heterogeneous on many attributes, to measure several related constructs that can be discriminated from each other conceptually, and to measure a variety of possible explanatory processes” (Shadish, Cook, & Campell, 2002, p. 499). Causal generalization will always be more complicated than evaluating the likelihood that a causal relationship exists between variables.

causal powers which may or may not produce regularities, and which can be explained independently of them (Maxwell, 2004a, p. 5).

And as Vermersch (2000) argues, each event that leads to an irreversible state of affairs takes its argumentative force with its sole appearance and in that case, no regularities are needed. Many changes in our lives are the result of unique occurrences (e.g. we don't need to die multiple times to be dead or to say "yes" multiple times to marriage vows to be married). These two examples from Vermersch don't have the same strength. Although a philosopher may argue that dying is what caused you to be dead, it's more intuitive to want to attribute being dead to something that made the physical body stop functioning in a way that maintained its life force. On the other hand, saying "I do" is a performance utterance that has the consequence of one being married. The conclusion is that it's not necessary to show multiple examples of a phenomenon in order to illustrate that it is a determinate force for change.

The second criticism Maxwell makes about the National Research Council (2002) report in terms of its view of causality being necessarily general is that causal explanations in the view of the authors of the report are intrinsically about systematic effects rather than single events and that the goal of scientific research is to replicate and generalize across studies. And although scientific theories are described as conceptual models that explain some phenomenon, the National Research Council authors accept that some research seeks to achieve a deep understanding of particular events or circumstances rather than a theoretical understanding that will generalize across situations or events. However, they don't give this research any value for discerning causes. On the contrary, in their view, such descriptive studies can only generate plausible hypotheses. This is a fundamental difference in how research traditions view causality. Can these two views coexist and if so, what are the consequences?

In a realist view, causation can be observed and descriptive studies can contribute to understanding causes. If we assert it is possible to *distinguish* "between what has been called "natural" causal perception in a single event, and "arbitrary" causal judgment based on the identification of regularities (Dickinson & Shanks, 1995; Kummer, 1995)" (Maxwell, 2004a, p. 8), then this allows us to simultaneously harbor two views of causation. The first corresponds to the realist view and the second corresponds to the Humean view.

THE SINGULAR AS UNIQUE

In addition, holding that both views of causality are possible allows us to argue how a single case is important for generalization. In fact, once a unique phenomenon is made known for the first time, its value is established because it is different from what was known previously. Its uniqueness allows us to either increase the categories with which we work or to invalidate a proposed law through Popper's falsification. The singular as unique is therefore related to the universal as a mode of generalization. The singular as unique must be reckoned with as it modifies our world map by its existence and demands integration into our system of knowledge (Vermersch, 2000).

THE SINGULAR AS THIS PARTICULAR CASE

Each event or individual has specific and original characteristics so that it is possible to distinguish a particular event or a particular individual from all others. Such a view can also be given an ethical take, in that an event or an individual should not be reduced to belonging to a category; an individual or an event is more complex than the category it belongs to. Yet, it is not

possible to take into account all the individual differences that distinguish each particular case from the others. Here again, what is the relation to generalization? According to Vermersch op. cit.), all research, including that which is oriented towards generalization, rests upon the determination of singulars. In other words, the point of contact between research results and the object of research is the manner in which a particular reality is determined. In Schegloff's words "Quantitative analysis is, in this sense, not an alternative to single case analysis, but rather is built on its back" (Schegloff, 1993 p. 102). The search for generalization must occur through data gathering and each elementary piece of data that is gathered is a contact between the researcher and the object of research. How did our data come to be? How did those who answer fill in the questionnaire? How were the questions developed in the first place? Whatever the intermediary character of our instruments or of our categories, there will always be a departure point founded on direct contact with the reality studied, that arose from describing the determinants involved. Was this initial descriptive work rigorous? Vermersch argues that orienting description toward one person, or toward an object of research within a perspective of treating it as unique, as singular, is to renew analysis of the determinants involved. It is therefore essential to pay particular attention to this descriptive level, as it is the basis of what constitutes new subjects of research. In other words, studying the singular as "this particular case" can be thought of as a necessary step towards generalization that is on the mark. It may be pretentious to plan to gather "new data" with research questions already formulated, in that perhaps we are ignorant of what would be interesting to study, given that we don't yet know what we will find. Granted, this is partially the role of pilot studies in experimental paradigms, but Vermersch's point is that it may be the case that in such situations, we have not completely understood the determinants involved.

8.5 Conclusions and implications for methods of investigation for connecting levels of analysis

In this section, I argued for accepting two views of causality: the Humean view where causality can only be discerned as regularities in relationships between variables and the realist view where causality can be directly perceived as part of naturally occurring processes. I take this position because it allows me to have each view's associated methodological techniques at my disposal and thus to enjoy a wider palette in order to answer my research questions.

Within the Humean view, I described the difference between causal description and causal explanation, the goal of the former being to discover a law-like relationship between variables and the goal of the latter being to discover what makes that causal relationship hold.

Within the realist view, it is argued that causality can be perceived — contrary to the Humean view. Realists then, focus on the characteristics of the process that are responsible for a perceived regularity. However, their focus may also be on causal processes that do give rise to regularities yet are nevertheless causal.

Finally, I discussed the two corresponding approaches to generalization, the first emanating from a regularity variable-based view of causality and the second from a more process based view. In the former, there are two goals: 1) how to go from the particular units, treatments, observations, and settings on which data are collected to the higher order constructs these instances represent and 2) to infer whether a causal relationship holds over variations in persons, settings, treatment, and outcomes. In the latter, it's more a question of causal explanation (as it is described in the experimental approach), yet there is not requirement of regularity for causality to exist (e.g. marriage). In addition, I argued for the importance of the single case, both as a unique

case and as the particular case in question. Concerning the former, a unique case brought to light demands integration and may cause theory falsification. Concerning the latter, particular instances can give us insight into defining how the phenomenon we are interested in is indeed *determined*. What *are* the determinants? Are we looking in the right places?

In the next section, I review a selection of my own work using the frameworks developed in §4 *Integrating across disciplinary boundaries: interests & dangers*, §5 *A cross-disciplinary analysis of the individual versus the group in learning contexts*, §6 *Methodological determinism and researcher agency*, §7 *Explanations that compete across levels of analysis*, and §8 *Methods of investigation for connecting levels of analysis*. This will enable me to set the stage for a research program that works toward a multi-theoretical and multi-dimensional model of the co-construction of knowledge in small groups.

9 Building a MULTi-theoretical and Interdisciplinary model of GRoup And INdividual (“Multi-grain”) knowledge building

“More generally, we can also wonder how to imagine a perspective which would allow us to include all different types of discourse [and interaction], written and spoken, monological and dialogical” (Kerbrat-Orecchioni, 2010, p. 95).

In this section I review my own collaborative work with a series of colleagues in a number of different disciplines, principally psychology, language sciences and education. I look back on these collaborations — representative papers are in the appendices (Lund & Bécu-Robinault, 2010; Lund & Bécu-Robinault, 2013; Mazur-Palandre, Colletta, & Lund, 2014; Polo, Lund, Plantin, & Niccolai, forthcoming; Eberle, Stegmann, Lund, Barrat, Sailer, & Fischer, 2013) — and I propose a model for each that illustrates the ways in which they all connected different facets of human interaction and levels of analysis through the construction of intermediate variables. By levels of analysis, like Levinson, (2005), I refer to systems of different orders and I will focus on the cognitive system, the interactional system, the linguistics system, and more peripherally, the social system. All of the examples are akin to Levinson’s in that a set of intermediate variables connects two or more systems together. The intermediate variables are semiotic bundle, procedural explanation, overall emotional framing of a debate, and level of collaboration. All of the intermediate variables are composed of different facets of human interaction. Some models make connections *between* facets of human interaction within one level (i.e. system) and then these facets of human interaction in turn connect different levels of analysis (i.e. different systems) together. The facets of human interaction I studied are diverse (i.e. modes of expression such as talk and gesture, drawing, or manipulation of experimental apparatus, choice of argumentative claim, emotional positioning of that argumentative claim, self-identity footing, group talk type, schematization, tonality of discourse objects, facework, subjects’ expressed feelings, community knowledge, community participation, and duration of face-to-face interaction). This way of modeling the co-construction of knowledge makes it simple to incrementally add new intermediate variables that are composed of different facets of human interaction, belonging to different systems, in order to explore new ways of relating the individual to the group.

In each of the following subsections, I first give the context for the idea of the paper in relation to the research questions we sought to answer and the project it was carried out in. Second, I share the abstract, followed by quotes from the paper that deal with learning and I comment on this latter in light of the literature review and analysis carried out in §5 *A cross-disciplinary analysis of the individual versus the group in learning contexts*. Third, I present a model, for the data in the paper, of how levels of analysis/systems of different orders are connected through intermediate variables that are in turn composed of facets of human interaction. This has the consequence of arguing that human interaction is an integral part all the systems that I write about: interactional, linguistic, cognitive, and social. Fourth, I discuss how we used researcher agency in implementing methods, inspired by §6 *Methodological determinism and researcher agency*. Fifth, with the exception of the article by Eberle, Stegmann, Lund, Barrat, Sailer, & Fischer, (2013), I describe the challenges we encountered or the interest we found in working across disciplines, in light of §4 *Integrating across disciplinary boundaries: interests & dangers*.

Finally, I use elements from §7 *Explanations that compete across levels of analysis*, and §8 *Methods of investigation for connecting levels of analysis* in order to comment on the model.

Before I present each of the ways of connecting between levels of analysis, I take a stand on the production of scientific laws versus the production of scientific models.

9.1 Choosing to *model* causal explanations

It is safe to say that none of the relations I will describe below have made it to the status of “scientific laws”. These are all empirical papers and the studies have been done in particular contexts. I propose that they are partial models that perform initial explorations of the relationships between levels of analysis. No one has tried to falsify them because their notoriety is not sufficient.

In the book *Discovering Complexity*, Bechtel & Richardson (2010) present mechanistic models as causal explanations. They use the term “mechanistic” in the sense that the system under examination produces a behavior in a way that is analogous to that of machines. A machine has interrelated parts, each performing its own function and in combination, these parts produce the behavior of the system. A machine is considered complex when one component can affect and be affected by several other components, with either a cascading effect or with feedback moving from subsequent stages to earlier ones. In this latter case, fundamental dependence is difficult to grasp and the interaction among the components becomes critical. Many other definitions of complexity exist, however, and there is no real agreement in the literature. Complexity sciences has been defined as “the study of the phenomena which emerge from a collection of interacting objects” (Johnson, 2009, p. 3-4); the theory of complexity has been defined as “small local changes [that] precipitate qualitative change in the behavior of a system” (Orden & Stephan, 2012, p. 3), and complex systems have been defined as “an ensemble of many elements which are interacting in a disordered way, resulting in robust organisation and memory” (Ladyman, Lambert & Weisner, 2012). In this HDR, I will treat the systems I study as complex and will call upon complexity theory in order to further model the phenomena I focus on, especially in the future research program.

A mechanistic model for Bechtel & Richardson is a model that ties together more than one level of description by shifting down from the global system to its parts in order to explain how the system does what it does. In other words, it accounts for the behavior of a system in terms of the *functions* (my italics) performed by its identified parts and the interactions between these parts. I propose that this is an additional way to successfully combine mechanistic and functional models of a phenomenon and it is a way to describe the ways I connect levels of analysis of *what can be viewed as a system* through intermediate variables in the examples below.

These models are featured in causal explanations because the model proposed allows a particular phenomenon to be explained in terms of its underlying causes. The goal of these authors is *not* to develop general laws, so when they did turn to a lower level of description within a system, it was not in order to derive antecedently developed laws at the higher level. Rather, they worked at a lower level to create models of mechanisms that would explain specific processes observed at a higher level. They consider this to be part of a dynamic model of theory development, even if laws may not be involved. Instead of a theory being a formal set of laws, it becomes rather a matrix of chaining models bound together partly by how a particular research domain has developed and partly by a shared commitment within a research community to a general framework and method. These models are like blueprints in that they are both partial and

abstract representations of the causal mechanisms at work. Researchers construct a model by envisioning the parts of a system and conceptualizing how such parts will interact with each other. The explanatory power of a model comes from its ability to illustrate how a particular phenomenon or a range of phenomena would be the consequence of the proposed mechanism. This is the approach I take in the next sections.

9.2 Learning and teaching physics in the high school classroom

The work I review here was originally inspired by my participation in an informal research group — locally organized within my laboratory ICAR — that studied reformulation⁴³ from different perspectives within language sciences and within research in education. This group wrote a book, edited by Rabatel (2010) in which a first paper was published in French (Lund & Bécu-Robinault, 2010). Bécu-Robinault — a physics educator (i.e. *didacticienne de la physique* in French) was not an original participant in the group, but I invited her to submit a chapter with me to this edited book, knowing that teaching and learning physics was an ideal context for exploring the issues of multimodal reformulation across different modes of expression. How did learners transform content knowledge when they expressed it in different semiotic registers? Did such transformations expose learner’s specific misunderstandings? I also suggested we submit an article to the International Conference of the Learning Sciences, held that year in Chicago and this section discusses that article (Lund & Bécu-Robinault, 2010), found in full in the appendices.

9.2.1 The semiotic bundle shows individual physics knowledge

Abstract of Lund & Bécu-Robinault (2010)

“This paper studies the multimodal reformulations that teachers and students make when they talk about and do physics experiments in class. Using the framework of semiotic bundles, we show that reformulating aspects having to do with physics knowledge while moving between talk, gestures, drawings and manipulations is done differently by experts and novices. Analysis of video excerpts illustrate that teachers are able to coherently package multiple sets of signs throughout their discourse and actions in the classroom, but students who are learning physics have specific problems that this framework makes evident. In particular, successfully reformulating from one semiotic resource to another implies that the first resource be correctly constructed. In addition, specific *tool affordances* hinder students in their attempt to coherently package multiple sets of signs while this is not the case for teachers. We conclude by suggesting

⁴³ In this book (Rabatel, 2010), we distinguished the pluri-semiotic characteristic of written language from that of oral language. For this group, the term ‘multimodality’ characterizes oral language, with its linguistic elements, its voco-mimo-posturo-gestural para-verbal parameters, to which proxemics are added. Polysemioticity, on the other hand, is a characteristic of both oral language and written language that results from the simultaneous usage — either concomitant or with very close alternation — of different semiotic systems. So, teaching-learning situations will call upon language, drawings, images, experiences, and comments on these elements, all of which are a set of signs that belong to different *sémioses* (this is the signification as a function of context, for example raising ones hand in class will signify I would like to speak, but on the street might mean a signal for a taxi to stop, or in a political gathering, a Nazi salute). In this context we reconsidered what reformulation could mean. Was it legitimate, on a linguistic plane, to speak about reformulation between one semiotic system and another? If we want to articulate thought and language, then can we analyze the continuum of thought in action and talk in action, as well as the emergence, and the co-construction of both thought and talk, within action and by action? How can we analyze action and knowledge without assimilating discourse and action (Rabatel, 2010, p. 9).

ways in which teachers can ease students' difficulties in constructing semiotic bundles" (Lund & Bécu-Robinault, 2010, p. 404).

9.2.2 Learning by manipulation of semiotic resources

<i>Quote on learning</i>	<i>Commentary on learning approach</i>
<p>“Physics learning in the classroom is a complex activity that is both cognitive and social where teachers and students use talk, gestures, drawings and the manipulation of objects to co-construct physics concepts. It has been shown that embodiment through gesture plays a role in learning new concepts (Goldin-Meadow, et al, 2009) and Roth & Pozzer-Ardenghi (2005) propose that for understanding communication in everyday settings, one must take into account not only words and gestures but also all other semiotic resources co-participants produce or find in the setting. In this paper, we choose to address students' conceptual difficulties in the particular case of learning electricity through the study of multimodal reformulation as a tool to co-construct discourse (De Gaulmyn, 1987; Apotheloz, 2001; Lund, 2007), thus taking into account all the semiotic resources that co-participants do” (Lund & Bécu-Robinault, 2010, p. 404).</p>	<p>We were working within the sociocognitivist paradigm where learning is primarily viewed at an individual level and localized within individual cognitive processes, yet influenced by social processes. The individuals interacted together through talk, gestures, drawings, and the manipulation of experimental elements (i.e. bulb, wires, battery) in order to co-construct their knowledge of physics. We considered individuals to be analytically distinct from their partners in interaction even though the ways in which they interacted were integral in how individual knowledge was formed. Learning in physics during the task we studied was carried out through talking, gesturing, manipulating experimental apparatus of wires, a battery and a bulb, and drawing abstract models of electricity. We were able to track the difficulties student novices had, compared to expert teachers, in reformulating concepts of electricity (e.g. current, charge, +/- terminals) from one semiotic mode to another.</p>

9.2.3 A model of knowledge co-construction for individual physics knowledge

In this model, the semiotic bundle is the intermediate variable and it is instantiated as a conglomerat of four different modes of expression, which are in turn instantiated, according to context. The semiotic bundle is a way for conceptualizing how these different modes of expression are connected to one another.

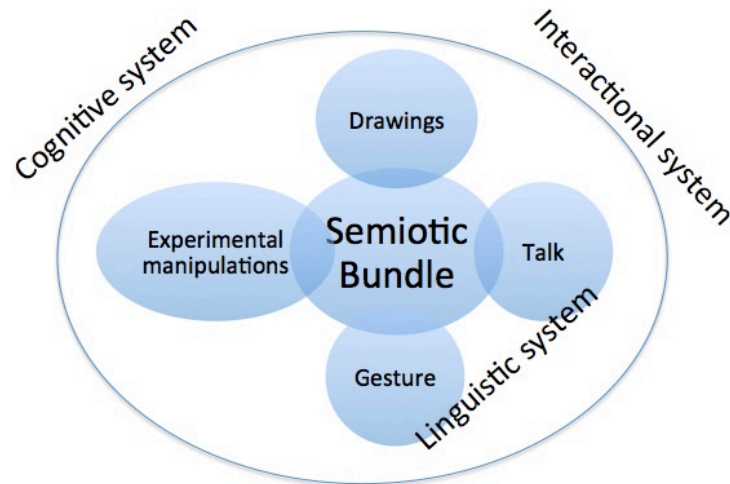


Figure 10. A model of knowledge co-construction for individual physics knowledge

Talk and gesture form a linguistic system and although the literature that argues this does not use the term semiotic bundles, the reason that talk and gesture are connected is through the meanings that they convey together, whether it be in how gesture and talk stem from the same processes (Kendon, 2004) or in how they are both finely coordinated with the actions of others (Goodwin, 1981). In fact, “complex behavior is best understood as a system of interrelated systems” (Levinson, 2005, p. 434). I assimilate the modes of expression here to facets of human interaction. And in the way this work is viewed, these facets are in fact part of two different systems: the cognitive system and the interactional system, with the linguistics system underpinning both of them through gesture and talk. The cognitive system focuses on knowledge content whereas the interactional system focuses on the interactional processes of the individual within the group or in relation to the environment. The levels of expertise (novice, expert) that we described with different instantiations of these semiotic bundles were snapshots of physics notions, expressed in the different modes.

There is knowledge of physics encapsulated within each of the facets of the semiotic bundle (i.e. talk, gesture, drawings of how battery, bulb, and wires can be put together to light the bulb, and experimental manipulations of battery, bulb, and wires in order to light the bulb). This cognitive content can be studied in isolation in order to ascertain the extent to which each facet/mode of expression correctly expresses the physics content. In other words, is the content in harmony with canonical physics? And indeed, it is the reformulation of one mode of expression to another that unmasks conceptual errors. A learner can give the impression of understanding a notion of physics in one mode of expression, but reformulating it into another mode of expression will reveal that this is not the case. This analysis gives us access to the cognitive system. On the other hand, the semiotic bundle is constructed in collaboration with other people, first and foremost through talk and gestures (linguistic system). But even if a student constructs drawings and experiments alone, these constructions are reactions to content that has occurred previously in interaction with others and they are in anticipation of interactions that will occur with others in the future (e.g. a teacher’s evaluation or a peer’s collaboration). If we can track how interactions play out between people through the intermediate variable semiotic bundle, these are phenomena that belongs to the interactional system. In this paper we focussed on static snapshots of level of competence (novice or expert), but we were preparing for analyses over time. Instantiating the

facet level as we think about an abstract model permits us to see how this is possible and it will set the stage for understanding how the semiotic bundle permits connecting the cognitive system to the interactional system once we look more closely at interaction during a class.

The next section shows how evolving semiotic bundles illustrate conceptual change in both groups and individuals. This corresponds to changing levels of expertise that are captured through how the facets of human interaction are instantiated, both through a cognitive system view, and an interactional system view.

9.2.4 Researcher agency in making conceptual difficulties salient

Recall that in Social Network Analysis, a researcher with agency chooses a theoretical assumption regarding causality of the elements in the network and when transcribing, a researcher with agency reflects on the events she wants to highlight and selects a representation to make salient those phenomena of interest. Researcher agency is therefore making a methodological decision that is in line with one's research questions without being forced to accept theoretical assumptions that might not be compatible with the paradigm in which the researcher works or might not be pertinent to the questions being asked. The theoretical assumptions come with our research practices.

In working on Lund & Bécu-Robinault (2010), I suggested that we use Arzarello's (2004) definition of the semiotic bundle, which required transposing its use from the study of mathematics didactical phenomena in the classroom to the study of physics didactical phenomena in the classroom. In order to understand how we illustrated agency in this transformation, we need to first understand the motivation behind Arzarello's desire to define the semiotic bundle in the first place. In his view, a classical semiotic approach was too limited for interpreting the complexity of didactical phenomena in the classroom. Granted, both students and teachers use standard semiotic resources (e.g. written symbols and speech), but they also use other important semiotic resources (e.g. gesture, gaze, drawings, and extra-linguistic modes of expression) and these latter were difficult to account for in the classical definitions of semiotic systems. So Arzarello invented the semiotic bundle to account for all of the resources that are present in mathematics learning processes. We added manipulations of experimental apparatus to our version of the semiotic bundle since they are crucial for physics lab work and since it is through such manipulations that it is possible to observe the extent to which learners have understood the underlying physics concepts. This addition was necessary in terms of observables, but work on integrating it theoretically continued (cf. 9.3.4 *Researcher agency in choosing a meaning for modality: discourse analysis and interactional linguistics*). Our researcher agency was thus expressed in broadening a conceptual tool — the semiotic bundle — to include aspects of the situation that were crucial for our sense-making process.

In addition, we made the decision to use the linguists' vocabulary (*reformulations*) instead of the math educator's vocabulary (*transformations* — Duval, 1993). We did this in order to account for how gesture, posture, gaze, pauses, and manipulation of resources — are all viewed as meaning carrying elements of how the interaction is organized and coordinated by the participants (e.g. Goodwin, 1986; Kendon, 2004). This is because the linguists in the "reformulations group" theorized these aspects of human interaction into an interactional system and we found that they such a system was central for our corpus, whereas the math educators focused more on content and representational aspects — also central, but also accounted for in our approach.

9.2.5 Interest in working between physics, semiotics, and interactional linguistics

One of the tensions we experienced was between limiting our interpretations to phenomena that were observable within the corpus (an injunction from Conversation Analysis) and allowing ourselves to make inferences, based on observable phenomena (acceptable in studying teaching and learning). Researchers differ greatly in their assumptions concerning the accessibility of mental events and processes and this raises questions about the validity of results. To what extent can a researcher have confidence in unobservable phenomena? *Are* mental events unobservable? Some argue that yes, they are unobservable and to mention them in research is mere speculation, whereas others argue that they can be *inferred* from behavior, and that this is sufficient for the purposes of valid research. Still others take a stronger view; they argue that mental events and processes are in fact clearly displayed in behavior, so inference as to what is going on the heads of the participants is not necessary. My position is that tracking the construction of the semiotic bundle through its facets displays how both novices and experts understand notions of physics, but there will always be ambiguities. In addition, it's not always analytically obvious to assure that one is *not* making an inference and is only adhering to what is observable, and in these cases it's important to either rely on inter-coder reliability or describe analyses in fine-grained detail so that the reader is in a position to decide for him or herself if inferences were made and if so, whether they were warranted.

9.2.6 Explanations that cross levels of analysis

I have argued that the explanatory power of a model comes from its ability to illustrate how a particular phenomenon or a range of phenomena would be the consequence of the proposed mechanism. Here, the mechanism is the construction of the semiotic bundle. It is an intermediate variable connecting three systems: a cognitive system, an interactional system, and a linguistic system. The facets of the intermediate variable are in this case modes of expression of notions of physics: talk, gestures, drawings, and experimental manipulations. They are simultaneously part of the cognitive system and the interactional system in that, respectively, all of these facets are 1) considered as exhibiting cognitive content that allows us to evaluate whether or not it respects canonical physics and 2) co-constructed with other people within pedagogical interactions. Here, although it could be much broader (e.g. prosody, mimics, posture...), I am considering only talk and gesture to be part of the linguistic system. However, it is clear that “that actions often analyzed by focusing only on linguistic materials are actually organized by participants drawing on a multiplicity of multimodal resources” (Mondada, 2014, p. 154). Irrespective of what is considered part of the linguistic system, is thus crucial to consider how participants embody resources in interaction in conjunction with the roles that language play.

I am looking for explanations that illustrate causality between facets. This means that all of these physics class activities (talking, gesturing, experimenting, & drawing) mutually determine each other over time — depending on their chronological order — in terms of the extent to which the physics notions are canonically correct in each mode of expression (cognitive system). For example, a student might give the following evaluation “The wire needs to touch the terminals” after seeing another student’s incorrect drawing. This could prompt the author of the drawing to redraw his diagram so that the wire touches both the negative and positive terminals. Any order of facets (talk, gesture, drawing, experimental manipulations) is possible in terms of one influencing the other. They also mutually determine each other in terms of being co-constructed in interaction (interactional system). For example, gestures and talk are formed from the same

initial process of message formation and mutually influence each other in their formation. In the same way, as interaction progresses, all of these facets can co-construct each other, as viewed from the semiotic bundle. In addition, it is also possible to argue that a facet of human interaction that has been expressed has the power to change the expression of a facet previously expressed⁴⁴. If a participant interprets a facet in a way that was not originally intended, but then if the expresser does not challenge that interpretation and moves the interaction forward, then there has been retro-action. However, unless the expresser admits his original intention, this will remain unknown.

All the intermediate variables that I propose will not always be considered to be simultaneously within both cognitive and interactional systems. Some intermediate variables will be more distinctly part of one system or another, but the combination of their instantiations will give rise to the phenomenon that interests us and this phenomenon will be part of both systems (e.g. §9.7 *Understanding how emotion relates the cognitive and social during debate*).

9.2.7 Methods of investigation for connecting levels of analysis

I have argued that some researchers work to develop general laws, while others create models. When one *does* work to develop general laws, describing what happens at a lower level of description within a system is done to derive antecedently developed laws at the higher level (and there are also examples where the reverse is true). However, one can describe what happens at a lower level in order to create models of mechanisms that would explain specific processes observed at a higher level, and this is my approach. Here, the semiotic bundle and its facets illustrate the workings of the cognitive system, interactional system, and linguistic system at a lower level, one where different building blocks describe how knowledge is co-constructed in interaction. In Lund & Bécu-Robinault (2010), however, instead of explaining a *process* at the higher level, we used the semiotic bundle and its facets to describe a *static snapshot* of an understanding of physics in order to compare the novice to the expert. Comparing one mode's expression to another illustrates the extent to which a notion of physics is mastered and is part of the cognitive system. The next section shows how we used the semiotic bundle to move to explaining a process, as we were able to use it to describe conceptual change, and there, the interactional system is highlighted to a greater degree.

9.3 Conceptual change during group lab work in the junior high physics classroom

In studying the differences in the ways novices and experts expressed physics notions when they reformulated them using different semiotic resources, it became obvious that it would be interesting to apply this method of study to pinpoint potential conceptual change as learners progressed through a lesson (Lund & Bécu-Robinault, 2013 — full paper in the appendices). As the Productive Multivocality project took shape, I invited Bécu-Robinault to analyze one of the corpora with me. It consisted of junior high school students using collaborative technology to learn about electricity. Three other research groups analyzed the same corpus from their own

⁴⁴ The reader will recall that a machine is considered complex when one component can affect and be affected by several other components, with either a cascading effect or with feedback moving from subsequent stages to earlier ones. In this latter case, fundamental dependence is difficult to grasp and the interaction among the components becomes crucial.

theoretical and methodological perspectives and Dan Suthers played the role of *provocateur*⁴⁵ for this corpus by inciting us to find ways to make our analyses comparable. (Suthers, 2013b) – see §9.5.2 *Challenges in sharing a corpus with researchers analyzing from different perspectives*.

9.3.1 Collaborative constructions of semiotic bundles show conceptual change

Abstract of Lund & Bécu-Robinault (2013)

“Our analyses illustrate nine instances of what we call multimodal and multimedial reformulations of content beginning either with drawings of physics experiments and going to the manipulation of the physics experiments themselves or beginning with the experiments and going to the drawings. We postulated that each time one of these reformulations occurred, it was a potential (yet rare) pivotal moment for conceptual change because content was being transformed across modes and media. Within the nine instances of reformulation, we found two types of pivotal moments (three instances in all). The first type was changing one’s conception from an intuitive everyday view on physics to a canonical view of physics. The second was maintaining a canonical view of physics, but while also integrating more complexity in terms of experiments constructed, drawings made or concepts talked about. In addition, the notion of the semantic bundle enabled us to show how the ongoing interaction supplied building blocks that illustrated either sustained conceptual change coherent with canonical physics or difficulties that students faced” (Lund & Bécu-Robinault, 2013).

9.3.2 Learning as conceptual change

The learning approach we employed here has the same socio-cognitivist foundation as the learning approach in (Lund & Bécu-Robinault, 2010), but we develop it more extensively. In this section, I do not use the two-column format that places my commentary next to the quotes on learning, as the quotes are extensive. My main commentary is that our theoretical foundations are multiple, but each piece of the puzzle is carefully chosen. They stem from physics didactics, multimodal discourse analysis, literature on internal and external representations, the notion of reformulation coming from language sciences, and semiotics, as viewed by a researcher doing mathematics didactics. This seems like an eclectic mix, but each stone has its place in the edifice and plays a role in the analysis we use to track conceptual change.

Throughout this manuscript, I will periodically employ the term competence. Weinert shows the variety of meanings given to this term:

“A review of the many scientific uses of the terms “competence” and “competencies” shows that they are ascribed a variety of meanings: (a) all performance abilities and skills; (b) only those inherited, domain specific

⁴⁵ Each of the 2011 and 2013 Alpine-Rendez-Vous workshops had a *provocateur* whose role was to challenge the workshop participants to bring out Grand Challenge Problems that could be formulated in relation to the workshop themes (Eberle, Lund, Tchounikine, & Fischer, 2016). Nicholas Balacheff, a member of the STELLAR Network of Excellence steering committee originally suggested this role. We adapted this concept to our Productive Multivocality workshop context by having “discussants” who challenged corpus analyzers to compare theoretical assumptions, analytical objectives, notions of action and interaction, and how data were represented and manipulated.

prerequisites necessary for acquiring primary knowledge systems (especially language); (c) learned (demand-specific) knowledge and skills; (d) individual needs for effectiveness; (e) subjective evaluation of the self; and (f) the entire set of cognitive, motivational and social prerequisites for successful action (action competence)” (Weinert, 1999, p. 3).

In this section, competence in connecting theories and models refers to (c) learned (demand-specific) knowledge and skills.

Quotes on learning

“We use five theoretical positions and/or constructs, construed as a series of steps. Each step is relevant for our approach to understanding difficulties students have in co-constructing and sustaining coherency of physics concepts across modes of communication. We begin with a view on describing physics learning as acquiring the competence to connect theories and models with objects and events in an experimental field (Bécu-Robinault, 2007).

[...]

Our second step concerns the theoretical assumptions underlying multimodal discourse analysis; which elements are involved in the learners’ communication and interaction? We note that language is a form of action and interpreted as a system of meanings, accompanied by forms through which meanings can be realized (Halliday, 1994), rather than statements corresponding to phenomena with an independent existence. We choose to study what we call the ⟨mode, medium⟩ couplet where modes are the abstract, non-material resources of meaning-making and media are the specific material forms in which modes are carried out Kress & Van Leeuwen (2001).

[...]

In our third step, we build on literature on internal and external representations in order to relate these theoretical constructs with modes expressed by particular media and conceptual change in terms of physics theories, models and objects/events. We maintain that it is possible to infer particular characteristics of internal representations of learners by their performance patterns and that there is a specific relationship between internal and external representations. However, we do not propose a specific mental structure for the internal representation, nor do we describe the steps a learner goes through in creating a mental model. Rather, like Zhang (1997) we explore how “cognitive activity is distributed across internal human minds, external cognitive artifacts, and groups of people, and across space and time” (p. 180). We agree with Vosniadou (2007) in that it is not possible to conceptualize learning if not in terms of some change in what is already known, but we add that these changes occur in both internal and external representations, both while taking the individual and the group as the cognitive unit; pinpointing learning becomes a matter of perspective and granularity.

In our fourth step, we consider the manner in which the kind of conceptual change we describe takes place using the notion of multimodal and multimedial reformulation. In this view, thought is constructed by and within language and interaction across talk, images, drawings, gestures, body movements and the manipulation of artifacts.

[...]

Finally, in our fifth step, we propose using a particular lens — the semiotic bundle (Arzarello, 2004) — for viewing as a conceptual unit the reformulations occurring between modes, media and amongst learners. A semiotic bundle is a collection of semiotic sets and a set of relationships

between the sets of the bundle. A semiotic set is composed of a set of signs produced with different intentional actions, a set of modes for producing signs and possibly transforming them and a set of relationships among these signs and their meanings embodied in an underlying meaning structure. A semiotic bundle is a dynamic structure changing over time due to the semiotic activities of the participants and can be “owned” individually or by a group.

9.3.3 A model of knowledge co-construction of both individual and group physics knowledge

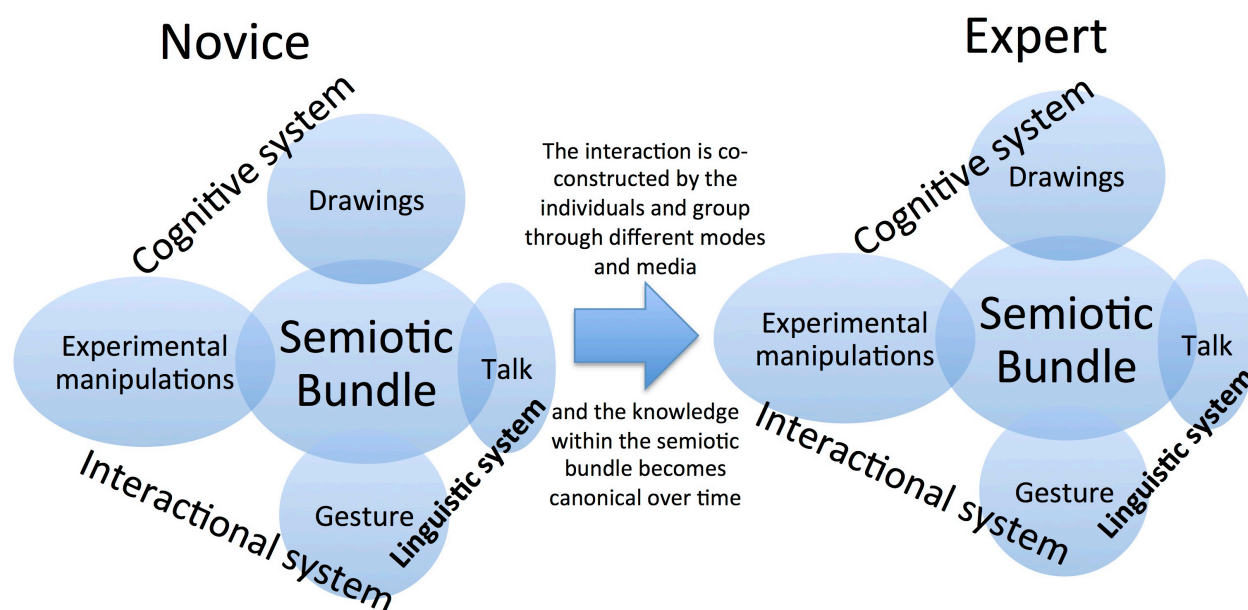


Figure 11. A model of knowledge co-construction of both individual and group physics knowledge

In this model, the semiotic bundle remains the intermediate variable, instantiated as a conglomerate of four different semiotic modes of expression that are facets of human interaction. The semiotic bundle allows these facets to be connected to one another. There are two main differences between this model and the previous one. The first is that it has become *dynamic*, because we used it to track the problem solving during an entire class. The different instantiations of these semiotic bundles change over time and may correspond to conceptual change, if the lesson is productive. The second difference is that a changing semiotic bundle can be attributed to either an individual or a group. In this article, we studied four students working together and as the students spoke, gestured, drew abstract models of circuits and built circuits with batteries, bulbs, and wires, it was possible to use the semiotic bundle to track how the individual was co-constructing knowledge in relation to how the group was co-constructing knowledge, as well as evaluate over the course of the class the extent to which that student knowledge respects canonical physics knowledge.

Looking at the semiotic bundle as a dynamic and changing structure, rather than as a set of snapshots of modes of expression (talk, gestures, drawings, experimental manipulations) makes this intermediate variable more complex. In its static form, the semiotic bundle is able to conceptualize the modes of expressions/facets in either the cognitive or interactional system and show how novices differ from experts. In its dynamic form, it's possible to pinpoint more precisely how particular errors in physics unfold throughout a lesson as we see a reformulation

happening between say, the manipulation of a battery, bulb, and wires and a drawing of a model of electricity, and then later through the verbalization and gesturing that accompany talking about those representations. In addition, since we are tracking group work in class, it's possible to observe when an individual's semiotic bundle does not correspond to the rest of the group's semiotic bundle. This allows us to unpack the interactional influences other group members may have (or not succeed in having) on the conceptual change of a particular member.

9.3.4 Researcher agency in choosing a meaning for modality: discourse analysis and interactional linguistics

As is the case for many specialist terms in the research world, the term multimodality is given different definitions, depending on the discipline that is using it within a particular methodological framework and for specific research questions. In this paper, we considered how two different communities built their definitions and the literature they referred to when we decided on our own definitions. The first community was interactional linguistics (e.g. Mondada, 2014) and the second was multimodal discourse analysis (Kress & Van Leeuwen, 2001).

Mondada (2014) works as an interactional linguist and defines multimodality in the following context:

“Multimodality is a term that has been used in very different ways within several epistemological and disciplinary fields (such as computer sciences, logistics and transports, semiotics, and studies of interaction). While in cognate disciplines ‘multimodality’ might refer to ‘channels’ and ‘medias’, as well as material representations and signs providing and affording diverse semiotic effects -- such as texts, fixed images, moving images, multimedia messages within multimodal semiotics (Kress and Van Leeuwen, 2001) -- within CA [Conversational Analysis] the term has been used to refer to the various resources mobilized by participants for organizing their action -- such as gesture, gaze, body postures, movements, prosody, lexis and grammar. The plurality of ‘modalities’ referred to in this term treats multimodality as constitutive and primary; moreover it considers that these modalities are constitutively intertwined. Consequently, in the literature there are almost no references to monomodality -- although for the practical purposes of a study scholars have sometimes focused their analytical attention on one single resource. Taking seriously the constitutive plurality of these resources has an important consequence: to consider that language is integrated within this plurality and that it is one among other resources, without any a priori hierarchy. In situ, participants might use these resources in a way that is selective and that prioritizes one of them” (Mondada, 2014, p. 138).

Mondada (2014) also defines what she calls complex multimodal gestalts that are local arrangements of resources, formed by “focusing on a web of resources formatting an action” (p. 139). A complex multimodal gestalt depends on the contextual resources mobilized, but one example is an instantiated detailed description of talk, of whole body movement, of pointing, and of directed gaze of a person, all coordinated with whom the person is interacting with. In scrutinizing such examples, we learn how actions mobilize resources that are both co-constructed over time and coordinated with other participants.

“The emergent construction of a complex multimodal Gestalt is done in response to the contingencies of the context and the interaction, adjusting to them and reflexively integrating them in building the progressivity of the action; thus it is done by encountering and solving in real time practical problems encountered by the speaker and the co-participants” (Mondada, 2014, p 142).

This notion of complex multimodal gestalt is both similar and different to our version of the semiotic bundle, adapted from Arzarello (2004). It is similar in that sense making is also understood as a “web of resources” or a set of semiotic resources being used in particular ways. But it is different in three ways. First, we use the semiotic bundle to represent how physics knowledge changes over time and our goal is compare the co-participants’ knowledge (both as individuals and as a group) to canonical physics knowledge in order to evaluate it. Mondada does not present complex multimodal gestalts as a method for assessment or evaluation, but work has begun in this direction (Konzett, 2015). Second, while the temporal boundaries of a complex multimodal gestalt are fairly short in duration, since they describe a particular action, the semiotic bundle is an entity that changes over time. It represents physics knowledge as it evolves and as different actions are carried out and as the lesson progresses, the semiotic bundle is updated. Third, although work in interactional linguistics has begun taking into account the embodied manipulation of artifacts (Mondada, 2012), using conversation analysis techniques to understand on-line interactions using technology is still fairly new (but see Gibson, 2009; Baldauf-Quilliatre & Colón de Carvajal, 2016).

The contexts we study and the focus of our research gaze drives our need for definitions and if we use conversation analysis techniques to study complex systems where modes can be carried out by different media (not usually the case for their analytic focus), then we need a vocabulary for describing this. The authors Kress & Van Leeuwen (2001), who do research in multimodal discourse analysis, distinguish between modes and media: modes are the abstract, non-material resources of meaning-making whereas media are the specific material forms in which modes are carried out. In this view, the modes of gesture, facial expression or bodily posture are carried out in the media of movements of the body, but could also be performed by a robot. Writing can be done with a pen, a typewriter or a computer. Speech can be spoken by a human or synthesized with a computerized voice. Other modes include light, color and texture and their meanings are also conveyed by different media. In the book O’Halloran (2004) edited on Multimodal Discourse Analysis, she writes of a shift in linguistic enquiry where language use is no longer theorized as an isolated phenomenon. Indeed, its analysis and interpretation is put into context by taking into account the other semiotic resources that are simultaneously used for the construction of meaning in whichever context being studied. As Dicks, Soyinka & Coffey (2006) conveniently point out, modes cannot be directly observed as they are abstract resources governed by rules: writing is governed by grammar and visual images may be lexically ordered. In fact, what we observe as researchers in the field are instead the various media in which these modes are produced. What should concern us is the extent to which each medium can mobilize a set of meaning-making resources. We know that different content is more or less effectively conveyed, depending on the form chosen to convey it (Dicks, Soyinka & Coffey, op. cit.). The study of multimodality is therefore achieved through each mode’s respective medium or media.

In our own paper then, we defined the mode, medium couplet in order to distinguish between the abstract meaning-making, per se, and the form used to carry that meaning-making out:

“We note that language is a form of action and interpreted as a system of meanings, accompanied by forms through which meanings can be realized (Halliday, 1994), rather than statements corresponding to phenomena with an independent existence. We choose to study what we call the ⟨mode, medium⟩ couplet where modes are the abstract, non-material resources of meaning-making and media are the specific material forms in which modes are carried out Kress & Van Leeuwen (2001). For example, each mode of communication in this corpus is coupled with a particular medium that allows for its expression: ⟨talk/speech⟩ , ⟨drawing/GS⁴⁶⟩ , and ⟨manipulation/battery-bulb-wires⟩ ” (Lund & Bécu-Robinault, 2013, p. 312).

We needed an analytic construct that allowed us to consider talk (gesture was related to talk within the linguistic system), the models of electricity that students drew on-line within the Group Scribbles (GS) interface, and the experiments they did when trying to get the bulb to light. All of these actions were important in analyzing the extent to which students understood the notions of physics relevant to electricity. Talk did not occur otherwise than through speech. Some text was written in the Group Scribbles whiteboard in the form of labels of drawings, but was not significant enough to use in analysis. Drawing occurred within the Group Scribbles interface and provided different affordances than drawing on paper. For example, GS allowed the students to instantly share their drawings with all of the other groups in the class, something that would have been more difficult just with paper. Upon later reflexion, we realized that the mode/medium couplet was an analytic construct that reflected the state of the art, but that was not necessary for the data we gathered, since each mode was always expressed in only one medium. That said, the construct does give rise to research questions involving the comparison of media for a particular mode in their effectiveness in pedagogical situations. For example, the immediate sharing of drawings representing abstract models of electricity allowed six groups to compare their views of concepts in electricity — almost in real time — as they prepared to manipulate experimental apparatus.

The odd man out in our mode/medium analytic construct was manipulation/ battery-bulb-wires. It makes sense to consider “manipulation” as an abstract non-material resource of meaning making, and also to say that manipulation can be carried out in different media. Here the manipulation is occurring with battery-bulb-wires. But this mode/medium couplet does not have the same status as the other two: talk/speech and drawing/GS. One could argue that the battery, bulb, and wires are the *object* of the manipulation and not the medium in which it is carried out. Perhaps we just needed to conceptualize *manipulation* differently. In 2013 I presented at a CNRS school at which Gunther Kress also presented and I discussed this choice with him. We did not come to a definite conclusion, but agreed that if the construct was useful for reaching our analytic goals, then this was positive, but that more work needed to be done.

⁴⁶ Group Scribbles offers instructors and students a powerful metaphor for thinking about and realizing collaborative learning activities. This metaphor is based on common physical artifacts from the classroom or office: adhesive notes, bulletin boards, whiteboards, stickers, pens, and markers. See <http://groupscribbles.sri.com/>

9.3.5 Challenges in sharing a corpus with researchers analyzing from different perspectives

As mentioned, the chapter reviewed in this section (Lund & Robinault, 2013) was part of the Productive Multivocality project (Suthers, Lund, Rosé, Teplovs, & Law, 2013). And it was the object of a discussion chapter, along with three other chapters that analyzed the same corpus. Below I cite an extract of this discussion chapter (Suthers, 2013b), where the lessons we learned in this particular instance are made explicit:

“The first challenge we encountered was that different analytic approaches make different demands on transcripts, so a transcript produced for one group’s needs is not likely to be appropriate for others. There are potential opportunities in the negotiation of shared transcripts as boundary objects, although these opportunities were not realized in this collaboration.

A related point is that (unlike collaborative video review) analysts in a multimethodological collaboration may be using different representations and tools that are integral to their ways of viewing the world. Therefore, if we are to achieve productive multivocality in such a collaboration by comparing analyses, it is essential to attempt to map between analytic representations, or learn from the intrinsic incommensurabilities that prevent such a mapping.

While abstractions such as transcripts, snapshots, and analytic structures play important roles in each analytic tradition, it may be necessary to go back to the original data record to resolve disputes[...]. The sequential and situated dynamic progression of action offers important information to analysts, as it did to participants. Nuances of how things are drawn and how things are held can change interpretation.

In order to be able to do both of the above (mapping, and returning to the original data), it is essential that the abstractions used by analysts index back to the data record in some shared coordinate system. Typically the shared coordinate is time, but we saw that analysts are likely to parse time into different kinds of units (e.g., utterances, episodes of inscription or interaction, 30 second intervals) and even to label their units using different naming conventions. In the present case, I had some trouble aligning the analyses for these reasons, and there were different time lines based on the 6 videos in the corpus.

But we also saw that not all of the benefits are found in the attempt to align and compare analyses. Some of the productivity of multivocality is found by comparing how analyses constitute the object of study, thereby making alternative theoretical conceptions explicit, such as in our discussion of the distribution of agency and activity across persons and media.

Finally, a third party tasked with moderating multivocal dialogue plays an important role in achieving the above. Some of the issues that turned out to be productive to address arose through my persistent questioning of authors in a public forum. We are accustomed to going our own ways, writing papers that are contributed as independent units, and avoiding conflict. Badgering by a provocateur may be needed to get analysts to look at each others’ work, identify differences, and work them

through. Having served as a provocateur on this project, I can attest to both the frustrations and fruitfulness of multivocal analysis” (Suthers, 2013, p. 369-370).

9.3.6 Explanations that cross levels of analysis

Given that my goal is to describe what happens at a lower level in order to create models of mechanisms that would explain specific processes observed at a higher level, I am using the semiotic bundle and an analysis of the relations between its different modes of expression/facets to describe the process of conceptual change. Conceptual change is viewed as the ways in which the semiotic bundle evolves over time and learning is the extent to which the notions of physics expressed in each of the modes of expression — or facets of interaction — are coherent with canonical physics.

A researcher may choose to consider conceptual change as a regularity. In this case, the facets constructing the semiotic bundle give an “account of the makeup, behaviour and interrelationship of those processes which are responsible for the regularity” (Pawson & Tilley (1997), p. 67–68)”. The construction of the semiotic bundle is the mechanism and the pedagogical context that allows students to reformulate from one mode of expression to another is responsible for conceptual change. Conceptual change is a process that is part of both the cognitive system and the interactional system. This gives a dynamic view to the cognitive system where the cognitive unit is either the individual or the group. The dynamic view of the interactional system is expressed by the changing semiotic bundle as the expression of the different modes — with their cognitive content — are coordinated by the participants. We don’t speak of the linguistic system evolving in this task as the pedagogical goal is gaining knowledge in physics. But arguing that a particular system is pertinent depends on the analytical focus of the researcher. We could have focused our attention on the interaction between the mechanisms of language and the cognitive processes that are used in modeling physics in a way that illustrates how the everyday usage of a term that also has a physics meaning is problematic for learners (Collet, 1996). Such an analysis can show how learners come to give new canonical physics meanings to words for which they previously only had an everyday usage meaning.

9.3.7 Methods of investigation for connecting levels of analysis

This work was situated within a process theory approach rather than within a variance theory approach. We were concerned with events and the processes that connect them. Each time a mode is expressed, this can be construed as an “event”. Each event can be evaluated as being coherent or not with canonical physics, but the reformulation process from one event to another is the most revealing of the origin of conceptual errors. The mechanism is the construction of the semiotic bundle and it may reflect conceptual change, be such change in harmony with canonical physics or not. In this work, we lifted into view the cognitive and interactional relations that constituted how the semiotic bundle was updated, both at individual and group levels.

9.4 Young children’s language development

This research began with the funded project PAMEALE, carried out in collaboration with my company CogniK, and on which Audrey Mazur-Palandre was a post-doc. Mazur-Palandre came to the project with a Ph.D. in psycholinguistics where she had studied the development of children’s oral and written language production. She had focused on the syntactical and discursive aspects of information flow. Her research had not included children’s gesture and

given that our laboratory specialized in gesture analysis in relation to talk, I thought it would first be coherent for Mazur-Palandre to add gesture analysis to her study of child language development and second, it would give her the possibility to branch out theoretically and methodologically, which is the *raison d'être* of a post-doc position.

After reviewing the literature and discussing, we became interested in studying both the linguistic choices and the gesture choices that children make when they are explaining while interacting with others in different contexts. Given the literature, our context, and our questions, Mazur-Palandre developed an experimental protocol where a child instructor explained how to play one of two games to a child-learner in two experimental conditions: game content and visibility condition (i.e. whether or not the children could see each other). The games were part of CogniK's personalized on-line educational game system. This project was a use-case for the difficulty in succeeding in meeting both the constraints of academia and of the company. To what extent could we develop a research project that made an original scientific contribution, but that also was useful in some way for CogniK's goals?

One game called for explaining a spatial task while the other called for explaining a task having to do with counting. The child-instructor was taught how to play the game he or she would explain in a first phase, and then in a second phase, he or she explained it to the child-learner. In this second phase, there were two experimental conditions. In one, the children could see each other. In the other, they could not. Finally, during a third phase, the child-learner played the game under the watchful eye of the child-instructor who could intervene if the child-learner had problems. Our research questions centered on understanding the relationship between talk and gesture, given that the two games mobilized cognitive content of a different nature, and given that participants were either visible to one another or not. We published on the:

- Influence of content on gestural practices of the young child during “how” explanations (Mazur-Palandre & Lund, 2012b);
- explanandum and visibility condition changes in children's gesture profiles during explanation and the implications for learning (Mazur-Palandre & Lund, 2012a);
- explanatory content and visibility effects on the young child's verbal and gestural behavior in free dialogues. Mazur-Palandre & Lund (forthcoming).

Unfortunately, none of these results were immediately exploitable for CogniK. Had CogniK wanted to develop an avatar that explained game instructions to children while they were on-line, then our results could have been pertinent and used to make the avatar's explanations more realistic. But this wasn't a goal.

I had always wanted to work with Jean-Marc Colletta since discussing with him how Jacques Cosnier had been influential for his research and given Colletta's expertise in the analysis of multimodality in the context of language acquisition, he was the perfect collaborative partner for Mazur-Palandre and I. In addition, he used experimental methods to study multimodality and this was compatible with the psycholinguistics approach. We began our collaboration by writing a paper that compared our experimental data with his where both projects involved children giving “how” explanations, but of a different sort:

“...we gathered data on two distinctive types of ‘how’ explanations. As a discourse genre, the ‘how’ type of explanation is interesting to investigate compared to the ‘why’ type of explanation. The ‘why’ type of explanation, also named ‘causal’

explanation, is a type of expository discourse (Nippold & Scott, 2009) that links an *explanandum*, i.e., a phenomenon or behaviour to be explained, to an *explanans* or cause, reason, or motivation for this phenomenon or behaviour (Hempel & Oppenheim, 1948; Veneziano and Sinclair, 1995). At the structural level, causal explanations necessarily link two sequences in the textual form < P because Q > (Grize, 1990; Adam, 1992). In contrast, the ‘how’ type of explanation is more closely related to depiction rather than to expository discourse (Adam, 1992). As a discourse genre and in the same way as depiction, ‘how’ explanation is less formally structured and is more dependent on reference features. As a consequence, there are several kinds of ‘how’ explanation’. In this study, we considered two kinds of ‘how’ explanations:

- ‘process’ explanation (hereafter referred to as PROCESS-EX): depiction of a set of actions leading to some result,
- ‘instructional’ explanation (hereafter referred to as INSTRUCT-EX): formulation of a set of instructions leading to some result.

We selected these two types of ‘how’ explanation as they show some strong differences. PROCESS-EX is fundamentally a monologue-type of discourse act: it does normally answer a ‘how’ question such as “how did this happen”, but subsequently, all a speaker needs to do is coherently build reference in order that the explanation succeed as a discourse act. In contrast, INSTRUCT-EX is fundamentally a dialogue-type of discourse act: it normally responds to a “how shall I proceed?” request from the interlocutor. To succeed as a discourse act, the speaker must both coherently build reference *and* monitor the interactional process with his or her addressee. Consequently, these two contexts differ in their inherent pragmatic constraints: both involve referential constraints, yet only instructional explanation involves interactional (i.e. joint-action) constraints” (Mazur-Palandre, Colletta, & Lund, 2014).

This section will focus on Mazur-Palandre, Colletta, & Lund, (2014), found in full in the appendices, but also on our current work that builds upon it, in progress.

9.4.1 Pragmatic constraints of explanation type influence syntax and gesture

Abstract of Mazur-Palandre, Colletta, & Lund (2014)

In this paper we describe how pragmatic constraints of two types of explanatory interactions influence both the organization of syntactic elements in clauses and gestural behaviour. We provide evidence on how young children confronted with a dual-goal task including both a referential issue and a social interactional issue start to show competencies that are not present in a single referential task. Further, our intention is to contribute to theoretical issues in pragmatics through a study of how children mobilize pragmatic constraints of language production and also to account for language development within the framework of it being understood as a multimodal phenomenon” (Mazur-Palandre, Colletta, & Lund, 2014).

9.4.2 Learning viewed as the development of a child’s competence in managing interaction

<i>Quotes on learning</i>	<i>Commentary on learning approach</i>
<p>“...we aim to describe how pragmatic constraints of two types of explanatory interactions influence the organization of syntactic elements in clauses as well as gestural behaviour. More precisely, we provide evidence on how young children confronted with a dual-goal task including both a referential issue and a social interactional issue show competencies that are not present in a single referential task. Further, our intention is to contribute to theoretical issues in pragmatics through a study of how children mobilize pragmatic constraints of language production and also to account for language development within the framework of it being understood as a multimodal phenomenon” (Mazur-Palandre, Colletta, & Lund, 2014).</p>	<p>Learning is not written about <i>per se</i> in this paper, but it sets the stage for our future work on the development of language acquisition. Our orientation is one of learning as a sociocultural process. First, there is a distinction to make between acquisition and learning in the study of language. Often the former refers to how children master their native tongue whereas the latter refers to mastering a non-native second language. Second, the former may also imply innate, linguistic knowledge (i.e. Chomsky’s generative grammar), but other theoretical assumptions are possible, such as the interactionist position (Al Ghazali, 2006). Third, language acquisition is considered a natural process whereas second language learning is often carried out within pedagogical contexts (outside of immersion type situations).</p> <p>Here, competence refers to a set of cognitive, and interactional prerequisites for successful action, similar to how Weinert (1999) defines action competence.</p>

9.4.3 Model of an individual’s explanation in interaction

In this model, the intermediate variable is explanation type and it mediates between two facets of interaction: syntax (within talk) and gesture. First, talk and gesture is understood as a rudimentary linguistic system, even if the larger linguistic system — at least in an interactional linguistics view — is made up of other facets such as gaze, body postures, movements, prosody, lexis and grammar (Mondada, 2014).

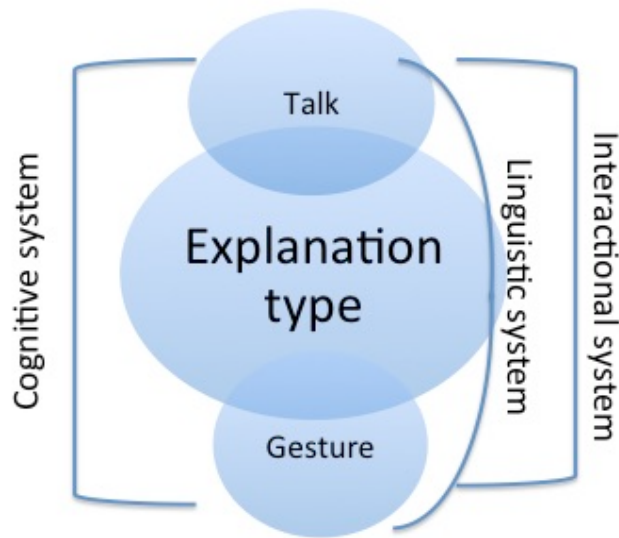


Figure 12. A model of an individual's explanation in interaction

An important point I argue is that what makes up a *system*, and indeed which system to focus on, is always a question of the researcher's analytical focus. In our view, each explanation type (explanations of instructions or explanations of a process) give rise to different interactional constraints. A process explanation requires the explainer to coherently build reference in order to succeed and this is part of the cognitive system. An instructional explanation requires the explainer to coherently build reference but also monitor the social interaction while explaining and this latter is part of the interactional system. These constraints give rise to different explanation profiles for young children and so our interpretation is that the variations in syntax and the variations in gesture arise from the requirements and constraints for each explanation type. Given that a dual-goal task (both building reference *and* managing social interaction) is inherently more difficult than a single goal task (just building reference), it makes sense to first look at characterizing the variations in explanation production and to then plan how to study the dynamics of those differences over time.

9.4.4 Developmental model of an individual's knowledge construction in interaction

Jean-Marc Colletta is currently on sabbatical in my team and he has taken the lead on a project that extends the work begun with Audrey Mazur-Palandre, and continued with the comparison between the experimental contexts of his project with ours. The extension involves moving our research questions to a developmental context by recording interactive explanations in primary school and in junior high and looking at differences across age groups. We have just finished taking data for this project, called GeDéCo (*Gestion multifocale et développement des conduites communicatives complexes* or Multifocal management and development of complex communicative actions), for which we obtained funding by ASLAN.

The GeDéCo project questions the notion of complexity as it is presented through the study of children's spoken language development. Although complexity is at the horizon of all research on acquisition, it is rarely explicitly defined, and it refers to heterogeneous concepts only partially defined that deal with linguistic knowledge, their application within discourse or more rarely pragmatic competence. Our premise is that the complexity of children's language can not be

measured within simple, single focus tasks, but that on the contrary, we need to track and describe this complexity within human interaction, during natural language use, that contains multiple foci (Colletta, Mazur-Palandre, Lund, 2015).

The model that follows is a hypothesis, given that we have not yet analyzed the data.

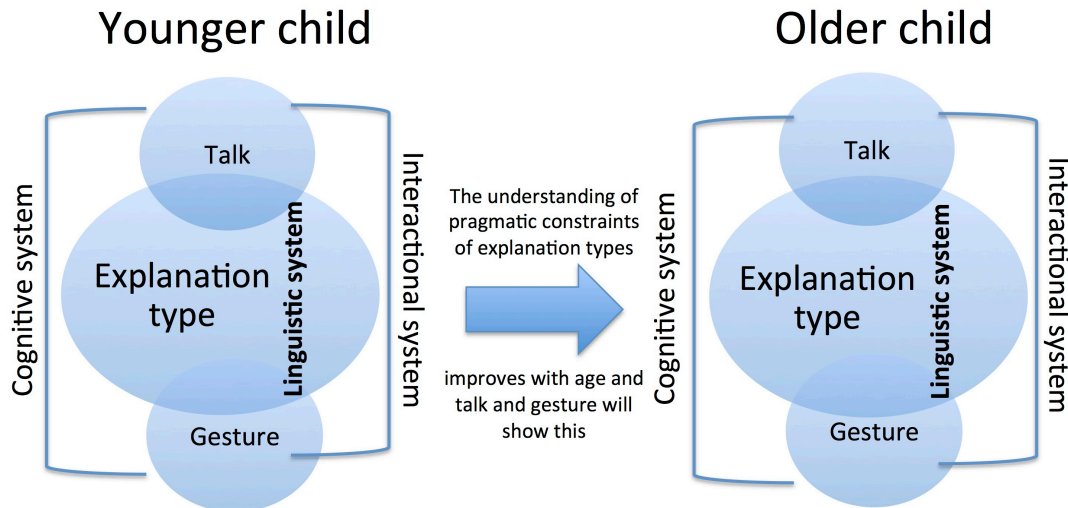


Figure 13. A developmental model of an individual's explanation in interaction

Our goal is to identify the age at which a child is capable of satisfactorily managing a linguistic production within a task where the task has its own constraints in terms of completeness and coherence, while at the same time managing intersubjectivity within an interaction with another person where there is a practical objective. We hope to give two results: 1) a first indication of the evolution toward complexity of children's performance within a multifocal task, and 2) an integration of gestural and verbal and gestural combinations into our analyses so that the evolution of children's performance takes into account multiple aspects of language performance of children at a given age.

Our general hypothesis is that the older a child instructor is, the better he or she will succeed in articulating the three sub-tasks that make up a finalized procedural explanation. We will measure this by evaluating for each child his or her linguistic and co-verbal behavior. The referential construction (task one) — belonging to the cognitive system — will be evaluated by the extent to which the child instructor presented the game, its objectives, the materials, respected the rules, and gave strategies of the game while explaining. Verbalizing the explanation (task two) — belong to the linguistic system — will be evaluated by the extent to which the child instructor sequentially organizes the explanation, employs meta-discourse in relation to his or her explanation, and furnishes an explanation that is complete. The management of the interaction (task three) — belonging to the interactional system — will be evaluated by the extent to which meta discourse on the interaction is present, and the implication of the partners in producing requests, phatic signaling or turn taking regulation. The linguistic system is still, as we have been describing it thus far, the relation between verbal and gestural messages within interaction. Our analyses (will, by hypothesis) illustrate a multi-directional causality between these three systems that is instantiated with how the explanation type is carried out differently, according to age.

9.4.5 Researcher agency in treating pragmatic constraints as a competence to be explicitly learned and not just naturally acquired

Although the distinctions between acquisition and learning referred to above exist, we have chosen to reflect upon the consequences of our results for the explicit teaching of pragmatic competencies. This is radical in that what is naturally acquired as a child develops is rarely the focus of explicit teaching. Yet, children — and even adults — do not enjoy the same level of pragmatic competence. The pragmatic abilities we reveal that are involved in socio-interactional goals could be explicitly taught to children instead of relying on their natural acquisition. This would necessarily involve developing curricula materials that do not currently exist.

Over twenty-five years ago, Plantin (personal communication, April 2016) developed a curriculum for teaching argumentation, but it was met with resistance because it was viewed by the administration and perhaps also by potential teachers as a kind of violation of personality. Such a reaction raises the question of what schools can teach. Are there frontiers around knowledge of a certain nature? Is building competence that is linked to personal expression and interaction with others off limits to the teacher?

9.4.6 Challenges in studying the multimodality of human interaction within an experimental paradigm

The goal of Conversation Analysis is to identify and delineate fundamental practices involved in the production and recognition of actions and sequences of actions (Antaki, 2011), but this has traditionally only been a focus for naturally occurring interactions. As the argument goes, if our goal is providing an inventory or a catalog of recognizable social actions as they occur naturally, it makes sense to *not* use imaginary, made-up examples of language use that are purported to be typical, based on intuition because there is reason to doubt such conjecture. And it is certain that experimental conditions do not embody ordinary contingencies of interaction, instead they “confront participants with quite distinctive, and potentially complicating, interactional exigencies” (Schegloff, 1999: p. 419). But I argue that our goal may not be to produce an inventory of recognizable social actions as they occur naturally, during ordinary conversation. Our goal may be to flesh out how experimental conditions do indeed affect language use within group interactions and to make probability assertions about that, a goal foreign to conversation analysts (Golato, 2003). We may hypothesize that experimental conditions could provoke new language use, not usually present in ordinary interaction, but that may be beneficial for learning, for example. Or we may want to illustrate how different experimental conditions (e.g. process explanations versus instruction explanations) change verbal and linguistic behavior. In that case, our data matches our assumptions and goals. Experimental data therefore escapes the criticism of not being naturally occurring, as it was never argued as being so and since experimental methods are used for different goals than conversational analysis, they can co-exist, as long as experimental researchers do not treat their interventions as “neutral resources for accessing some truth or reality beyond or beneath the data” itself (Speer, 2002).

I have already used a discussion around “multimodality” to argue that specific analytic terms are defined differently according to the disciplinary framework in which they are used. It’s also the case that researchers use different terms for the “same” phenomenon and these terms tell us which discipline is performing the analyses. For example, interactional linguistics and conversation analysts use “talk” and psycholinguistics and phoneticians use “verbal productions” or “speech”. Such terms already give an orientation to what the researcher is attending to and to

how they conceptualize the phenomenon. In our own work, we have tended to use “verbal productions”, given Mazur-Palandre’s training in psycholinguistics (and in order to relate to gestural productions within a developmental framework), but here I am inclined to use “talk”, if only to keep coherent with the previous models involving the semiotic bundle and in order to express solidarity with the usefulness of studying language as it naturally occurs, even within the constraints of a finalized dialogue or task. However, it could be the case that conversation analysts would consider that the work described here may not “merit” using the term ‘talk’ because we may not associate all that conversation analysts do to the term.

A third and related source of tension between studies of multimodality in conversation analysis and studies of multimodality within psycholinguistics that is pertinent for us regarding the project reviewed in this section is to question the extent to which quantitative studies of conversation are possible (Schegloff, 1993). Schegloff argues that there are concerns that constrain the prospects for quantification in studying talk-in-interaction, but that such reflection helps to specify some of the conditions under which it may be done. According to Schegloff, the main idea behind quantification is to assert that what has been observed is not incidental or epiphenomenal, but is on the contrary, significant. But statistical significance is but one form of significance and the seriousness of a claim can be illustrated in different ways. He puts it this way:

“The best evidence that some practice of talk-in-interaction does, or *can* do, some claimed action, for example, is that some recipient on some occasion shows himself or herself to have so understood it, most commonly by so treating it in the ensuing moments of the interaction, and most commonly of all, next. Even if no quantitative evidence can be mustered for a linkage between that practice of talking and that resultant “effect”, the treatment of the linkage as relevant — by the *parties* on *that* occasion on which it *was* manifested — remains” (Schegloff, 1993, p. 101).

He further argues that quantification on large amounts of data is the study of multiples or aggregates of single instances, so “quantitative analysis, is, in this sense, not an alternative to single case analysis, but rather is built on its back” (Schegloff, 1993, p. 102). Showing orderliness at the aggregate, statistical level does not relieve us from having to show orderliness at the level of singular fragments of talk. The questions he addresses then are: Under what conditions can researchers justifiably accomplish building upon single case analysis? What are the dangers and how can they be dealt with? And finally, what can we hope to gain if quantitative studies of conversation are indeed possible and desirable?

Schegloff builds his argument around the notion of proportion, fraction, or percentage. He discusses if something happens $n\%$ of the time, or x out of every y times, what could be the analytically defensible notions of the denominator, the numerator, and the domain on which such a fraction or proportion is taken to report. For him, the denominator represents the “environments of possible relevant occurrence” (p. 103). The numerator is “the set of types of occurrences whose presence should count as events and, given an adequate conception of environments of relevant possible occurrence, whose non occurrence should count as absences” (p. 103). Finally, there should be “a warranted conception of analytically coherent universes that it is relevant for a statistic to refer to, because they are relevant organizational domains of activity for the participants in interaction” (p. 103).

He raises several difficulties linked to quantitative analyses of conversation that can be summarized into needing to know what the phenomena are that interest us, how they are organized, and how they are related to each other as a precondition for being able to bring to bear on them methods of quantitative analysis. I don't have space to go into the motivations behind the examples he gives, but an example where the denominator does not make sense is speaking of "laughter per minute", because people do not laugh "per minute"; laughter only makes sense when its context is taken into account. As for the numerator, choosing what counts as an occurrence is problematic — what *are* all the ways in which speakers do some job or respond to a particular type of utterance? And even when an instance of our numerator does *not* occur, we still need to determine whether this nonoccurrence was nevertheless indicative of an orientation to the practice we are looking for⁴⁷. Finally, on what kind of domain are we making our claims? Not all talk in interaction is organized in similar ways, so it makes sense to make sure that our claims are limited only to the context in which they occur, as understood by the co-participants.

Under the heading *Methods of investigation for connecting levels of analysis* for this section, I detail how we are planning to quantify human interaction. I argue that doing quantitative analyses in order to compare the ways in which different age groups manage procedural goal-based explanations will illustrate differences in development. It is useful only because the entity being quantified is a contextualized action and not an element that has been isolated. The quantifying of the completeness and correctness of the explanations are straightforward. The difficulty lies in determining the ways in which we can account for "managing an interaction" during explanation. We cannot hope to be exhaustive in this at our first analysis, but we will have made a start that can be built upon in later work.

9.4.7 Explanations that cross levels of analysis

Previously we explained the higher-level process of conceptual change by the lower-level mechanism of semiotic bundle construction. The ways in which the semiotic bundle was constructed could be conceptualized as belonging to the cognitive, interactional, and linguistic systems, although our analytic focus was on the first two. Here, we explain the higher-level process of gaining pragmatic competence — a type of conceptual change — by the lower-level mechanism of building finalized procedural explanations. The building of finalized procedural explanations can also be conceptualized as belonging to the cognitive, interactional, and linguistic systems.

Given that the explanatory power of a model comes from its ability to illustrate how a particular phenomenon or a range of phenomena would be the consequence of the proposed mechanism, here we (hope to) illustrate in some detail (see next section) how gestural and verbal practices during finalized procedural explanations change as children get older, from three different system perspectives: cognitive, linguistic, and interactional.

One difference between the two mechanisms proposed so far — semiotic bundle construction and building finalized procedural explanations — is that the former is an analytic construct not

⁴⁷ Schegloff (1993) gives the following example: "Arthur may ask Bill a question, thereby "selecting him as next speaker". If Charles then speaks, the key observation may be not that it was he and not Bill who took the next turn, but that he begins his talk with an apology to Bill for the interruption, or that he makes *his* utterance also select Bill as next speaker, thereby showing his orientation to the relevance of the speaker-selection job done by the prior utterance in the very course of superceding it".

recognized by the participants themselves whereas the latter is recognizable by the participants. On the other hand, neither of the participants in the two projects are necessarily cognizant of their conceptual change in progress — as revealed by the mechanism in question — be it regarding canonical physics knowledge or pragmatic competence in managing human interaction.

9.4.8 Methods of investigation for connecting levels of analysis

Contrary to the first two examples dealing with physics learning where we used a process approach, here we use a variation approach. That said, we are not carrying out an experiment where we have an experimental condition and a control condition. Rather we are comparing three different age groups (8-9, 10-11, 12-13 years) that carry out the same task. We just finished recording the same 3-phase protocol — based on the PAMEALE project — for these three different age groups. In the first phase, a group of child instructors learn how to play Hanabi.⁴⁸ In total, we will have 45 child instructors. In the second phase, they explain the game to three other students of their age group. In the fourth phase, the three students play the game. The child instructor does not play; rather he or she supervises the playing and continues to explain and/or correct the playing if necessary.

We are elaborating a multi-step analytical process where:

- we transcribe the child instructors explanations as well as any interaction with him or her during game playing;
- we suppress a certain amount of extraneous elements (needed later) from the child instructor's transcribed talk so that we can segment it and this allows us to perform a lexical analysis and figure out which terms correspond to the rules and instructions for playing;
- we give each part of essential information in the instructions and in the rules a code and each time we see that the instructor has verbalized such information, we record the code;
- we decide if when the essential information is given, whether it is correct and complete/incomplete or incorrect and incomplete, including multimodal information that gives information not present in verbal productions.
- we code different interactional phenomena that indicate if the interaction is being managed or not (e.g. phatic expressions that relate to language use for social interaction such as “you see?”). Other phenomena include gazes meaningful for management, and different ways of taking into account what a player has said (e.g. requests or feedback).

Our analyses are set up so that quantitative values can be compared across age groups. Each of the steps in our analytical process gives us either cognitive, linguistic, or interactional elements. And yet again, the system that makes it all possible is the interactional system.

⁴⁸ Thanks to Julia Eberle for recommending this collaborative game, invented in Germany.

9.5 Understanding how emotion relates the cognitive and the social during debate

The article reviewed here (Polo, Lund, Plantin, & Niccolai, forthcoming, but found in full in the appendices) is one of the five journal articles published from the work of Claire Polo’s Ph.D. thesis in educational sciences (Polo, 2014), the goal of which was to better understand the spontaneous argumentative practices of students in three countries (Mexico, France, and the USA), who debate a socio-scientific issue. I choose to review this article — *Group Emotions: The Social and Cognitive Functions of Emotions in Argumentation* — because it is a more complex view of how the interactional and cognitive systems can be intertwined. I suggest some minor changes in the proposed model as compared to what was published, notably I change the social system to interactional system, and I reconsider some of the relations between the facets of human interaction. I explain the reasoning behind these propositions in the sections below.

9.5.1 Overall emotional framing of a debate

Abstract of Polo, Lund, Plantin, & Niccolai, (forthcoming)

“The learning sciences of today recognize the tri-dimensional nature of learning as involving cognitive, social and emotional phenomena. However, many computer-supported argumentation systems still fail in addressing the socio-emotional aspects of group reasoning, perhaps due to a lack of an integrated theoretical vision of how these three dimensions interrelate to each other. This paper presents a multi-dimensional and multi-level model of the role of emotions in argumentation, inspired from a multidisciplinary literature review and extensive previous empirical work on an international corpus of face-to-face student debates. At the crossroads of argumentation studies and research on collaborative learning, employing a linguistic perspective, we specify the social and cognitive functions of emotions in argumentation. The cognitive function of emotions refers to the cognitive and discursive process of schematization (Grize, 1996, 1997). The social function of emotions refers to recognition-oriented behaviors that correspond to engagement into specific types of group talk (e. g. Mercer, 1996). An in depth presentation of two case studies then enables us to refine the relation between social and cognitive functions of emotions. A first case gives arguments for associating low-intensity emotional framing, on the cognitive side, with cumulative talk, on the social side. A second case shows a correlation between high-intensity emotional framing, and disputational talk. We then propose a hypothetical generalization from these two cases, adding an element to the initial model. In conclusion, we discuss how better understanding the relations between cognition and social and emotional phenomena can inform pedagogical design for CSCL” (Polo, Lund, Plantin, & Niccolai, forthcoming).

9.5.2 The role of emotions in learning

<i>Quotes on learning</i>	<i>Commentary on learning approach</i>
“CSCL, and, more generally, research on collaborative learning, generally accepts that emotions play a role in the socio-cognitive processes related to learning. This literature recognizes two different impacts of emotions on collaborative learning.	Contrary to the paper analyzed in the section §9.2 <i>Learning and teaching physics in the high school classroom</i> where the research was situated more clearly within the sociocognitive paradigm, the research for the current paper is situated at the crossroads of the sociocognitive

<p>On one hand, emotions appear to have a positive impact on learning, by fostering socio-cognitive conflict (Roschelle & Teasley, 1995). Such effects have been studied for conceptual or practical change, deepening of the space of debate, or even improvement in knowledge (e.g. Andriessen, Pardijs, Baker, 2013, Baker, Quignard, Lund, van Amelsvoort, 2002, Sins & Karlgren, 2013). On the other hand, some studies show that emotions related to argumentative interactions can be detrimental to group achievement. Facing a socio-cognitive conflict implies disagreeing. This can lead to some tensions as thematizing disagreement corresponds to an undesirable move in ordinary conversation (Traverso, 1999, Pomerantz & Heritage, 1984), which can be difficult to manage. The cognitive process can be disturbed by these tensions and participants might use relaxation strategies that do not foster argumentation and learning (e. g. Andriessen, Pardijs, Baker, 2013). These results concerning the potential negative impact of emotions led educational researchers and practitioners to claim that there is a need to develop studies and tools for emotions awareness and emotion regulation (e.g. Järvenoja & Järvelä, 2013)” (Polo, Lund, Plantin, & Niccolai, forthcoming).</p>	<p>and sociocultural approach. It’s similar in that way to the paper in §9.4 <i>Young children’s language development</i>, even though methods are different.</p> <p>Being at the crossroads means that the underlying epistemological assumption is indeed that the individual and the social are seen as two separate units that establish relationships and interact without losing their distinctiveness. Yet at the same time, this position regards human interaction as a process that is mutually influencing in terms of how the individual relates to the group, and vice versa, both within the cognitive system and the interactional system.</p> <p>Finally, the fact that Polo gathered data in three countries predisposed her to an analysis of cultural differences that may intervene in how emotions play a role in the sociocognitive processes related to learning. And we were therefore confronted with the question of whether or not the generalization of our analyses was warranted.</p>
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9.5.1 Model of the cognitive and interactive functions of emotion

In the original diagram, we called the side with facework “social system”. In this manuscript, I have changed it to “interactional system”. This choice is mostly due to the way Levinson (2005) frames the social system versus the interactional system. He places kinship theory in the social system and interactional systematics in the interactional system and he uses the relationship between them to illustrate how the nature of verbal interaction is tied to culture and to social institutions. His intermediate variable is type of social relationship and it has a manifestation in both social systems (through kinship) and linguistic systems (through terms of address). Therefore, since in our model we are not talking about frameworks that describe cultural and social institutions, but rather how the individual interacts with others in order to contribute to the overall emotional framing of the debate, it seems that the better choice is interactional system, rather than social.

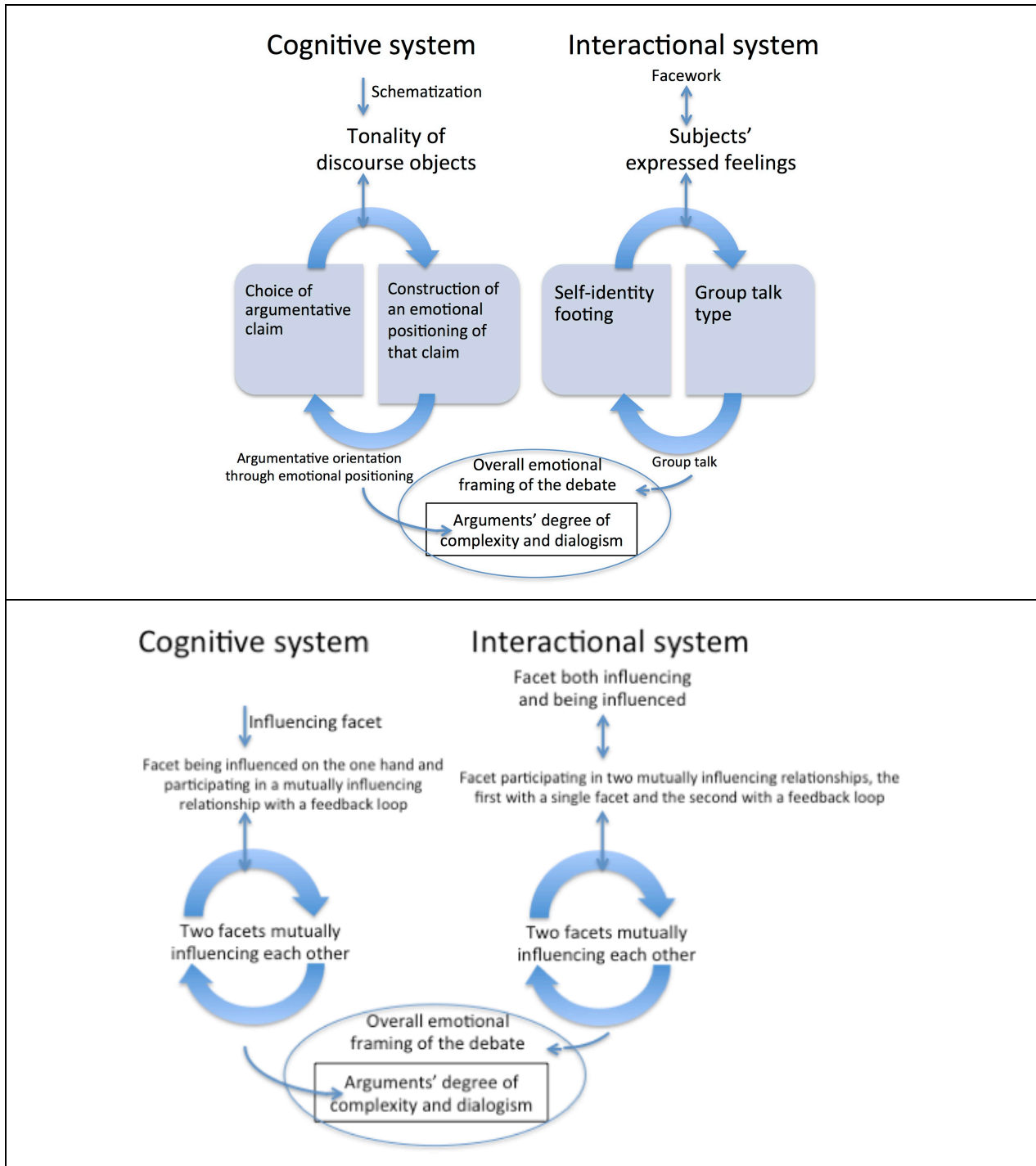


Figure 14. A model of the cognitive and interactional functions of emotion (above) and facet relations (below)

As for the linguistic system, both the cognitive and the interactional systems are characterized through linguistic markers, so unless I put a big label, diagonally positioned, over the whole figure in the likes of “DRAFT DOCUMENT” but where it says “LINGUISTIC SYSTEM”, it’s not obvious how to label this so that it is meaningful from a representational standpoint.

So, what is the intermediate variable in the above model for Polo, Lund, Plantin, & Nicolai, forthcoming? In fact, we have a complex web of intermediate variables that allow for connecting

the cognitive and the interactional systems. If we separate these systems, it is for analytical clarity, but our position is that argumentation is an affectivo-sociocognitive process. On the cognitive side, schematization gives rise to the emotional tonality of discourse objects by giving them an argumentatively oriented representation. Speakers do this by using emotions as argumentative resources. The objects under discussion are given an emotional dimension that provides grounds for elaborating reasoning about them. Still on the cognitive side, in the original diagram (cf. appendices for original paper), the arrow pointed downwards toward the feedback loop between “choice of argumentative claim” and what is now “construction of an emotional positioning of that claim”. This was meant as an analysis path that moved from the individual to the group. Another arrow pointed upwards from “argumentative orientation through emotional positioning” to “tonality of discourse objects”. This was meant as an analysis path that moved from the group to the individual. My own representation emphasizes the feedback loop between “choice of argumentative claim” and “construction of an emotional positioning of that claim” as what happens in interaction with others. And the bi-directional arrow in my own diagram illustrates that this process and the tonality of discourse objects are mutually influential, meaning that the individual tonality of discourse both defines and is defined by this feedback loop. This is not different from what we argued in (Polo, Lund, Plantin, & Niccolai, forthcoming). That said, representations are crucial for conveying ideas and the complexity of this very ambitious modeling merits more thought. In that vein, the original diagram conveyed an aspect that is lost in the version above and that is the idea of a filter on both sides that select what the individual brings to the interaction. On the interactional side, the subject has an internal state. It is facework that filters out from all of the subject’s feelings, which feelings are to be expressed in the interaction. On the cognitive side, the individual begins with an initial formulation of the problem and it is schematization that gives the tonality of discourse objects.

The argument regarding the respective feedback loop holds for the interactional side, but there is a difference on the interactional side that is not present on the cognitive side. In the model here, the arrow between facework and subjects’ expressed feelings is bi-directional. Originally, we had facework provoking subjects’ expressed feelings, but feelings also affect the extent to which a speaker works to preserve his or her own face or that of others (Goffman, 1974). Subjects’ expressed feelings also influence and are influenced by the feedback loop involving self-identify footing and group talk type. The descending arrow moves from the individual to the group and the ascending arrow from the group to the individual. I refer the reader to the original paper in the appendices for details, but the group talk type can be cumulative, exploratory, or disputational whereas the self-identify footing — at the individual level — can be consensual, constructively critical, or competitive. A group talk type is typically distinguishable if each of the participants holds a corresponding self-identify footing. However, participants’ self-identify footing may differ from each other (some may be consensual and other competitive), thus making it difficult to characterize a group talk.

The two feedback loops simultaneously give rise to the overall emotional framing of the debate and define the arguments’ degree of complexity and dialogism. The model in the bottom half of Figure 14 brings out the complexity of the relations. Mutually influencing facets enter into a mutually influential relation with other mutually influencing facets. This is a prime example of complex behavior being best understood as a system of interrelated systems (Levinson, 2005).

9.5.2 Researcher agency in articulating frameworks from different research fields

One reviewer for this article pointed out that the theories of Plantin (2011) and Grize (1996; 1997) are cognitive-linguistic in that they bear on discursive structures. He or she also suggested that the theory of Mercer et al. (e.g. Wegerif & Mercer, 1999) is quite different and may not be a ‘theory’ of the social function of argumentation at all. The reviewer acknowledges that the figures in the paper attempt to link three frameworks in a theoretical manner, but does not find the explanation sufficient. Our response, led by Polo, argues for our position:

“Reviewer four here questions the possibility of articulating theoretical frameworks of different research fields. This comment would lead to a global discussion on epistemological beliefs that would necessarily go far beyond the scope of this paper. Our research is fundamentally interdisciplinary, and based completely on the integration of analytical tools developed in different research traditions, for the purpose of understanding the studied phenomena. We believe that a key contribution of our paper, and its specificity, consists of proposing to deepen such integration by addressing it at the theoretical level. So far, we have found no conceptual obstacle that proved this goal impossible to achieve, on the contrary, we have some modest success over a variety of episodes. Is that a sufficient explanation? Probably not, since it is a first step in a wider challenge that would need the implication of an interdisciplinary community of research. But we do not see how to advance in our study of these multidimensional phenomena (the functions of emotions in group argumentation) without making such attempt” (accompanying letter to the reviewers of Polo, Lund, Plantin, & Niccolai (forthcoming)).

These are complex issues, but our position is that we have made a first start in untangling the emotional, cognitive, and social aspects of socio-scientific debate. Indeed, we should not only model our data, but we should model our processes as interdisciplinary researchers. The elements in §10 *Perspectives for future research* will, in part, define the next steps.

9.5.3 Various challenges in this particular context of the study of socio-scientific debate

CHALLENGES IN WORKING BETWEEN EDUCATION, LANGUAGE SCIENCES, AND COMPUTER SUPPORTED COLLABORATIVE LEARNING

Although it has a strong language sciences focus, the article reviewed here was published from a Ph.D. defended in Educational Sciences. One concept that was often difficult for the education community to apprehend was the absence of targeted knowledge that we wanted the students in our pedagogical situation to learn. How can a pedagogical situation not have targeted knowledge? But we were interested in students’ spontaneous argumentative competence⁴⁹, and not on teaching them to learn to argue, or to argue to learn (Andriessen, Baker, & Suthers, 2003; Jonassen & Kim, 2010; Von Aufschnaiter, Erduran, Osborne, & Simon, 2008). We come back

⁴⁹ Competence is defined similarly to the pragmatic competence in the previous section on monitoring the giving of a finalized procedural explanations to a peer. Here, competence refers to a set of cognitive, and interactional prerequisites for successful action — that is, succeeding in competently performing the various facets of interaction. For example, to what extent can a student construct an emotional positioning of a claim? To what extent can a student given a tonality to discourse objects? Use facework?

here to a distinction between a competence that is explicitly learned and a competence that is implicitly acquired, and to the questions that are raised when we consider explicitly teaching the latter.

We framed the article reviewed in this section with the observation that many computer supported collaborative argumentation systems fail to address the socio-emotional aspects of argumentation and collaborative learning and that our model can be useful for doing so. One criticism we obtained was that our model did not have sufficient empirical results that demonstrated the model's value. The first point regarding amount of empirical results often comes from researchers in a coding and counting type of approach where an analysis grid is applied to two different situations in order to quantitatively distinguish between the effects of the two situations. Although Polo gathered data in three cultural contexts, our first goal was not to compare these situations, but rather to illustrate their similarities (pinpoint universal aspects?), despite potential cultural differences. Regarding the model's value, it allows us visualize the social and cognitive functions of emotions that play a role in argumentation and may help the educational researcher in interpreting how authentic classroom interactions play out. Secondly, our model aims at understanding collaborative learning, per se, and this should be relevant for the *computer supported* collaborative learning community, but they usually prefer that a model have some advice to give on how to design computer supported collaborative learning situations, be it in terms of technology, or in terms of pedagogical scenario. For example introducing sociocognitive tension may be beneficial, but too much high-intensity disputational talk may inhibit group reasoning. Finally, it may not be beneficial to be aware of ones own emotions and those of the group even if emotion awareness is currently a hot topic (Järvenoja & Järvelä, 2013). It may be that the way in which emotional positioning works within argumentation is best left to work implicitly, so this should be considered in any potential teaching initiative. For example our data showed that when a group was aware of cognitive conflict that was not particularly uncomfortable, this only led to relaxation strategies of a social nature and this encouraged disengagement. The awareness effort could be better put into choice of topic, scripting the pedagogical interaction, and scaffolding the learners.

DIFFICULTIES BETWEEN COMMUNITIES OF RESEARCH WRITTEN IN DIFFERENT LANGUAGES

There is no obvious solution to what this sub-heading alludes to, but it's an important point. The reviewers for this article complained that the references we made to the French literature, notably work of Plantin and Grize had not been sufficiently translated into English for them to adequately understand them in the context of our article. Those of us who can read in more than one language are definitely at an advantage in terms of increasing our understanding of a phenomenon of interest. Interestingly, the burden of making that understanding known to the monolinguals falls upon our shoulders.

CROSS-CULTURAL ANALYSES

Although we have made some tentative distinctions between the nature of the corpora from Mexico, France, and the USA (do the French exhibit more disputational talk and the American more cumulative talk?), we have not focused on this. Working in a cross-cultural context, Brown & Levinson (1987) argue that the degree of threat to face depends jointly on:

- the normative level of imposition of the given action within the culture;
- the power relationship between the speaker and the hearer;

- the social distance between the speaker and the hearer.

If so, then one culture may treat a request as a trivial matter whereas another culture may interpret the request as a major imposition. And we cannot safely ask the same thing of a close friend and of a supervisor. But given that our data in Mexico, France, and the USA were all schoolmates debating together in similar contexts, we need to reflect more about how to frame any potential cultural differences and how to address them methodologically, given that one criticism we have had is that our corpora are not extensive enough to allow for cultural generalizations.

9.5.4 Explanations that cross levels of analysis

The overall emotional framing of the debate is necessarily built up over time as an interaction progresses. In the previous examples, the mediating variable was a type of conceptual change: either learning physics or gaining pragmatic skills in managing the interaction during explanation. Here, socio-scientific debate can also lead to conceptual change — be it in terms of cognitive content or interactional competence, but we focus on the emotional framing of the debate where different facets exist that belong separately to either the interactional or cognitive system, while being located with linguistic markers. It could be interesting to consider the emotional framing of a debate as a competence to be developed, as it is an argumentative resource that functions in both cognitive and interactional systems. Such a competence has never been the focus of explicit learning, only of natural acquisition, and it may be more difficult than for interactional pragmatic competence to imagine a curriculum for it in which it could be explicitly taught.

The difference between this model and the ones concerning learning physics or gaining pragmatic competency in language acquisition is that the intermediate variable — here, overall emotional framing of the debate — does not have only one set of facets that are considered to be *both* part of the cognitive system and part of the interactional system. In this model, there are facets on the cognitive side and different facets on the interactional side, but they come together to form the overall emotional framing of the debate. However, this model is similar to the previous models in that the facets I have defined *co-construct* the intermediate variable. The multi-directional relations between talk (and gesture), drawings of electricity models, and manipulations of batteries, bulbs, and wires construct the semiotic bundle. The bi-directional relation between gesture and talk construct the explanation type. And here, complex relations of embedded, mutually influencing facets construct the overall emotional framing of the debate.

In this section then as opposed to the previous models, it's additionally a question of levels of analysis being *embedded* in other levels of analysis. In other words, the linguistic system — in the form of a variety of linguistic markers — is what allows us to qualify both the cognitive and interactional systems. We just illustrate one set of these linguistic markers (but see Polo, Lund, Plantin & Niccolai, (forthcoming) in the appendices), but others exist for the other variables in the model. Five discursive markers allow us to decide whether talk is exploratory, cumulative, or disputational:

- 1. Are assertions and refutations justified?
- 2. Do participants elaborate on the argumentative content of previous turns?
- 3. Do they critically evaluate each other's arguments?

- 4. Is everyone taken into account when making a collective decision?
- 5. Do the individual contributions gradually integrate the rest of the group's supporting or opposing argumentation?

If we answer yes to these questions, talk is exploratory. It is more cumulative if there is not much critical evaluation of each other's arguments. It is more disputational if there are more repetitions than justifications (indicator 1), an intermediate amount of topical alignment (indicator 2), strong but unconstructive use of critical sense (indicator 3), a lack of accounting of other's opinions (indicator 4), and not a lot of collective ownership of complex dialogical arguments (indicator 5).

The mechanism we call overall emotional framing of the debate is more similar to semiotic bundle construction than to building finalized procedural explanations. Participants themselves recognize the latter whereas the first two are not recognizable by participants as something they do. On the other hand, none of the participants in the three projects are necessarily cognizant of their conceptual change in progress — as revealed by the mechanism in question — be it regarding canonical physics knowledge, pragmatic competence in managing human interaction, or developing an overall emotional framing of a debate.

9.5.5 Methods of investigation for connecting levels of analysis

Given the multiple feedback loops that we argue are under mutual influence in the model of how emotion relates the cognitive to the interactional through linguistic markers, I argue that in order to go further, we need a methodological approach that adapted to the study of complex systems. Often the study of complex systems is taken to be similar to the understanding of a complex machine. In the same way that a machine can be broken down into components, a system can be broken down into components. If it makes sense to say that a component has a function and that functions can relate to each other, then we can attempt to build a mechanistic model as causal explanation (Bechtel & Richardson, 2010).

However, even though Bechtel & Richardson (2010) pursue the development of mechanistic models as causal explanation, they also realize that mechanistic models are not always possible to construct and attempting to elaborate them for a particular complex system may fail. What is interesting is that when this process fails, it can be productive. In this HDR, I propose to take on board this methodological approach for work in the future, which is one way to approach the explanatory pluralism that I seek. In order to see how failure to develop a mechanistic model can be productive, and how this can lead to explanatory pluralism, I must first present the two heuristic techniques that Bechtel & Richardson (op. cit.) use in developing their mechanistic models.

The first heuristic technique is decomposition:

“Decomposition allows the subdivision of the explanatory task so that the task becomes manageable and the system intelligible. Decomposition assumes that one activity of a whole system is the product of a set of subordinate functions performed in the system. It assumes that there are but a small number of such functions that together result in the behavior we are studying, and that they are minimally interactive. We start with the assumption that interaction can be handled additively or perhaps linearly. Whether these assumptions are realistic or not is an open question;

indeed, at the outset we often simply do not know.” (Bechtel & Richardson, op. cit., p. 23-24).

The second heuristic technique is localization:

“Localization is the identification of the different activities proposed in a task decomposition with the behavior or capacities of specific components. In some cases, we may be able to identify (through fairly direct means) the physical parts of the system in which we can localize different component functions. In other cases we may have to rely on various functional tools for determining that there are such parts, without being able to identify them; for example, we may be able selectively to inhibit their operation and observe the consequences on behavior. We need not assume that a single part in this sense is a spatially contiguous unit; in fact, we know that in many cases it is not. A functional unit may be distributed spatially within the system. Localization does entail a realistic commitment to the functions isolated in the task decomposition and the use of appropriate techniques to show that *something* is performing each of these functions” (Bechtel & Richardson, op. cit., p. 24).

Some complex systems resist decomposition and therefore systemic functions cannot be localized. The extent to which the assumption of decomposability is realistic can be decided only a posteriori, by seeing how closely we can approximate system behavior — presumably by simulation — by assuming that decomposition is possible. Assuming that decomposability is possible can lead to erroneous explanations, but this is an interesting way to begin the task of explaining and understanding complex systems. In addition, failure of decomposition can be productive in that it can lead to the discovery of additional important influences on behavior, or perhaps to the acceptance of indeterminate behavior. When decomposition and localization fail, when these heuristics do not allow us to produce a mechanistic explanatory model, it becomes necessary to expand the range of explanatory models under consideration. This can be done with other techniques, such as causal narratives.

There are three other reasons that Bechtel & Richardson give, for which it is interesting to attempt to construct mechanistic models. First, there is often no initial well-delineated *space* of explanatory models in complex systems. Second, there may not even be a clearly defined range of phenomena to be explained. So attempting to build mechanistic models is a way to explore their possible space and also a way to determine the precise range of phenomena to be explained. Third, hypothesizing that the complex system under examination is decomposable reduces the cognitive demands that understanding it requires. Decomposition and localization guide the search for an adequate model within the space that we have initially defined, but in the case of failure, even if rethinking that space becomes necessary, the procedure of developing explanatory models has been begun and it is likely possible to see in which direction now to go.

9.6 Fostering interdisciplinary collaboration at the community level in the field of technology-enhanced learning

This collaboration began when I approached the piloting committee of the STELLAR Network of Excellence of Technology Enhanced Learning with a proposition, once it was established that I would host the 2011 Alpine Rendez-Vous in the French city of La Clusaz. In the jargon of the European Union, the Alpine Rendez-Vous was called an instrument. An instrument was supposed to accomplish a goal and one of the goals of the Alpine Rendez-Vous was to build and maintain a

European network of researchers in Technology Enhanced Learning. But how was the building and maintenance of such a network to be measured? I knew a colleague in physics — Jean-François Pinton, the current director of *Ecole Normale Supérieure*, who had worked in modeling networks of different types. He was involved in a company that manufactured RFID tags⁵⁰ and had already used them to model networks in different contexts (e.g. a hospital setting where the tags were used to track the hypothetical progression of infectious diseases - Vanhems, Barrat, Cattuto, Pinton, Khanafer, Regis, Kim, Comte, & Voirin, 2013). They had also used them at a conference (cf. Cattuto, Van den Broeck, Barrat, Colizza, Pinton, & Vespignani, 2010), and so I thought that we could use the RFID tags to ask questions involving level of collaboration between disciplines, and between type of position, in order to get an idea of the extent to which the Alpine Rendez-Vous was successful in building the community. I suggested this to the piloting committee who agreed. Frank Fischer from the University of Munich was on that committee and he recruited one of his Ph.D. students — Julia Eberle — to put together a formal study. I financed and organized the RFID tags with the help of Pascale Pauly, paid by the University of Munich to help me organize the conference locally. Julia, Pascale, Karsten Stegmann, some other Munich colleagues and I gathered the data during the 2011 Alpine Rendez-Vous and we continued the study for the Alpine Rendez-Vous 2013, organized in Villard-de-Lans, also in France (Eberle, Stegmann, Fischer, Barrat, & Lund (2016, March). In this section, I review our first paper together (Eberle, Stegmann, Lund, Barat, Sailer, & Fisher, 2013, found in full in the appendices).

9.6.1 Knowledge influences level of collaborative relationship

Abstract of Eberle, Stegmann, Lund, Barat, Sailer, & Fisher (2013)

“In this study, the integration of new members into a scientific community that comprised to a large extent members from the CSCL community was investigated. New members usually lack the necessary knowledge to interact successfully with more experienced members of a scientific community and to find collaboration partners. We investigated how the level of community participation and support for community knowledge were related to the building of new collaborative relationships during a scientific conference. Participants’ interaction behavior was tracked using RFID devices; social network questionnaires and a bibliographic analysis provided additional data. We found that newcomers do not interact less with other participants than experienced members, but develop fewer collaborative relationships. The chances that newcomers’ interactions lead to the building of new collaborative relationships were increased by access to explicit relevant community knowledge. Making such knowledge explicit seems to be a useful means for supporting newcomers in scientific communities” (Eberle, Stegmann,

⁵⁰ The RFID tag is based on an open design by the OpenBeacon project and features a microcontroller, a radio transceiver operating in the 2.4 GHz ISM band, an antenna embedded in the printed circuit board, and a lithium battery. The spatial proximity relations are relayed from RFID tags to radio receivers, called RFID readers, installed in the experimental area. The radio receivers are connected to a central computer system by means of a Local Area Network. The readers listen on the infrastructure radio channel for incoming packets, and whenever they receive a packet they encapsulate it in a UDP (User Datagram Protocol) packet and relay it to a central server, where it is time stamped and stored (Cattuto, Van den Broeck, Barrat, Colizza, Pinton, & Vespignani, 2010).

Lund, Barat, Sailer, & Fisher, 2013).

9.6.2 Learning how to be a member of a scientific community

<i>Quotes on learning</i>	<i>Commentary on learning approach</i>
<p>“A usual way to learn about a scientific community is reading papers. But to read only some of them can give a peripheral community member a very biased idea of the community. In this respect, face-to-face meetings are of high relevance to scientific communities; among other things, they provide possibilities for peripheral members to gain community knowledge and interact with other members. Such meetings make communication easier, especially in scientific communities like CSCL [Computer Supported Collaborative Learning], which consist of members with different native languages complicating the distribution of results and effective communication (Kienle & Wessner, 2005). Workshops and conferences are used to foster researchers’ communication and learning about the findings and approaches of others, but also to integrate newcomers. Such events bring participants together and allow them to focus on learning activities and on community building, and can be called encapsulation. Although encapsulation is a widely used strategy in different contexts (Levine & Moreland, 1991), it can be organized in different ways: workshops usually allow for more one-on-one interaction, while (larger) conferences usually focus on other types of communication. However, it is unclear how one-on-one interaction is related to researchers’ learning in the scientific community. Access to community knowledge, especially to knowledge about other members, seems in particular to be also very relevant and it might be helpful to foster it during encapsulation events.</p> <p>In this study we investigate factors which influence the integration and learning of newcomers in a scientific community. We adopt a social network perspective on learning</p>	<p>This paper takes a sociocultural view on learning, in the tradition of Lave & Wenger (1991) where the unit of analysis is situated social practice. But as I have already argued, even though these authors hold that the individual cannot be separated from the social and cultural context, they accept independent units (e.g. speaking about a member of a community means that there <i>is</i> an individual who is a member <i>and</i> a social and cultural context, which is a community). Therefore, analytic dualism is implicitly accepted. In our paper too, we worked from the acceptance of analytic dualism. But although we were taking the view of learning as becoming an active participant in a community, we performed an experiment and experimental methods are more typically used in paradigms of behaviorism, cognitivism, and socio-cognitivism. Our goal was to measure the extent to which an individual participates in a community and we proposed an intervention to see if gaining community knowledge could change the level of community participation. In addition, we examined two factors. First, we measured the extent to which individuals had been participating in the community (i.e. peripheral or active community member). Second, we measured three types of collaborative relationships: new interactive relationships during the ARV, as measured by the RFID tags, new subjective collaborative relationships, as measured by questionnaire responses indicating with whom the person would like to collaborate. Finally, third, we measured the development of new objective collaborative relations, as indicated by results of a Google Scholar search showing joint papers.</p> <p>Our first research question focused on what</p>

<p>and integration by focusing on the building of collaborative relationships between community participants as the visible and desired consequence of integration and learning. The social network approach offers two different ways to look at the building of collaborative relationships. First, we can look at individual persons and how successful they were in building new relationships; second, we can also look at all individual relations between two community members and what factors influence the probability of a random relation to become a collaborative relationship” (Eberle, Stegmann, Lund, Barat, Sailer, & Fisher, 2013).</p>	<p>can be considered as personal characteristics – the extent to which a participant knows about other people and the extent to which they participate in the community. We asked if these characteristics predicted a participant’s number of new collaborative relationships. Our second research question focused on relations with other people — to what extent does time spent interacting with others (taken in addition to the intrinsic characteristics) predict a participant’s number of new collaborative relationships?</p>
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9.6.3 Model of an individual’s level of collaboration within a research community

In each of my models, the intermediate variable is one whose nature changes over time in a way that is comparable to conceptual change, whether or not this learning is explicitly sought out or acquired naturally within normal human interaction. Here, the intermediate variable is levels of collaboration and the facets of human interaction which contribute to defining it are level of community participation (e.g. newcomer, oldtimer), interaction time (i.e. face-to-face time during the conference, as clocked by the RFID tags), and access to community knowledge (i.e. whether or not the participant was in a workshop where they received information about other participants). Levels of collaboration relates the interactional system, the cognitive system, and for the first time in this manuscript, the social system. The interactional system only deals with a simple component of human interaction : time spent face-to-face. The cognitive system also deals with a simple aspect: the knowledge an individual has about community members.

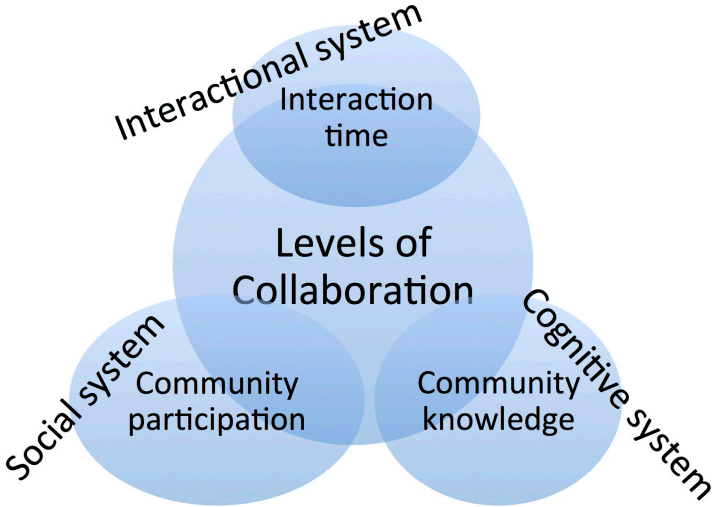


Figure 15. A model of an individual’s level of collaboration within a research community

I class community participation — whether one is an outsider, a peripheral member, an active member, or a core member— as part of a social system in that these are part of the analytic

constructs that allow us to consider how communities of practice can be used to manage knowledge and create value (Wenger, McDermott, & Snyder, 2002). Recall that the social relationship of two people can be described with kinship theory, but can also be used to explain interactional practices (Levinson, 2005). Here, it is levels of collaboration that connect the social system through community participation, the interactional system through interaction time, and the cognitive system through community knowledge. Each of these facets of human interaction: community participation, interaction time, and community knowledge influence the level of collaboration that will exist between two people.

In this paper, we don't focus on the linguistic system, but in newer RFID work, there is also a way to audio and/or video record the interaction and not just the face-to-face interaction time (Thomas, 2013), paving the way for linguistic analyses.

9.6.4 Dynamic model of an individual's level of collaboration within a research community

The dynamic model in this article (proposed as a hypothesis at this point) is similar to conceptual change occurring in physics, to the gain in the pragmatic competence of managing human interaction in language development, and to the building of an overall emotional framing of a debate. It is similar in that there is change in the quality of the intermediate variable (here, levels of collaboration), and as expressed by its facets, as time progresses.

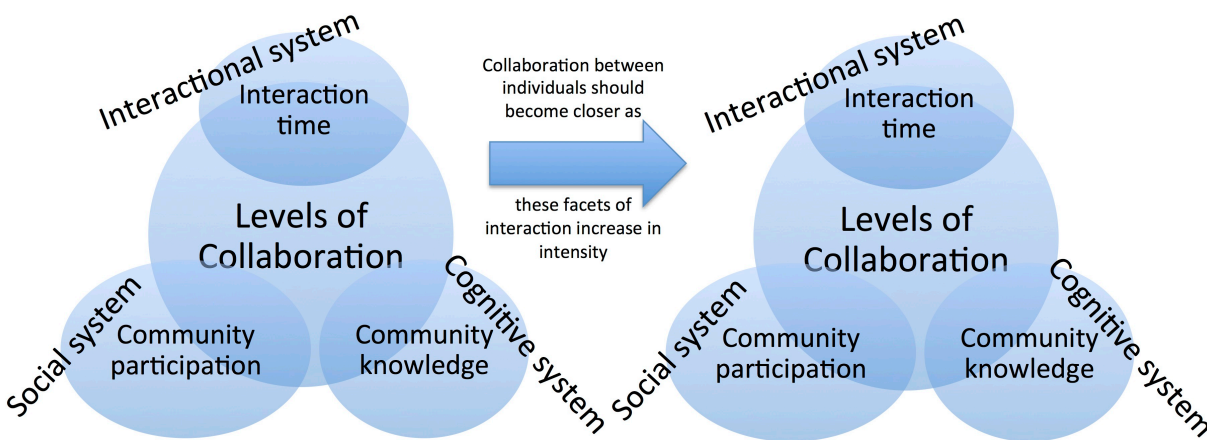


Figure 16. A dynamic model of an individual's level of collaboration within a research community

The basic social network factors (social system) we tracked were participation status, amount of previous collaboration and with whom, gender, language spoken, discipline, reciprocated intentions of collaboration, actual collaborative projects between participants. Finally, we recorded the duration of face-to-face interactions during the ARV conferences (interactional system). In the overarching argument of this HDR — instantiated in different contexts — each of the systems will mutually influence each other ways that need to be determined. In the work reviewed in this section, this hypothesis is instantiated in the follows ways. First, the more knowledge a researcher has about other researchers (in a community of practice), the more likely it is they will collaborate, and the more one collaborates with others, the more information one learns about them. In addition, the more one is an active member of a community of practice, the more one will collaborate with others and the more one collaborates with others, the more one becomes an active member. Lastly, the more one interacts face-to-face with others, the more one

will establish new collaborations, and the more one establishes new collaborations, the more one will interact face-to-face with them (in the context of the conferences).

In our ongoing work, however, not all of these relationships between facets have been illustrated by our data (Eberle, Stegmann, Barrat, Fischer & Lund, 2015). We found two kinds of effects over data from 2011 and 2013. First, only certain workshops had an effect on the number of face-to-face interaction partners any one participant had. This led us to hypothesize that design of workshop structure was important for provoking interactions. Second, the number of previous collaboration partners as well as transitive triplets⁵¹ affected new intended collaborations. In other words, you have to first know people who know people, then, you need to get introduced to new acquaintances and finally, interact with them. On the other hand, we did not find any differences between newcomers and active members: both are chosen just as often as collaborative partners and both choose all kinds of collaborative partners. Nor did we find differences between disciplines: no discipline treats another discipline (social sciences versus computer science) in a preferential manner in regard to choice of collaborative partner. Finally, contrary to when we analyzed the 2011 data alone, providing knowledge about potential new collaborative partners' research had no effect on collaboration partner choice nor did it affect face-to-face interaction with others. These results show unidirectional influences and we would need to employ different methods to capture complex relations that are potentially in play between the systems.

9.6.5 Researcher agency in using an experimental method to study a a sociocultural phenomenon

Recall that Ageyev (2003) made fun of how the experimental method was not suited to Vygotsky's sociocultural vision for studying learning and development – e.g. samples were small, no attempt was made to control the independent variables, and no statistics were calculated. Yet Vygotsky did use the experimental developmental method in which developmental changes are provoked in laboratory settings (Vygotsky, 1986; Kozulin, Gindis, Ageyev, & Miller, 2003; John-Steiner & Mahn, 1996). Let us briefly revisit the differences between viewing learning from behaviorist and cognitivist perspectives as compared to viewing learning from sociocognitive and sociocultural perspectives. As I have argued, in the first two, learning is viewed as a change in an individual's disposition to carry out a behavior or perform a mental act. In the latter two, the focus is more on the process that leads to learning, and in the sociocultural approach, the process is studied within its historico-developmental context. Vygotsky's focus was to capture the process of cognitive change in diverse contexts (John-Steiner & Mahn, 1996, p. 195):

“Through intervention, the experimenter is able to record participants' initial efforts to solve a problem beyond their existing means or strategies. One of the intervention methods was providing auxiliary means through which the problem could be solved. This type of mediated assistance was of theoretical and methodological interest to Vygotsky. In studying memory in complex choice responses, he focused on the developmental changes taking place in the course of one or several sessions during

⁵¹ A significant transitive triplet effect means that it can be shown that two people (who did not previously collaborate) will collaborate based on the number of shared collaboration partners they have had. The more prior collaboration partners each have had who could do introductions between them, the more likely it is that they will collaborate (Snijders, van de Bunt, & Steglich, 2010).

which the learner appropriated new psychological tools (Vygotsky, 1978, p. 194-195).

In Eberle, Stegmann, Lund, Barat, Sailer, & Fisher (2013), we also proposed an intervention as a mediated assistance to help newcomers integrate into a research community. Our intervention was giving extra knowledge about participants' research. We hypothesized that this knowledge could help newcomers to establish new collaborations and it is this on-going process that helps newcomers to potentially transition through peripheral, active, and finally core membership in a community of practice.

I argue that our chosen methodological approach is not incommensurable in studying a phenomenon that is seen as sociocultural, first if we accept analytic dualism, and second if we focus our analytic lens on the process. Having lacked in this project the ability to audio and video record the human interactions, we were not focused on how language mediated human activity, even if the face-to-face duration of human interaction is part of the mediating symbol system of language. Level of participation, both in terms of membership status and new collaborations are certainly indicators of the process of acculturation within a community of practice. In order to gain a longitudinal perspective by following a subset of researchers over time, in current work, we are analyzing both of the Alpine Rendez-Vous for which we took data: 2011 and 2013. With that in mind, we have not yet taken position in relation to which view of causality we will ultimately adopt: network-centric or attribute-centric. Do people establish collaborations together because they are already connected or because they have similar attributes?

9.6.6 Explanations that cross levels of analysis

In the previous section, in relation to constructing an overall emotional framing on the debate, I argued that some levels of analysis were embedded in other levels of analysis. For example, linguistic markers (linguistic system) allowed us to qualify facets of both the cognitive and interactional systems. The model in this section is more similar to the models concerning the semiotic bundle and the finalized procedural explanations as intermediate variables. In other words, level of collaboration — as an intermediate variable — connects the social, interactional, and cognitive systems more simply, in that some facet within each of these systems contributes to changing a participant's level of collaboration. It remains to be evaluated with either current or new data, how the change in a level of collaboration could influence some facet within each system.

9.6.7 Methods of investigation for connecting levels of analysis

In all of the previous sections, the intermediate variable corresponded to a mechanism: the creation of the semiotic bundle, building finalized procedural explanations, and constructing an overall emotional framing of the debate. In this section, the mechanism can be qualified as achieving a level of collaborative relationship. In the models concerning the creation of the semiotic bundle and building finalized procedural explanations, it was fairly straightforward to see how the facets of human interaction co-constructed each other as time progressed (i.e. were mutually influential) as the focus was on describing in detail the interactional processes between learners participating in the respective, finalized task. In the model on constructing an overall emotional framing of the debate, linguistic markers illustrated how both interactional and cognitive processes were mutually influential. On the other hand, the study of achieving a particular level of collaborative relationship was only focused on a detailed description of one

aspect of interactional processes and so if a mutually influential relationship is to be uncovered between facets belonging to different systems (cognitive, linguistic, interactional, & social) then perhaps a variation theory approach is better adapted than a process theory approach. Alternatively, we can change the nature of the data we gather.

9.7 A MULTI-theoretical and Interdisciplinary model of GRoup And INDividual (“Multi-grain”) knowledge building and a model of the process that leads to it

In his book *Monde Pluriel* (A Plural World), The French sociologist Bernard Lahire argues that although it is true that the diversity in the human and social sciences has part of its origin in the way that researchers construct their objects of study, this is not the only reason for the scattered and dissipated nature of the research in this sector. It also is a result of the social division of scientific work into disciplines (e.g. the sciences of “language”, “psyche” or “society”) and further into specialties within disciplines. Such a division means that researchers of different ilk separately study each domain of practice or sector of social life and form parallel theories of the actor. Lahire asks three questions that emanate from this state of affairs (Lahire, 2012, p. 11):

- How can we obtain a global view of the social world if each researcher must keep his or her nose glued to the functioning of his or her small world parcel?
- How can we conserve a complex conception of individuals in society when disciplinary boundaries and within those, internal specialties constrain researchers to work on the dimensions that are particular to narrow practices?
- How can we maintain a high level of scientific creativity when a narrow vision of professional research leads to hyper specialization and a normalization of research and researchers?

When Lahire asks — rhetorically — if it’s possible to understand the invention of the economic market without taking into account how economy relates to law, religion, politics and culture, I take a similar stance and ask how it’s possible to obtain a broad understand of knowledge co-construction while considering only one specific discipline. These are approaches that take the stance of interdisciplinarity as types of integration between separate disciplines (Kline, 1990).

In his own academic context centered in sociology, Lahire’s goal is to obtain a global view of the social world and in order to do so, he asks the following question: why do individuals do what they do, think what they think, feel what they feel and say what they say? He works to answer this question by attempting at the origin, a combination of different research foci in sociology — those focused on actors’ inherent proprieties and those who focused on context. He uses a “formula” to describe his approach, evolved from a criticism he makes of the one proposed by Bourdieu (1979) — habitus + field = practices:

Incorporated past of the actor (dispositions or competencies) + context of the present action = observable practices

This formula underlies the abstract model I propose below. Each of the practices my colleagues and I observed were the result of both dispositions and competencies and the context of the action participants carried out their activities. The elements of this formula are found in various forms in the wider literature that treats the co-construction of knowledge.

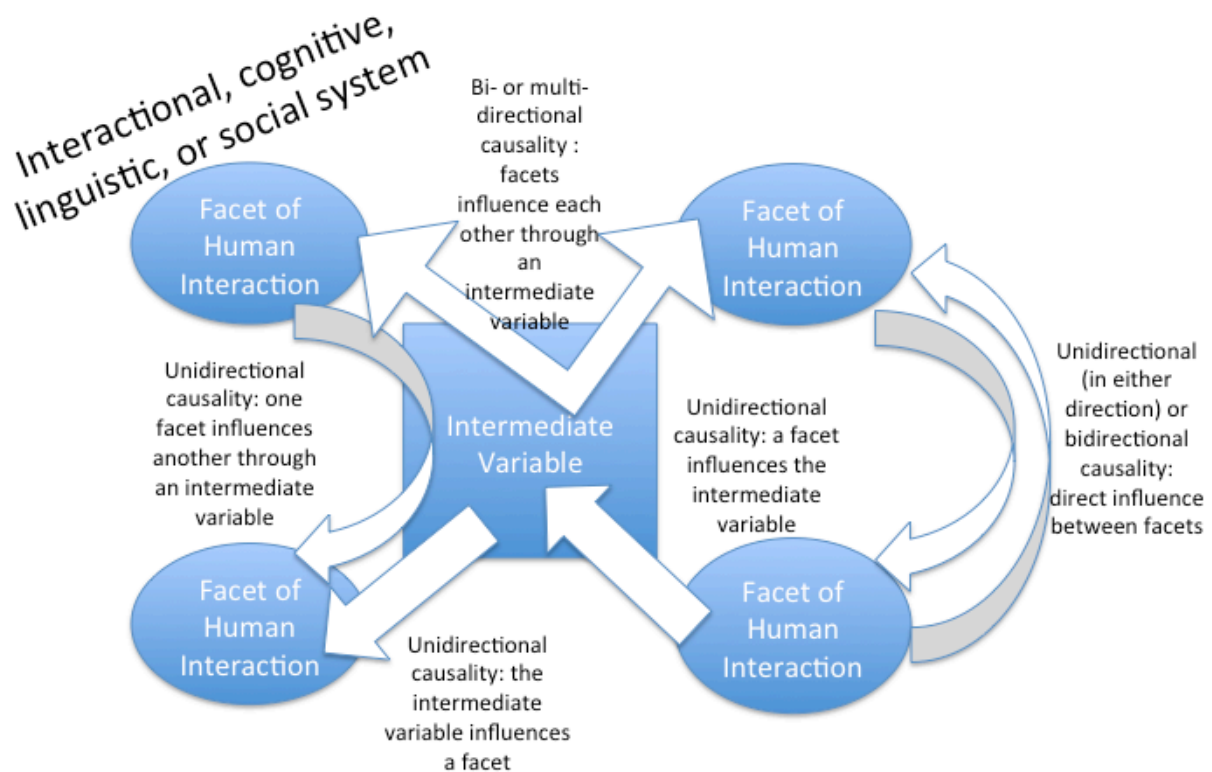


Figure 17. An abstract model that connects systems to facets

As stated, each of the articles I have reviewed in this HDR has served to build part of this abstract model. At the center is the intermediate variable. The examples of intermediate variables I have discussed are semiotic bundle, procedural explanation, overall emotional framing of a debate, and level of collaboration. In the spirit of the realist approach, I argue that these intermediate variables are also simultaneously mechanisms that account for the makeup, behavior and interrelationship of those processes or facets of human interaction which are responsible for the effect, be it a regularity or not. Higher-level effects are defined in the following ways: 1) the semiotic bundle can illustrate conceptual change in physics, 2) the procedural explanation can illustrate changing competence (both cognitive and interactional-pragmatic) as children develop their language, 3) overall emotional framing of a debate can illustrate the group's argumentative complexity and degree of dialogism, as it is constructed over time and 4) level of collaboration can illustrate the trajectory of a participant as she becomes a more active member of a community of practice.

The accounting is carried out through a study of the facets of human interaction. This can be approached with diverse methodological approaches, using variation theory, process theory or approaches within complexity sciences. The facets used to build this model have been modes of expression such as talk and gesture, drawing, or manipulation of experimental apparatus, choice of argumentative claim, emotional positioning of that claim, self-identity footing, group talk type, schematization, tonality of discourse objects, facework, subjects' expressed feelings, community knowledge, community participation, and duration of face-to-face interaction. What I propose is an open model that can accommodate other intermediate variables and other facets.

Depending on the context, the facets I reviewed exhibited one of the possible relations represented with the arrows in the figure above, and as a result bring into play different systems. In the first three models (i.e. physics, explanation, and emotion in debate), bi-directional causality exists between talk and gesture (linguistic system), be it within the individual as a message is being conceptualized or when an individual interacts with others as talk unfolds. Multi-directional causality exists between the modes of expression (cognitive, interactional, and linguistic systems) of the semiotic bundle as students reformulate physics notions from one mode to another and back again, cycling through different modes as the lesson progresses. In interactive finalized procedural explanation, the bi-directional causality of the co-construction of talk and gesture, be it individual or within interaction, is analyzed from three system perspectives (linguistic, cognitive, and interactional) and we track the develop of linguistic, cognitive and interactional competencies in three different age groups. In the overall emotional framing of a debate, two facets mutually influence each other in a feedback loop within interaction (i.e. choice of argumentative claim with construction of an emotional positioning of that claim — feedback loop 1, and self-identity footing with group talk type — feedback loop 2). The individual gives a tonality of discourse object that is in bi-directional causality with feedback loop 1 (cognitive system) and the individual expresses feelings in the interaction that is in bi-directional causality with feedback loop 2. In the work on achieving a level of collaboration in a community of participation, we postulated bi-directional causality between this intermediate variable and facets belong to the interactional system (face-to-face time), the cognitive system (community knowledge) and the social system (community participation), but not all were borne out. The temporal character of this model is illustrated in the arrows that accompany the different forms of causality. These arrows show the influence that depends upon the progression of time. Crucially, both Humean and realism view of causality are possible under this model.

The model I propose is billed as a model of group and individual knowledge building. How have each of the five models added to this understanding? What does the final abstract model offer? I answer these questions below.

The semiotic bundle was first used as an analytic construct for describing level of expertise in physics knowledge and the differences between novices and experts in terms of their competencies (Lund & Bécu-Robinault, 2010). Then we mobilized it for tracking both individual knowledge and group knowledge during the course of a class on electricity (Lund & Bécu-Robinault, 2013). The reformulations that we tracked between modes of expression/facets of human interaction illustrated how an individual's conceptual change of physics was a result of what the group did. The reverse was also true: the individuals influenced the group.

In Mazur-Palandre, Colletta, & Lund (2014), we understood through the analysis of young children's talk and gesture that they had difficulties in managing the interaction with their peer when they gave an instructional explanation for a finalized task. For example, they rarely asked if their interlocutor understood the explanation, if he or she was paying attention, or if he or she had any questions. In the current, continuing work within the GeDÉCo project, we hope to show how children develop the pragmatic competence of interaction management by analyzing the talk and gesture of three age groups for instructional explanations in a new finalized task. If all goes well, we will be able to show how talk and gesture work together to form a message that a speaker conveys, but also perhaps how through an interaction, talk and gesture may even stimulate children to make progress in their pragmatic competence of interaction management.

In Polo, Lund, Plantin, & Niccolai, (forthcoming), the individual and the group are mutually influencing each other through all of the systems that we bring into play. In the cognitive system, the individual schematizes the tonality of discourse objects, but the emotional positioning that is constructed with the group also influences the tonality of discourse objects. In the interactional system, facework influences which of the subject's feelings will be expressed in the group, and what happens in the group retroacts with facework. In addition, subject's expressed feelings are also influenced by the way in which self-identity footing and group talk type merge to create group talk in general. At the same time, a subject's expressed feelings influence an individual's self-identity footing in the group as well as what kind of group talk type he or she will strive for. All of these interactions contribute to the overall emotional framing of the debate.

In Eberle, Stegmann, Lund, Barat, Sailer, & Fisher (2013), we hoped to show that the individual and the group mutually influenced each other in order to change the level of collaboration of an individual in the community of practice. Certain forms of workshop organization (and thus group) provoked face-to-face interactions with others. And if a person knew many others (a property of the individual?) who were well connected, then it was more likely the person developed new collaborations. More analyses are under way and it is clear that these questions could benefit from more data collection.

Both intermediate variables and the facets that compose them belong to one or more systems. Interacting facets can belong to the same system, such as the case of talk and gesture belonging both to the linguistic system and to the interactional system. I have argued that systems are defined in terms of analytic point of view and in light of the interdisciplinary nature of this HDR where I work to cross boundaries, I propose a second model of the researcher process in interdisciplinary contexts⁵² that explains how I arrive at the model above. In it, systems are conceptualized (i.e. interactional, social, cognitive, linguistic) as a function of researcher focus.

⁵² I am grateful to Christian Plantin and to Claire Polo for discussion on this topic.

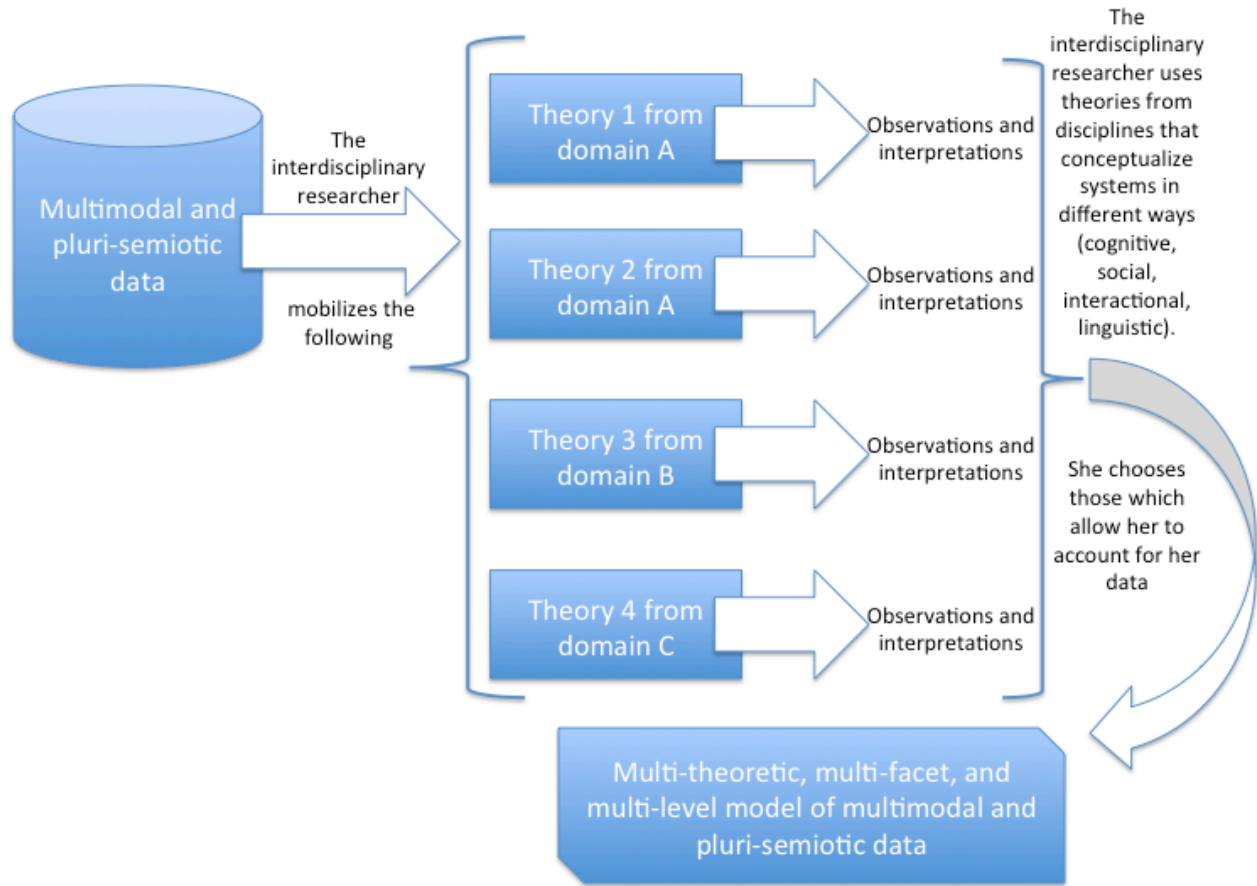


Figure 18. An abstract model of the interdisciplinary process

The interdisciplinary researcher mobilizes multiple theories that come from different disciplines, according to analytic need. Each theory allows for observations that are oriented in particular ways and methods of interpretation that are based on specific assumptions. It is important to note that research questions drive the choice of theories and methods. For example in Polo, Lund, Plantin & Nicolai (forthcoming), we mobilized theories on cognition, language, interaction, and emotion because we needed them to understand the phenomena we were interested in. Contrary to some disciplinary researchers, the interdisciplinary researcher does not begin with a theoretical framework and its associated methods and formulate research questions that are only askable within that context.

Now that I have proposed both an abstract interdisciplinary model of group and individual knowledge building that is multi-theoretical, multi-facet, and multi-level and a model of the interdisciplinary process that allows such a model to be constructed, there are two main questions I should answer.

9.7.1 Uses of the “Multi-grain” knowledge building model

First, what kind of findings can be generated with this model that previous studies were unable to produce? I intended this model to be predictive in certain cases, and a mere “thinking tool” in others. The five different collaborative articles I reviewed here (Lund & Bécu-Robinault, (2010); Lund & Bécu-Robinault, 2013; Mazur-Palandre, Colletta, & Lund (2014); Polo, Lund, Plantin & Nicolai (forthcoming); and Eberle, Stegmann, Lund, Barat, Sailer, & Fisher (2013) all illustrate

an intermediate variable, seen as a mechanism that gives rise to a change, which is one of the broadest ways to define learning. The construction of the semiotic bundle gives rise to conceptual change in physics, but conceptual change also drives our qualification of it. The procedural explanation gives rise to a gain in pragmatic competence of interaction management as a child ages and the quality of the procedural explanation also changes as a result. The construction of an overall emotional framing of a debate may give rise to many types of conceptual change: cognitive changes concerning the topic of debate, competence in giving a tonality to discourse objects in order to be more convincing, and better understanding how a self-identity footing interacts with a group talk type. Finally, a change in the level of collaboration reflects how a participant becomes a more or a less active member of the community. The participant may become cognizant of this rise and/or fall in type of community membership and reassess whether it is in line with his or her goals, adapting a metacognitive stance.

In terms of ability for a model to predict, perhaps only our current work with Mazur-Palandre & Colletta and Eberle, Stegmann, Lund, Barat, Sailer, & Fisher (2013) will have succeeded, at least at first glance. Both use a variance theory approach and this is coherent with a Humean view of causality. If we can show age differences in pragmatic competence for managing interactions, then we can predict them for other groups. In community participation, we can predict that to get new collaborations, you need to become colleagues with well-connected community members. However, in the work on conceptual change of physics and on the varied competencies involved in constructing an overall emotional framing of the debate (both in a process theory framework), we have illustrated at a very detailed level how the individual and group relate to each other. The question of doing what it takes to *provoke* such conceptual change is another issue. In fact, the same question can be asked for gaining pragmatic competence and getting better connected in a community of practice. First, we could image curricula that help young students to gain pragmatic competence or an ability to build emotional framing of an argumentative claim and second, as we have seen, perhaps particular forms of workshops that enhance interactions with others can help with meeting well-connected community members.

In sum, I propose a way in which to link together what currently exists, in the form of intermediate variables that are composed of facets of human interaction, each within a system that is defined by the theoretical and methodological choices of the researcher. I have shown that this model can accommodate different paradigmatic approaches, thus paving the way to unmask false scientific oppositions. My model gives a framework that allows systems within different disciplines to “speak” to each other. I argue that is both phenomenally adequate for the contexts in which I have developed it, but it is also (partially) explanatory. As Craver (2006, p. 358) puts it:

“For now, I suggest an instrumentalist defense: Explanatory models are much more useful than merely phenomenal models for the purposes of control and manipulation. As Woodward (2003) argues, explanations afford the ability to say not merely how the system in fact behaves, but to say how it will behave under a variety of interventions (Woodward says to answer more “what-if-things-had-been-different” questions, or w-questions). Deeper explanations show how the system would behave under a wider range of interventions than do phenomenal models, and so they can be used to answer more w-questions. Because phenomenal models summarize the phenomenon to be explained, they typically allow one to answer some w-questions. But an explanation shows why the relations are as they are in the phenomenal model,

and so reveals conditions under which those relations might change or fail to hold altogether. In that case, explanations outperform models that are merely phenomenally adequate because they cover a wider range of possible contingencies, afford a greater possibility of control over the phenomenon, and so allow one to answer a greater range of questions about how the phenomenon is dependent on various background conditions and underlying conditions” Craver (2006, p. 358).

In each of the collaborative articles I review here, we give detailed descriptions of how the “system” behaves in interactions that are natural for their participants, but in which we have intervened (e.g. give children a collaborative on-line interface to use for learning physics in class, give children a game to play at school and monitor a three-phase procedure, have children participate in carefully organized after-school socio-scientific debates, and give (or don’t give) information about potential collaborators at a conference, while tracking face-to-face time. We have some answers, but we have not covered all contingencies.

The two heuristical techniques of decomposition and localization (Bechtel & Richardson, 2010) that I have already discussed are interesting for continuing this work. Craver has also worked to this end (Craver and Darden, 2001) and argues that:

“...the construction of mechanism schemata typically proceeds gradually and piecemeal by revealing constraints on the mechanism, constraints from the behavior of the mechanism, the available entities and activities for the mechanism, and features of their active, spatial, temporal, and hierarchical organization. Finding such empirical constraints prunes the space of plausible mechanisms and often suggests potentially fruitful avenues for further research” (Craver, 2008, p. 72).

I propose, for example that these methods will prove useful for better understanding the complex *feedback* processes present in the model on the cognitive and interactional functions of emotion as well as the *emergence* of cognitive and interactional competencies in procedural explanations as children’s language develops. The model is general enough to frame other feedback and emergence processes involving other intermediate variables and other facets of human interaction.

9.7.2 Uses of the interdisciplinary process model

In a way parallel to how Lahire views sociology, others have pinpointed the specialized and isolated nature of researchers working in the domain of learning sciences. For example, Lemke (1999) observes:

“We cannot account for the dynamical, self-organizing, and emergent character of spontaneous social interaction and activity if our data, or our focus on the data, artificially dismembers the unity of meaningful action into what our various semiotic analyses (linguistic, kinesic, graphical, etc.) have evolved to describe separately. If we separate, it should only be in order to more richly reconnect” (Lemke, 1999, p. 183).

The interdisciplinary process model is a way to understand how such “richly connecting” research can be carried out. It can be used for training interdisciplinary researchers and as a “thinking tool” when putting together an interdisciplinary project.

10 Perspectives for future research

“Academic disciplines are made, not found. They are socially constructed, just like ideas, organizations, identities or relationships... Like other social constructs, disciplines have become reified, such that social actors forget their responsibility as creators, perceiving what they themselves have made as solid and unchanging...” (Leeds-Hurwitz, 2012, p. 1).

I have proposed an interdisciplinary, multi-theoretical “Multi-grain” abstract model for the relations between the individual and the group concerning the co-construction of knowledge. This model allows researchers to connect cognitive, linguistic, interactional, and social systems through the use of intermediate variables that are composed of facets of human interaction. The model was based on my own interdisciplinary work within educational sciences and language sciences, and more specifically in physics didactics, educational psychology, pedagogical debate, argumentation, interactional linguistics, and psycholinguistics. I have also proposed an interdisciplinary process model that simply shows how researchers mobilize theories from different domains in order to address research questions that are not answerable from one disciplinary perspective.

The “Multi-grain” model is meant to be an abstract framework in which other facets of human interaction can be explored through other intermediate variables, whatever these facets and variables may be. In addition, researchers interested in the contexts that served to develop the model can use it to support further examination of more focused research questions.

In addition to continuing the work in the collaborative projects I have reviewed, I plan on using one of my current projects — EducMap — to examine in more detail new areas where it would be useful to connect work between disciplines, and I describe how, below.

10.1 Decompartementalize the research in education in order to offer new scientific opportunities

Research in education is multidisciplinary. Work focuses on a wide variety of subjects, using many theoretical and methodological approaches. Although this diversity leads to a productive and dynamic domain, I argue that it also creates missed opportunities (Suthers, Lund, Rosé, Teplovs et Law, 2014). A global vision of research in education would allow us to identify and to respond to these missed opportunities. Scientometric approaches can help us to obtain this global vision. This has been the objective of the EducMap project (Lund, Jeong, Grauwijn, Jensen, 2015), financed by the CNRS. The EducMap project coincides with a time when more importance is being given to interdisciplinary research in education. In January, 2016, the *Laboratoire de l'éducation* (UMS LLE) was created, under the tutelage of the CNRS and the *Ecole Normale Supérieure de Lyon*. The LLE is an interdisciplinary nursery supported by five partners: the French Institute of Education (IFÉ) and the research laboratories ICAR, LARHRA, Centre Max Weber et Triangle. These structures study language sciences, disciplinary and professional didactics, history, sociology, and political science. In addition, the LLE is establishing collaborations with researchers in computer science and cognitive science.

10.2 Produce a cartography of the research fields in education

Scientometric analyses are on the rise. They have been used to develop cartographies of scientific institutions in order to understand their productivity, the sub-domains in which they publish, their

subjects of interest, their international collaborations (Grauwin & Jensen, 2011), as well as their level of interdisciplinarity (Jensen & Lutkovskaya, 2014) defined as the degree of integration between disciplines (Wagner, et. al, 2011).

Research in education puts into place forms of integration for participating disciplines, but there is a lack of communication between sub-domains. Using the method of bibliometric coupling (cf. Lund, Jeong, Grauwin, Jensen 2015), Figure 19 shows the principal sub-domains of research in education, as they appear in the Scopus database⁵³ (with the caveat that database is incomplete and favors research written in English). Table 1 presents a list of clusters, sorted according to their size, as a result of the bibliometric coupling algorithm.

Each cluster has its own set of core references that correspond to the references that are the most cited by the journals of that cluster. For example, the references the most cited in the Learning cluster are Lave et Wenger (1991), Vygotsky, (1978), Brown, Collins, et Duguid (1989), Wenger (1998), et Rogoff (1990). These are theoretical references within the sociocultural paradigm where there is an interdependence between the self and the other (person, group, community, society) and the accent is put on symbolic mediation through cultural objects (Glaveanu, 2011).

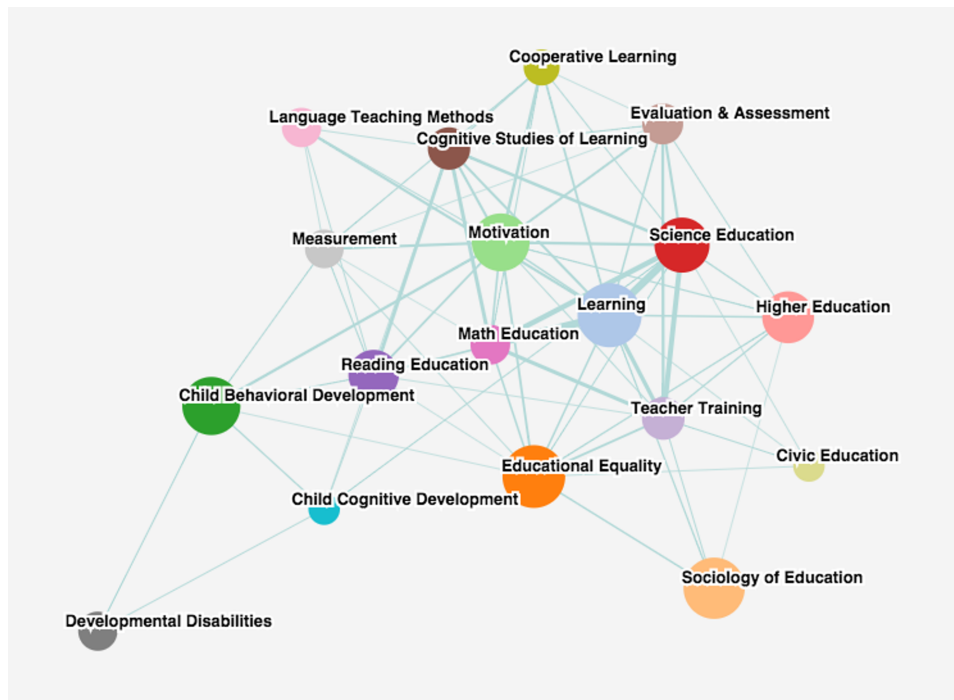


Figure 19. A cartography of the 18 principle clusters of research subjects, according to Scopus 2000-2004.

The thickness of a line between two nodes of Figure 19 shows their connectivity proportionally to the number of references that each cluster shares. Note that the link between clusters mostly stems from the common references that are not in the core references of the clusters (i.e. the 20 most cited references). That said, two references Vygotski, 1978; Lave & Wenger, 1991) are shared by all the clusters and are the two most cited of the Learning cluster for

⁵³ We now have 15 years of data, up until the end of 2014.

the 2000-2004 period. This result pleads in favor of theoretical core references being shared in education, but we need to carry out qualitative analyses on the ways in which these authors are cited in order to validate this hypothesis.

Table 4. The 18 principle bibliographic clusters of research in education

Clusters triés par taille	N	Clusters triés par taille	N
Learning	1,883	Cognitive Studies of Learning	790
Educational Equality	1,800	Evaluation & Assessment	733
Sociology of Education	1,715	Math Education	685
Child Behavioral Development	1,534	Language Teaching Methods	675
Motivation	1,514	Developmental Disabilities	667
Science Education	1,370	Measurement	648
Higher Education	1,207	Cooperative Learning	554
Reading Education	1,140	Civic Education	417
Teacher Training	799	Child Cognitive Development	415

Our analysis has shown that certain clusters: *Learning*, *Motivation*, *Science Education*, *Math Education*, and *Teacher Training* are more connected between each other than *Language Teaching Methods*, *Sociology of Education*, *Child Cognitive Development*, *Civic Education*, and *Developmental Disabilities* which are less linked to other clusters. These five last clusters are more heterogenous in the references that they share. This illustrates their focus on subjects linked to research on education, but also to research in other domains.

10.3 Identify missed oportunities

A first time of missed opportunity is the absence of communication between sub-domains. Scientometric results show the different ways in which theoretical constructions are used. Scientific analyses are fragile when they are carried out from one point of view. This approach radically limites conclusions. Operationalizing theoretical constructions under different foci makes them more robust.(Rosé et Lund, 2014). EducMap identifies zones where theoretical constructions are operationalized differently. For example, the research that treats personal epistemology, epistemic cognition and development, beliefs, theories and epistemological resources are dispersed in different clusters and do not share the same references, although these constructions could benefit from being compared.

A second type of missed opportunity is due to not understanding that representations of data are already optimized for particular analytical objectives. When a researcher attempts to align represnetations of a common corpus in order to compare analyses, these differences make the comparison difficult (Lund, Suthers, Rosé, et Baker, 2014). In this case, EducMap allows us to detect articles with different representations of similar data. The objective is to develop analytic constructions that are more robust by carrying out comparisons.

A third type of missed opportunity occurs when a field divides in order to pursue specific objectives, but does not maintain contact with the the evolution of the other part of the field. The two domains *Computer Supported Collaborative Work* (CSCW) and *Computer Supported Collaborative Learning* (CSCL) are an example. This division allowed them to concentrate respectively on work and learning. Attempts to re-integrate are recent: CSCL @ work (Goggns, Jahnke, & Wulf, 2013).

These three missed opportunities show that EducMap can help in integrating disciplines.

10.4 Orientations

In the EducMap project, recently funded by the *Laboratoire de l'Education*, we work with experts of the clusters in order to identify how to benefit from the diversity of research in education. With fifteen years of data, the priority is to develop tools of visualization in order to determine how a domain evolves, in terms of theories, methods, supportive technologies, or conceptual constructions. The MULTI-theoretical and Interdisciplinary model of the GRoup And Individual with its proposition of intermediate variables and facets of human interaction give a framework in which to study this evolution, at least in terms of knowledge co-construction.

We plan to use EducMap (soon to be on line at le *Laboratoire de l'Education*) to introduce students to research in education, and to train researchers in interdisciplinarity. We have also begun to work with the French Ministry of Education in order to determine how EducMap could respond to questions concerning educational policy.

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