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Trivialization and Detrivialization in Cancer Research: *Knowing, Unknowing* and *Meta-knowing* (A Discursive Psychology Study)

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in Communication

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DEDICATION

To my wife with love;

To my parents with gratitude;

To my 2 sons *Dhia* and *Mehdi* for their patience;

To my best and exceptional friend *Hmida Chammam*, with all respect.

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"We should treat all the trivial things of life seriously, and all the serious things of life with sincere and studied triviality"

Oscar Wilde (1895)

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1. Introduction

1.1 Background

The field of Communication, which only started to be institutionalized around the 1950's in the US and gradually later in other countries, has long been kept outside the debate about how humans know or learn, perhaps due to the dominance of the long established cognitive paradigm, coupled with a heavy legacy of misconceptions about human communication. Wiseman (2007) traced back the concept of communication in the ancient Greece. He found an abundance of metaphors in the Homeric epics that portrayed communication as "breathing words out of one person's lungs and placing them directly into another's lungs" (Craig & Muller 2007, p. 2). Such a deeply rooted myth is echoed in the contemporary over simplistic views of communication; i.e. the dominance of the transmission model that survived up to the twentieth century (Shannon & Weaver's 1949). Hence, it has been very hard for the Communication discipline to become involved in the question "*what is knowledge and what makes knowing*?" on an equal footing with philosophy and psychology, because knowledge has long been understood to be an individual construct or product (located in brains or bodies) rather than an inter-subjective construction. It is the paradigmatic shift from cognitivism to social constructionism that paved the way for the field of Communication to research on how humans know and to contribute to this broad area of interest. With the advent of the Communication scholarship, a number of Communication scientists became well positioned to contribute to the interdisciplinary study of knowledge and knowing (Kuhn 2002 and 2014; Kuhn & Corman 2003, Kuhn & Porter 2011, Tempest & Starkey 2004; Orr 1996; Collins & Evans 2007; Weick & Roberts 1993 etc.).

Studying knowledge and knowing from *Organizational Communication* perspective is gaining an increasing interest that will not soon fade (Kuhn 2014) perhaps due to the importance of exploring this area contextually, descriptively, interpretively and inductively; rather than from theoretical, prescriptive, positivist and deductive stances. In line with this trend, the current research will think as globally as "*what is knowing?"*, and act as locally as studying a number of *trivialization* practices in a cancer research. I assume that studying the trivialization practices can be an effective strategy to understand aspects of how humans know. Hence, the research theoretical part will

reflect on the legitimacy of the Communication's involvement in the area of *"what is knowing?"* through a survey of the dominant learning theories available in the literature. A corollary aim is to expose our fundamental viewpoint with regard to the inextricable relationship between human *communication* and *knowing*. The empirical part of the research will however go microscopically. We will enter a cancer research lab, as a random choice of a learning organization; we will particularly explore the *trivialization* practices, in order to study their underlying dynamic. I will suggest that studying trivializations is a strategic entry point to explore aspects of knowing. I will use the trivialization practices as key and unique elements to know about knowing. Thus, the ultimate aim of the empirical research is oriented into examining how ideas happen to be trivialized by individuals, groups and organizations, and second to reflect on how studying the trivialization and detrivialization practices from our perspective contributes to a better understanding of the larger area of interest of knowledge and knowing.

1.2 A case study: The Trivialization practices in cancer research

The empirical part of this research will aim at exploring the trivialization practices in a cancer research lab. There will be a need to understand the covert and overt dimensions of this dynamic and how it is related to knowledge and knowing. Therefore, I formulated the main research question as follows: *How can the (de)trivialization practices be useful to know about knowing?* From the main question, I generated 3 corollary questions: (a) what is the role of trivialization in the lab's knowledge construction? (b) What is the role of the detrivialization- triggered by our research- in the lab's knowledge construction? And (c) how can we know about knowing, by examining the (de)trivialization, from our perspective?

1.3 Defining Triviality and Trivialization

The Oxford dictionary defines the term *triviality* as "lack of seriousness or importance; insignificance" ("Triviality", 2015); and the term trivial as "of little value" or importance ("Trivial", 2015). The verb to *trivialize* means to "make (something) seem less important, significant, or complex than it really is (example. *the problem was*

either trivialized or ignored by teachers" ("trivialize", 2015). Etymologically, *trivial* comes from Late Middle English as 'belonging to the *Trivium', from medieval Latin 'trivialis' and from Latin 'trivium'. Trivum was "*An introductory course at a medieval university involving the study of grammar, rhetoric, and logic ("trivium", 2015). According to the same source, the origin of Trivium is a "place where three roads meet', from *tri-* 'three' + *via* 'road' ". In French, *trivial* can mean the same thing as in English, but it can also mean *vulgar* or *rude*: "Qui est d'une crudité choquante, malséante; grossier, vulgaire: Plaisanteries triviales" ("trivialité", 2015). Other dictionaries give another meaning to the term trivial; it is that of "mundane" or "commonplace" (The Free dictionary, 2015; and dictionary 2015).

Our research is speculating that at a cancer research lab, when I (as an outsider) ask questions about ideas or practices that have otherwise been considered to be trivial (i.e. commonplace or mundane), I would trigger an opportunity to rethink their triviality (i.e. insignificance, or of little importance). Hence, such questions would meet Garfinkel's (1967) breaching experiment, as they would make an opportunity to detrivialize. *Detrivialization* is therefore a neologism that I am using to refer to the trivialization reversal. All along this research, to avoid saying *"trivialization and or detrivialization"* all the time, I may substitute the latter expression by the following form, both as a noun or gerund: *(de)trivialization*.

At this level, our use of the term *detrivialization* intricately crosses Jeanneret's special use of the term "trivialité" to mean popularization or vulgarization in most of his works (Jeanneret, 1998, 1999a, 1999b, 2006, 2008). As I will detail in sections 1.4, Jeanneret is interested in how cultural and scientific discourses travel in space and time; i.e. how a specific scientific knowledge (example about volcanoes) would be communicated in a scientific or academic journal and how it would be translated (reshaped or reframed) to fit in a newspaper intended for everyone. From this standpoint, what Jeanneret analyzes is the scientific and cultural knowledge that undergoes a form of vulgarization or popularization, which then makes it mundane and commonplace. In our research, when the lab participants would be explaining to me issues from their own area of expertise, adapting their discourse to make

their knowledge accessible to me (i.e. when they are detrivializing in my sense), they would then be popularizing or vulgarizing their knowledge (i.e. trivializing in Jeanneret's sense). Consequently, our research is inviting the lab's members (the participants) to get their scientific discourse to travel through space and time, by adapting their discourse to me (as outsider). This time, their travel is transcendatal rather than physical, because it happens in situ.

1.4 Triviality across disciplines

In mathematics, triviality means a simple demonstration or proof and trivial is used to describe groups or objects that have simple structures. Conversely, *non trivial* means "a statement or theorem that is not obvious or easy to prove" ("triviality mathematics", 2015). In describing solutions to equations having simple structures, the term *trivial* is used for the sake of completeness and cannot be omitted. Therefore, mathematically, *trivial* means very simple, obvious, but is at the same time indispensable in the demonstration process. The Nobel Prize-winning physicist Richard Feynman stated that "mathematicians can prove only trivial theorems, because every theorem that's proved is trivial" (Feynman 1997, p 84). Similarly, the American mathematician and philosopher Gian-Carlo Rota stated that "the mathematician's ideal of truth is triviality, and the community of mathematicians will not cease its beaver-like work on a newly discovered result until it has shown to everyone's satisfaction that all difficulties in the early proofs were spurious, and only an analytic triviality is to be found at the end of the road" (Rota 2008, p 118). Furthermore, Rota considered that "the ideal of all science, not only of mathematics, is to do away with any kind of synthetic *a posteriori* statement and to leave only analytic trivialities in its wake" (Rota, 2008, p 119). Again, from a mathematical perspective, triviality refers at the same time to the easy, simple and banal on the one hand, and to the indispensable on the other hand. In quantum physics, physicists such as Landau, Abrikosov, and Khalatniko identified a phenomenon, which they called quantum triviality ("quantum triviality", 2015). From his end, Callaway (1988) described triviality saying that "much of the formal work done on triviality involves statements that a given theory is a trivial "generalized free field" if certain preconditions are met". Once again, triviality seems to combine the easy, banal and basic characteristics with the indispensible or unavoidable ones.

From a managerial perspective, (but applying rules and allegories from physics) the naval historian Daniel Northcote Parkinson has developed what is now known as the Parkinson's Law of triviality, where he found that organizations give inconsistent interest to trivial matters (Parkinson, 1958). Parkinson detailed the example of a nuclear plant staff that spent much more time discussing the color of the personnel bike-shed than discussing the design of the nuclear power plant. Obviously, Parkinson's objective was not to understand the human behavior or the underlying interactional dynamic. Rather, his approach was prescriptive with an objective to make judgments based on the financial cost of each of the two subject matters (the nuclear power plant cost £ 10 million, while the bicycle shed only cost £350).

To get close to our discipline, Yves Jeanneret has in the last couple of decades been devoting his research to triviality (Jeanneret, 1998, 1999a, 1999b, 2006, 2008). Jeanneret gives a special meaning to triviality. It is more about knowledge travelling from one space to another and how it gets popularized ("vulgarisé" in French). Jeanneret started from the same etymological sense of triviality, which we explained in the first paragraph of the current subsection ("trivum", 2015). His research is mainly about how science and culture get disseminated, revived, shared and spread across society. Using a semiological approach, Jeanneret is interested in the meaning that affects knowledge, as this latter keeps moving from one space to another. The similarity between our research and Jeanneret's is the fact that we are too examining how a scientific question circulates within a research lab (but not across different social spaces). How it first emerges; how it is received; how it changes over time (synchronically and diachronically) and how it ultimately shapes knowledge building practices within the lab.

1.5 Our epistemological standpoint

Considering that any view about communication is partial, partisan and problematic (Eisenberg, Goodall & Trethewey, 2010), I epistemologically situate myself on the overlapping area of the following 3 interrelated theories: *Social Constructionism*, *Discursive Psychology* and *Communication as Constitutive of Organizations (CCO)*. As a matter of fact, *Discursive Psychology* is both a socio-psychological theory of Communication and a method of discourse analysis that came into existence based on Social Constructionism as a wider theory of knowledge that in turn emerged within the fields of Sociology and Communication. It is not always easy to unpack the main characteristics of *Discursive Psychology* without revealing its philosophical foundations and the assumptions on which it hinges; i.e. *Social Constructionism*. And because our current research is within the scope of Organizational Communication as a subfield, I happen to favor the *CCO* perspective in examining organizational knowledge and knowing. Therefore, this section will unpack my theoretical assumptions and reveal the background foundations, through which I will later approach my case study.

1.5.1 Social Constructionism (SC): our philosophical foundation

Thinking globally, I paradigmatically situate myself in the Social Constructionist arena. *Social Constructionism* or the idea of the *social construction of reality* is a theoretical assumption that I make and that underlies my views of *communication* and *knowing.* SC is a major theoretical breakthrough in the way we conceive of the world or of reality. It is a paradigmatic shift from the idea that reality is by itself a truth that is independent from human interactions with and about it. SC basically views *reality* to be continuously processed, through social interactions. It is through our contextual social interactions that our knowledge about things and the world is shaped. A reality is hence never constant and heavily depends on how we approach it. And likewise, our knowledge is never constant; it changes as long as our social interactions are going on. The theory was born in the overlapping areas of interest of Sociology and Communication. Lock & Strong (2010) traced the origin of the idea of SC, arguing that it goes back to the 18th century's ideas of the Italian philosopher Giambattista Vico.

Perhaps the most controversial issue in SC is related to the actual and factual practices that people use to construct realities. Theoretically, it was relatively easy to state that social interactions shape our realities and in particular our views about them. However, empirically, it is very hard to capture and visualize the very complex social interactions and understand how they shape our views and realities in a moment-to-moment basis. The difficulty stems from the fact that: a) social interactions are constantly on the move and therefore changes are constantly ongoing; b) social interactions travel across space and are happening everywhere; c) What has long been called 'attitude' about things is not only unobservable, but is a notion that is still obscure (Allport, 1935), fuzzy (McGuire, 1985) or simply a myth (Potter & Wetherell 1987). *Discursive Psychology* came up to provide an alternative, which is to stick to discourse (language use) and not to care about attitudes as enduring entities or as underlying cognitive mechanisms. Drawing from Social Constructionism, Discursive Psychology considers *discourse* to be not only a factual and concrete element to study contextual realities, but also a substantial part of the studied reality. Before the two theories were born, 'talk' was considered to be 'only talk' and reality was in the 'action'; presuming that discourse is not an action or less concrete, traceable and describable than actions.

1.5.2 Discursive Psychology: Our approach

Discursive Psychology (DP) is first and foremost a theory that contradicts mainstream psychology, which treats discourse as a reflection of cognitive mechanisms, schemata and the like: "while theory and method in social cognition presume an out-there reality that provides input to cognitive operations, discursive social psychology focuses on the way both "reality" and "mind" are constructed by people conceptually in language, in the course of their execution of various practical tasks" (Potter & Edwards, p. 103). DP as both a theory and a discourse analysis method was first developed by Derek Edwards and Jonathan Potter to argue for the need to consider language as a crucial source of data, in understanding socio-psychological issues. The theory has kept developing by a number of theorists, such as Bethan Benwel, Charles Antaki, Carly Butler, Malcolm Ashmore, Frederick Attenborough, Steve Brown, and more. The theory entails the gist of *Social Constructionism* described in the previous subsection. In Potter & Wetherell (1987), the authors stated that *Discursive Psychology* as a *theory* builds on assumptions from

speech acts, ethnomethodology, and *semiology*, which I will unpack in the remaining part of the present subsection. In section 3.2, however, I will a) unpack the foundations of our discourse analysis as a *method*; b) discuss the differences between *discourse analysis* and *conversational analysis*; and c) compare the *Discursive Psychology Analysis* with Laclau & Mouffe's 1985, Fairclough's 1995 and 2001, and the Foucauldian (Kendall 1999) discourse analyses.

Starting with the most obvious *Speech-Act* theoretical assumptions that DP makes, I will now develop the idea that language not only describes things, but also does them. This is what Potter & Wetherell (1987) call 'doing versus stating' role of language. More particularly, Austin (1962) argued that a speaker with any utterance does simultaneously 3 sorts of things: a) a specific meaning; b) a particular force; and c) effects on the consequences. From this stand point, I understand language to be a tool that people use to get things done. These 3 characteristics of language can be obvious in instances such as Austin's (1962): "I now pronounce you husband and wife", "I nominate John to be president" or "I sentence you to ten years of imprisonment". Through the previous utterances, we clearly see the meaning, the force and the effects that each of them encapsulates. However, other utterances are less obvious, as it is not always easy to determine the force and the effects that they engender, no matter whether the meaning is or not incontestable. From the Speech-Act theoretical stand point, any utterance would have a meaning, a force and a consequence, no matter how complicated, ambiguous, less obvious, indirect and hidden they can be. But, since it is not always easy to determine these 3 characteristics in people's utterances, and because Speech-Act-Theory remains abstract and mostly de-contextualized, DP also uses *Ethnomethodology* to understand language functionality in context, rather than out of context.

In line with the same idea of the language role of *doing* besides *saying*, Ethnomethodology goes beyond Speech-Act-theory, as it studies real life contexts and situations, focusing on the methods people use to make sense of their social life. Garfinkel's (1974) is of particular interest, as it details how language is used in everyday situations, based on real examples and field observations. From an Ethnomehodological stand point, language does

not come pre-packaged or preordained, but is reproduced differently on each occasion. Therefore, people do not passively use language, but actively produce it to make sense of their own contexts and to build their own methods of understanding in their respective social life (Potter & Wetherell, 1987). It is with Garfinkel's (1967) and Garfinkel's (1974) conclusions that the distinction between a fact and an opinion started to be made. Furthermore, Garfinkel made it obvious that opinions, unlike facts, are multiple and they depend on people's perspectives and social contexts. Moreover, people's talk does not only describe things and situations, but always implies their own reflection about them; hence the reflexive feature of talk (Garfinkel, 1974). From his end, Wielder (1974), who studied people's life in a half-way hostel for narcotics felons, was able to detail the multi-formulative and the multi-sequential nature of talk. People formulate ideas differently, according to their own needs; and the different formulations engender different consequences. Through acute examples, Wielder was able to unveil the power and effectiveness of talk, depending on how it was formulated. For Wielder, talk is potent, because it is constitutive; i.e. it effectively shapes reality.

Using the *Semiological* foundations, DP goes beyond Saussure's understanding of sign that is limited to one level of signifier-signified relationship, and adheres to Barthes' second level signification or myth (Barthes, 1972). Barthes' essays on the semantics of trivia (wines, steak and chips, writers abroad, Einstein's brains and striptease) detailed, through concrete examples, that a sign (signifier-signified) in context has a second meaning added to Saussure's out of context meaning. Barthes was also able to provide examples illustrating that the signs' meanings (signified) are not natural; they are not inherent properties of their corresponding signifiers, but are rather arbitrarily and culturally constructed. More interestingly, Barthe's examples illustrate how the "second level of signification'', "depend on the underlying set of conventions and distinctions, which people use for decoding the meaning of actions and events" (Potter & Wetherell, 1987, p 18).

Unlike the *structuralist* and *postructuralist* approaches, *Discursive Psychology* considers discourse to be on the one hand abstract when we speak about it out of context, but on the other hand the only concrete and reliable data

if it is contextualized (i.e. when we have a context, we can best understand our realities through discourse). Furthermore, because humans communicate rhetorically, DP studies how they organize language, focusing on the role of the different interactional practices and the roles people play, in context. Additionally, psychoanalysis can be used to account for the related non verbal and non said aspects of discourse (Potter & Wetherell, 1987). A unique interesting advantage of this theory and method is that it was mostly originated by the researcher Jonathan Potter, who had gained a long and profound research experience within the cognitive psychology arena, before switching to the social constructionist perspective. Since the empirical part of the current research aims at understanding the dynamic of trivialization as a practice to be studied from within the community of cancer research, and because trivialization can mostly be observable in discourse, we found it quite logical to approach this concern from the Discursive Psychology perspective. In other words, this perspective enables us to study discourse methodologically (based on its speech acts' and semiological foundations) and study group and organizational practices contextually (based on its ethnomethodological foundations).

Following Gilbert & Mulkay (1984), Potter & Wetherell (1987) used "the term discourse to cover all forms of spoken interactions, formal and informal and written texts of all kinds" (p. 7). Moreover, Discursive Psychology as a discourse analysis theory and method is oriented towards 'the role' that discourse plays in context. Focusing on the role of discourse in context is a unique substitute for the need to know the informants' motives behind their practices. In other words, instead of asking why the informants say what they say, we would ask what role this or that practice plays in this or that context. Consequently, we get as close as possible from the motives, but without taking the risk of establishing cause-effect relationships between the observed practice and the "guessed" motive.

Unlike Saussurean structuralism, which views *language* as a well established system that humans use to communicate (a toolbox); Discursive Psychology favors the term *discourse* over *language*. Discourse is understood to be a contextual practice and does not reflect a pre-existing reality. Rather, it is discourse that creates reality in context. Hence, in order to understand reality, we need to analyze discourse that is constantly creating and recreating

it. Discourse, in this sense, is the most important locus of data that can enable researchers to interpret or understand how reality is born, maintained and re-enforced, shaped, reshaped and how it is destroyed. By the same token, the empirical part of this research will aim at tracing back how scientific facts are created and especially how they are destroyed- through the trivialization practices- in discourse.

Potter & Wetherell 1987 have on the one hand praised Noam Chomsky's extraordinary effort in introducing language as a major field of study in the 1950's and 1960's, after it has long been neglected in social and human sciences; they nonetheless have introduced a completely different way to approach it. Chomsky's *Generative Grammar* and *Universal Grammar* theories did not only provide a revolutionary and unique description of how language operates, but also suggested that his described linguistic system mirrors a cognitive system, i.e. language use reflects cognition. Unlike Chomsky, who approaches language "as a reflection of an external world or a product underlying mental representation of this external world" (Jorgensen & Phillips 2002, p 107), Potter & Wetherell's *Discursive Psychology* considers language to be "a dynamic form of social practice which shapes the social world including identities, social relations and understandings of the world" (Jorgensen & Philips 2002, p 107).

1.5.3 Communication as Constitutive of Organizations: Our framework

The case study of the current research is within the *Organizational Communication* framework, simply because it studies the dynamic of trivialization within an organization: a cancer research lab. More particularly, the research is in the same vein of thoughts of the theories of *Communication as Constitutive of Organizations* (CCO). By *Communication as Constitutive of Organizations,* it is assumed that organizations cannot be reduced to structures, within which people communicate. Rather, organizations are themselves shaped by our communication practices (Ashcraft, Kuhn, & Cooren, 2009; Putnam & Nicotera, 2008). This entails that an organization is not limited to its physical shape, but goes beyond it to involve the complexity of everyday interactions that are tremendously more ramified and bigger in space and time. A cancer research lab as an organization is by the same token shaped by the communication practices of its members. Therefore, studying the communication practices of the lab members can

also involve studying their (dis)organizing role (Vasquez, 2015; Kuhn & Burk, 2014). A special attention will be given to how knowledge building takes place, assuming that knowledge building is primarily visible in interpersonal interactions. Furthermore, *trivialization* is by definition (see section 1.3) a communication practice. From there, we should explore its eventual potential to shape the organization (the cancer research lab in our case) and more particularly this latter's main activity: *knowledge building*. Thus the theory of Constitutive Communication aligns with the theory of *Discursive Psychology* and shares its philosophical foundations, namely the Social Constructivist tenets, but focuses in particular on organizational communication rather than communication at large. It is through the lenses of CCO that we examine how interpersonal interactions shape the cancer lab that we are studying: i.e. how scientific facts are constructed, how decisions are made etc. In line with CCO, I understand interpersonal, group and inter-organizational interactions to be: contingent (upon previous interactions), consequential (because they will have consequences on the future of the individuals, groups and the organization), contextual (as they depend on prevailing psychological, social factors etc.), emergent (because they are unpredictable and emerge depending on how the interaction is evolving). Consequently, communication is too complex that it is not possible to observe and to determine how it shapes the organization or how it makes its reality. Because communication is happening everywhere within and outside the building of the organization, because it never stops working round the clock, because even when individuals are not interacting physically or directly they keep interacting indirectly, mentally and remotely; tracing back and sketching how interactions shape the organization is virtually impossible.

Fairhurst & Putnam (2004) introduced a unique perspective on CCO; a perspective that perfectly matches the theory of Discursive Psychology detailed in the previous subsection, because they both understand reality to be the product of discourse; i.e. it is through discourse that (organizational) reality is created, negotiated and recreated over and over again. Although, Fairhurst & Putnam (2004) have introduced 3 ways to interpret organizations as discourse constructions (organizations as objects, organizations as grounded in action, and organizations as perpetual states of becoming), we are here interested more on how organizations are 'perpetual states of becoming'. This latter approach focuses on the everyday activities of organizing- the micro levels- and is interested to understand how they contribute

to the formation of the macro levels (shaping the organization), always through discourse use. And because the empirical part of this research is within the framework of organizational communication, this latter perspective, enables us to draw conclusions pertaining to the macro level (the cancer research lab as an organization, its identity, how it is shaped etc.) starting from micro levels (the observed and analyzed everyday interactions).

2. Literature Review

The literature review will be divided into 3 main parts: The first part (section 2.1) will discuss the interrelated concepts of *knowing* and *communicating* in the dominant contemporary *learning theories*. The aim is to provide a discussion on the literature's discrepancy with regard to the relationship between the two concepts, which have ultimately come together. The second part (section 2.2) will introduce a number of social studies that emphasized the inextricable relationship between *knowing* and *communicating*. Finally, the third part (section 2.3) will survey a number of social studies that cross our current research and legitimize the need to study (de)trivialization as a communication practice and relate it to knowing.

2.1 *Communication* throughout *the history of Learning* theories

Throughout the 5 subsections of this section, I wish to emphasize how the concept of learning has all along the 20th century been shifting along the following perspectives: 1) learning as conditioning (behaviorism: where one person can be able to condition another or more and make them learn a new habit); 2) learning as a cognitive structure (cognitivism: where humans are born with a biological predisposition to learning and where underlying mechanisms, cerebral architecture, underlying schemata etc. govern the whole process); 3) learning as a construction (constructivism: where knowledge is no longer reduced to information confined in individual brains and bodies, but is rather a construction and a process); 4) learning as dependent on individual learners (Humanism: where the situated learner is the focus; where feelings play a role along with thinking; and where individuals are understood to have different learning styles, strategies and motivations); 5) learning as an intersubjective construction (social constructionism: where knowledge is rather a conventional collective construction, and is therefore contextual).

2.1.1 Behaviorism (conditioning)

Classical conditioning

Based on the Russian physiologist Ivan Pavlov's most famous experiment, which he had conducted on dogs, Watson (1928) made similar studies on children. Pavlov's (1927) experiment enabled him to find out that animals

learn through what he considered to be stimulus-response psychic reflex. He initially used to feed a dog right after ringing the bell and opening the door. He noticed that the dog's saliva dribbled each time the dog was exposed to food. Then, without exposing the dog to any food or food's smell at all, Pavlov noticed that the dog's saliva continued to dribble only with the bell ringing and or the door opening noise. He concluded that the experiment developed a reflex in dogs, based on 2 elements- the initial stimulus (food) and the response (saliva dribbling). Furthermore, the association between the direct stimulus (food) and the indirect stimulus (bell ringing) was so strong that the dog's saliva dribbled (responded) without the presence of food. This association between 2 events or types of stimuli, made Pavlov to conclude that animals learn by way of associative learning. The experiment was the first to point out that animals' reflexes are conditioned by external stimuli and that the external stimuli conjugated together result in reflexive response by animals.

Watson, the American psychologist, who founded the psychological school of behaviorism, went beyond Pavlov's experiment and extended it to infants. Eventually, behaviorism started to spread and gain popularity among educationalists, which used to see learners as recipients that teachers can shape. Watson (1930) made it as straightforwardly as this: "Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I'll guarantee to take any one at random and train him to become any type of specialist I might select – doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors" (p. 82). Hence, at least in the contemporary period, learning theories started with Watson's translation of Pavlov's dog conditioning experiment into humans.

Operant conditioning

Skinner (1938 and 1950) went beyond classical conditioning that could even happen naturally, and discovered that animals (mostly rats and pigeons) and humans alike can further be conditioned through the consistent combination of rewards and punishments to induce behavior – *the operant conditioning*. Based on the operant conditioning experiments and findings, behaviorist educators relied heavily on drilling to positively reinforce the "right" behavior in children, believing that the more learners practice, the better they learn. Idioms such as "*the*

carrot and the stick" and "*practice makes perfect"* mirror the operant conditioning assumptions and reveal the related teaching practices. In classrooms, behaviorist practitioners used to praise the "right" behavior and reward it, in order to positively reinforce it. Conversely, they used negative reinforcements to respond to the "wrong" behavior, by punishing pupils whenever they did it "wrong".

Clearly, both classical and operant conditioning were extremely simplistic with regard to human faculties and capabilities, and this is true for behaviorism altogether. Although it was due to the behaviorist scientists that the concept of conditioning was discovered, they were not able to account for other elements that interfere in the learning process. A strong debate was always open between the radical behaviorists, who believed that humans are born as empty as "blank slates" and that all they learn is the result of what they are exposed to; and the *nativists* (Kant, Schopenhauer, Fodor, Chomsky, Pinker etc.) , who believed that humans are born with innate capacities. This is usually known as the *nature vs. nurture* debate. The result of this debate is the emergence of less and less extremists on both sides and an increasing recognition that human learning is neither a matter of mere conditioning, nor a mere predisposition and inheritance.

Although the *Shannon & Weaver's* (1949) model was theorized and established in the literature later in time, the prevalence of the *send-message-receive* understanding of human communication was already powerful. *Behaviorism* seems to have ignored that unlike animals, human *responses* to stimuli, which we now consider to be *communication practices*, are clearly unpredictable and hardly controllable. It was only after a couple of decades following its birth that behaviorism started to lose popularity among educationalists, leaving the floor for new learning theories. Today, it is counterintuitive to imagine that a teacher is able to teach anything to anyone, by forcing them to practice over and over again, rewarding the good behavior and punishing the bad one. From my stand point, a teaching-learning situation is a communication situation (a sequence) that is governed by more complex and interrelated factors, which I have already outlined in my epistemological standpoint- section 1.5.

More precisely, in any learning situation, the learner who is interacting with the other interlocutors (no matter who they are and what type of learning style is going on) is behaving in a way that is contingent upon past factors and conditions that differ from other learners'. Consequently, their behaviors are in reality much more complex than a matter of conditioning and are thereby unpredictable. Furthermore, these behaviors are always contextual, emergent and consequential. In any possible and imaginable learning situation, different humans' behaviors are not possible "to condition" in the same way. Moreover, the same person cannot be successfully "conditioned" in the same way at 2 different times. Finally, it is never sure that a "stimulus" presented to any person would trigger a particular response from their part. This is more and more obvious in the democratic educational institutions and systems.

2.1.2 Cognitivism (transmission)

The frustration emanating from the dissatisfaction with the behaviorist learning theories, which underlies over simplistic and sometimes extremist views about human communication, has urged scholars to shift to nearly opposite views: cognitivism. I understand that the emergence of the cognitive theories, which constituted a major paradigmatic shift, followed by the advent of the neurosciences, has caused a decisive turning point in the history of learning theories and the views about communication altogether. I understand this shift to have emanated from the difficulty to explain *human communication practices*- which the behaviorists call "*behaviors*"- because they are unpredictable and infinite. It seems that when scholars were confronted to the complexity and uncontrollability of the human "responses" to a single "stimulus" (in the behaviorists' sense), they happened to resort to the brain, in a positivist fashion, and started theorizing about 'the cognitive processes' assuming they are responsible for human behaviors. They perhaps found it more appropriate and easier to study the brain and the so called *cognitive processes*, and to establish cause-effect relationships between mental states or conditions and behaviors, than to study communication practices.

The obsession with the *black box*, coupled with the inexistence of the scholarship of Communication as a well-established field (which only became a subject of serious discussion in late 1980's, according to Craig & Muller

2007), caused scientists from different disciplines to take the mind theory for granted and not to consider studying (instead of conditioning) behaviors as such. Until today, there is no real evidence that knowledge is located in the brain. There is still a considerable need to understand whether the brain is vital in learning, because it guarantees the functionality, or because it is also the locus of knowledge.

The social learning theory was one that paved the way for cognitive theories, and was considered to be a bridge between Behaviorism and Cognitivism. As early as 1963, Bandura stated that learning is a combination of *attention*, *memory* and *motivation*. The 3 concepts were new in that they oriented the attention to the brain, but Bandura stressed that individuals do not use these faculties independently from others, but by *observing* and *imitating* them, using their behaviors as *models*. Moreover, in order for others to serve as models, Bandura and Walters (1963) and Bandura (1969) suggested the following 4 conditions: *attention*, *retention*, *reproduction* and *motivation*. Interestingly, the 4 conditions can only be achieved collectively and never individually. Attention to others' behaviors obviously requires the presence of the other and the mental capacity of the individual to pay attention. Although retention is described as a cognitive capacity, it also depends on how others presented it to the individual, in order for him or her to be able to retain it. Similarly, for an individual to be able to reproduce what they retained, they need responses from others to ensure that their behavior is socially accepted. Finally, individuals need to be motivated to reproduce what they retained through previous attention to others' behaviors. But again, what makes motivation? It seems that Bandura believed that motivation is both internal i.e. a mental state or decision and external i.e. through others' incentives and various social rewarding.

With *Attribution theory*, Weiner (1986) made it clear that the individual learns alone, through a 3-stage process: first, the individual observes and perceives a behavior. Second, he or she thinks of the behavior to be intentional. And third, an individual attributes the behavior to internal or external causes. For Weiner, what make people learn and achieve results are their ability (physical and mental), the efforts they spend, the task difficulty itself, and finally their luck. Paradoxically, although these elements were introduced by Weiner as cognitive and

internal elements, they are from my standpoint rather social and external: firstly, when an individual observes a behavior, there needs to be a behavior of another person, before he or she can observe it. The perception of the observer is itself contingent upon how the other person behaved. Secondly, the observer would not independently determine if the other's behavior is intentional or not, but would also depend on the hints that the other shows in his or her behavior. And thirdly, the observer would not be able to determine whether the other's behavior was initiated by internal or external causes, independently from the context, in which the other plays a substantial role. I thus understand those 3 faculties to be a shared work that emanates from social interactions.

Furthermore, while Weiner mentioned the factors of luck and difficulty of tasks in any achievement, considering them to be external elements, he considers the ability and the efforts spent by an "individual" to be individual. In contrast, the individual's efforts are not completely independent from others, but contingent upon the interaction with them. The others' (re)actions play a crucial role in making us decide whether we should spend more or less efforts. And because the ability is itself influenced to a great extent by how much efforts we spend, it is also dependent upon the others and can hardly be individual. Thus, even using the elements that Weiner considers to be cognitive demonstrating the fact that learning takes place inside the brain, I refute this view, as these elements can never happen outside interactions, be they face to face or in the individual's imagination.

With *cognitive load theory*, Sweller (1988) and Sweller, Merriënboer, & Paas (1998) introduced the idea that the human brain is governed by a "cognitive architecture" that regulates our learning process. In line with its positivist background the theory views the human brain to be a master, a controller and decider of our thinking abilities. For Sweller (1988) and Sweller et al. (1998), any human brain is equipped with a memory. This memory is biologically tangible and observable within the brain. According to the same scholars, any learning task has an intrinsic cognitive load, an extraneous cognitive load and a germane cognitive load.

To begin with *the intrinsic cognitive load*, although this latter was described as completely independent from cognition, and to be all about the difficulty of the subject matter as such (example: the addition of 5+5), the authors still preferred to call it *"cognitive"* load. Likewise, although the authors described *the extraneous cognitive load* to be about how the information presented to the learner (mentioning the teaching-learning situation in particular) and has nothing to do with the brain structures, they again considered it to be cognitive. Finally, the *germane cognitive load* is about how much load a person devotes to the construction, processing and automation of "schemas". Again, from my view point, it is not clear how the schemas are presented in the brain, what shapes they may have, and how they can be automated. While cognitive load theory has advanced the discussion about what facilitates the memorization and retrieval of information, it failed to explain how and why this memorization is considered to be a cerebral work and a "central executive".

In line with Sweller's views, Mayer & Moreno (1998) Moreno & Mayer (1999) and Mayer (2001) introduced the cognitive theory of multimedia learning. The theory assumes that humans process information through 2 main channels- the auditory and the visual, and that the channels' capacity is limited. More importantly, the theory assumes that the learning process is an active operation of data selection, filtration, organization and integration. I understand the term "*active*" to signal a key advancement in the direction of cognitive theories, because it considers the brain to be computer-like, controlling other devices and maneuvering the processes. But again, although the theory acknowledged the fact that the brain bases its processes on past experiences and information, it did not account for what exactly makes the human mind seem to be active and responsible for the learning operation. Moreover, the theory distinguishes between 3 types of memories: a sensory memory, which collects information and stores them for short term, a working memory that processes information and reorganizes them in the shape of "schemas", and finally the long term memory, which constitutes a warehouse that stores all the information of any type received. Furthermore, the theory assumes that the brain does not interpret information in a discrete way, but selects and organizes them in a rather dynamic pattern, combining auditory information to words and images altogether at once, and assigning meaning to those combined information.

Contrary to both *Cognitive Load Theory* and the *Cognitive Theory of Multimedia Learning*, which have epistemological roots that emanated from the dissatisfaction from the behaviorist views in the 1970's, *Discursive Psychology* theory, which Jonathan Potter and Derek Edwards introduced as late as the 1990s, came to reverse cognitive psychology and focus on the human memory as a mere result of past interactions and social contexts that an individual has experienced. Obviously, *Discursive Psychology* does not ignore the presence of the biological brain, and the fact that it is in a way or another involved in the thinking and learning processes. However, the theory prefers to stay aloof from the premature deduction that the brain is the active organ that controls our behaviors, and favors the idea that the so called cognitive processes are rather "passive", as they result from social interactions (Potter & Wetherell 1987).

Aiming at elaborating an instructional design theory and focusing on the teaching-learning situation, Reigeluth' Elaboration theory (1987, 1992 and 1999) claims that in order for humans to learn there must be a meaningful context, with which they have a minimum degree of familiarity; and where they gradually build knowledge from simple to more complicated. Reigeluth's views marked a new era within the framework of educational research, as it drew particular attention to the learner, unlike previous cognitive theories, which were influenced by the legacy of behaviorism and focused on the teaching practices at the expense of the learning process itself. It also paved the way for subsequent learning styles theories.

Once again, although Elaboration theory assumes that learning is an active process, where the brain is the master, it surprisingly focuses on teaching strategies that are clearly external to the individual. These strategies are: a) motivating, b) analogy building, c) synthesizing and d) summarizing. These strategies themselves seem to align with *social constructionism*, which happens to be epistemologically opposite to Elaboration theory's cognitivist foundations. The elements a through d are today clearly key areas of interest within the discipline of Communication, and more precisely in persuasion studies. From my standpoint, *what motivates people*, *how to establish a convincing*

analogy, what's the best way to synthesize or summarize; are questions that we can only address within contextual interactions, based on emergent and contextual elements rather than theoretical assumptions about the mind.

Finally, Jean Piaget, who had a multidisciplinary background, ranging from epistemology to biology, psychology and physiology, introduced the Stage Theory of Cognitive Development, where he described how children develop their learning abilities through 4 stages until they reach the adult level. Although Piaget's work was a significant advancement to the understanding of how humans embrace knowledge, it nevertheless faced serious criticisms claiming that his suggested learning milestones are only approximations. Interestingly, Piaget's findings with regard to the 4 stages, do not contradict social constructionism and *Discursive Psychology* assumptions. If we take all Piaget's findings and try to understand them from a social constructionist stand point, they can still hold true, but explained differently.

Piaget's 4 stages, mentioned in Barbel & Piaget (1958) are: 1) the Sensorimotor stage (from birth to 24 months); 2) the preoperational stage (from 24 to 48 months); 3) the concrete operations stage (from 7 to 11 years); and 4) the formal operations stage (starts around ages 11 to 15 and up). During the first stage, infants develop a basic understanding of themselves and the environment. Despite the fact that the present model is cognitivist, Piaget (who later in his life shifted of perspective and declared himself "constructivist") made it clear that the first stage is shaped by interactions, and no predisposition or schemata are yet there. During the second stage, Piaget found out that children still need concrete and tangible situations in order to understand a matter around them, and are not yet ready to grasp abstract ideas. Again, Piaget admits that the interaction with environment is the only source of knowledge and that there is no sign of any congenital or innate capability in the mind.

During the third stage, according to Piaget, children create logical schemas through which they make sense of the situations they go through. Piaget names the progress that children make at this age "schemas" that they build inside, although there is no hint of any brainy predisposition to such ability. What Piaget calls schema, considering it

to be an internal faculty is understood within social constructionism to be first and foremost external- i.e. the result of the previous social interactions (although they can later be internalized). Again without accounting for what exactly happens in the brain, Piaget's model suggests that it is due to the development of the so-called "cognitive schema" that children become like adults, think abstractly, and make hypotheses and deductions, during the fourth stage.

2.1.3 Constructivism (construction)

With the advent of Constructivism, human knowledge started to be considered a construction rather than an out-there product or reality waiting for us to discover. Knowledge that humans build can be different from the real world (if only the world has one reality), and is not transmitted (by conditioning or from one mind to another), but is constructed internally. No matter what exists around us and what the object is, our knowledge is the product of our own perceptions and interactions. Hence, human knowledge started to be described rather as an invention than a discovery of an existing truth. Along with Constructivism, philosophers started to speak about the inter-subjective construction instead of objective reality (Raskin, 2002). They also substituted the idea of truth with the idea of viability (Jonassen 1991); because there is no truth out there, but there can be a viable idea or understanding that we can conventionally share and use to communicate.

Although the *Cognitive Apprenticeship* theory is so termed, it is obviously considered to be constructionist, given the fact that it describes the process of learning as a social practice. The theory describes the learning process in a deliberate teaching-learning situation, based on views about how humans integrate knowledge. For Collins, Brown & Newman (1987), humans learn through modeling, coaching, scaffolding, articulation, reflection and exploration. The 6-stage process of learning is clearly a communication situation or practice; it is an interaction situation and needs to be studied as such. Through this theory, I wish to provoke the question of why the *teaching-learning* has long been considered to be an area of interest that is outside the Communication discipline. Why a Communication scholar would be considered an intruder in the teaching-learning area of interest? From my stand

point, the 6 stages that Collins et.al (1987) suggested are at the heart of what communication scholarship needs to examine.

Modeling, whether it takes place in a classroom situation, or in any social setting, is but an interactional situation, where one serves a model to another that observes, in order to imitate the behavior at a later stage. *Coaching*, where one is trying to perform and the other is providing feedback, is also an interactional situation. *Scaffolding*, as an advanced type of coaching that is similar to mentoring in a professional situation, is clearly an interactional situation too. In *Articulation* learners learn to articulate the knowledge they were exposed to and use it in different contexts, so as to demonstrate their adaptive and interpretative competencies. Similarly, learners articulate the previously exposed knowledge, owing to the skilled questions of the teacher- or by extension of the person that severs a model- in any social situation, and never alone. Furthermore, the *reflection*, which Collins et al. (1987) consider being an internal work, a brainy one, without further explaining what exactly is in the brain that gets humans to reflect. Interestingly, even in the reflection, as a cognitive aspect, the authors did not account for any evidence that humans are equipped with cerebral capacities that enable them to treat information or reshuffle them as each individual would want. They, on the contrary, mentioned that reflection is but a process of comparison where the learner compares his or her process to that of his or her coach and mentor. Once again, reflection is described as an interactional practice, but named as such without any evidence that it is a cognitive practice.

And finally, the *exploration* phase comes once more to confirm that the whole cognitive apprenticeship model is not only describing cognition as an internal process, but also as an interactional process. In this model, exploration is the last phase of teaching-learning, where learners are increasingly encouraged to use the knowledge in open situations and show their creativity in shuffling and reshuffling the information they have learned and to particularly solve highly situated problems. During this stage, the learners are ultimately allowed to experience learning as they would in any authentic social interactional situation.

From Lave & Wegner's (1991) perspective, the idea that learning is a collective activity that is achieved through social interaction, parallels our statement- that learning is communicative or it cannot happen. Obviously, we are now referring to learning in its largest and most spontaneous way, and not only to the deliberate learning-teaching activity, no matter whether it takes place in a classroom or elsewhere. Lave & Wegner (1991) argued that groups of humans, whether at modern work settings or in any other regular gathering setting no matter how simple or complex it can be, sharing a particular activity or if they happen to share a place and a repeated experience together, tend to form what they called "communities of practice". The idea of *community of practice* has made it clear that groups happen to craft or design their respective micro cultures through interactions. Therefore, the shared knowledge and practices are conceived of to be collectively shaped over time and as a result of the daily and evolving interactions that in turn are affected by the multiple contextual factors that are particular to this or that group.

2.1.4 Humanism (A learner-centered construction)

Although the humanistic approach to learning was too focused on the individual rather than groups, this perspective contributed to "freeing" the teaching-learning activity from the obsession with the brain. It extended the scope of the learner's faculties to feelings. Perhaps the shift from cognitivism to humanism has made it easier to widen the scope and understand learning as a communication (i.e. collective) construct.

ARCS is a prescriptive teaching method that lies within the scope of the humanist methods. The method was developed by Keller in 1983 focusing mostly on how to motivate the learner, arguing that motivation is a key element in the learning process. *ARCS* stands for *Attention, Relevance, Confidence* and *Satisfaction*. It argues that the four mentioned conditions are the basic sources of motivation, and therefore they need to be made available to the learner by the teacher. It is beyond the scope of this research to discuss the teaching methods as such. We can nevertheless discuss the underlying assumptions of these methods with regard to how humans learn.

Interestingly, the method argues for the importance of playing the devil advocate by the teacher, in order to draw and retain the learner's attention, which is the first key element in the learning process (Keller 1983). Moreover, the method emphasized the role of asking questions that surprise the learner, in the learning process. This supports the current research assumptions with regard to the role of asking the trivialized question in the learning process. The trivialized question is a surprising one, because it entails that it is about something that is otherwise taken for granted or considered to be insignificant. The question is challenging and may address the Groupthink phenomenon (Janis, 1982), where members avoid asking questions to avoid controversies.

The Emotional Intelligence (EI) theory and method is a major breakthrough in the way we view the learning process. As its name reveals, the theory shifts the attention to emotions as a means to grasp knowledge (a way to know). Before Gardner's (1983), it was hard to conceive of human intelligence to be partially guided by emotions and not totally by the brain. Goleman (1995) was then able to demystify the theory, combining the trait and the ability models of EI into a mixed one. *Self awareness*, *self regulation*, *social skill*, *empathy* and *motivation* are the 5 core skills of EI as introduced by Goleman (1998).

How would members of a research lab (as in our case study) benefit from EI? The theory suggests orienting one's attention to one's own character. From the *self awareness* perspective, scientists may need to regularly review their own habits and predilections. Unless biologists (like any another scientists) consider to systematically think about their own characters or why they favor a particular way of doing research, they would not be able to distinguish between the biased and the less biased conclusions they make. If one is totally convinced that his or her decisions and choices are made by mere logical thinking emanating from an infallible brain, one would not be able to see the role that feelings for example play in the decision making and how they interfere in the thinking process itself. In groups, *Empathy* is a feeling that can seriously engender negative consequences with regard to the collective knowledge building. For instance, avoiding to ask a question or to make a point, because they feel it might "hurt" or "intimidate" a colleague or a supervisor in a scientific lab, is an empathetic posture that does not serve knowledge building. This

is also a pitfall of what Eisenberg et al. (2010) consider to be an empathetic dialogue, as opposed to real "meeting dialogue".

Another Humanistic theory that overtly argues for the importance of interaction in the learning process is the *Self-determination theory*, developed by Vansteenkiste, Lens & Deci (2006). The theory considers that "*relatedness"*, "*autonomy", and competence"* are three sources of motivation that enable humans to integrate knowledge. It argues that individuals cannot learn alone in a vacuum; they rather need the other in order to grasp knowledge. *Relatedness*, is perhaps more obvious, in emphasizing individuals' willingness to interact with others in order to experience the collective knowledge building and sharing. In Vansteenkiste et al. (2006)'s sense, relating to others or interacting with them is not an option, but a must, for individuals to be able to become knowledgeable in any matter. Hence, one's own contribution in a (scientific) discussion is always related to others' contributions.

By the same token, when a speaker says that his or her question is trivial beforehand, he or she is relating it to the others' views about the question. In other words, he or she is anticipating (or imagining) that others would say that his question or comment is trivial. Autonomy itself, according to Vansteenkiste et al. (2006), does not mean being independent from others. It rather means achieving the feeling of being able to integrate knowledge in one's own way, but always in relation to others. Hence, an individual might feel to be autonomous, if he or she is said to be so by others. In this sense, the individual is dependent upon the others' views about him or her, and in particular about his or her own knowledge. It is due to this feeling of dependency upon others that an individual would describe his or question to be trivial. Similarly, competence is not the ability to master knowledge in the brains, but knowledge that is being shared and experienced together. An individual would not know that he or she is competent without the others' endorsement. In this sense, individuals would need to optimize their discourse practices to please the others and make sure that they admit that he or she is a competent person. And when an individual feels that his or her contribution would not be in harmony with the others', he or she might start by describing his or her talk to be trivial, not to deceive them.

2.1.5 Social Constructivism (knowing as an intersubjective construction)

In section 1.5.1, I have already detailed the social constructivism (SC) as a theory of knowledge that lies in the overlapping area between Sociology and Communication. In this subsection, I will focus on 3 prominent learning theories that go in the same vein of thought as SC. More importantly, these theories will now broaden the picture of learning, which will go outside bodies and brains and will be a rather shared activity.

Hutchins' Distributed Cognition

Scientists such as Hutchins (1995 a) and Perry (2003) introduced the idea of *distributed cognition*. The more important word here is "distributed", as the theory considers that thinking and knowing is not an activity confined within individuals, but is diffused everywhere in the environment including the artifacts and objects. Hutchins' (1995 b) case study, which he conducted on the US navy, was a unique research that went into details about how reality is socially constructed and how knowledge emerges from interactions, which are contingent upon the contextual factors, and is not a matter of internal cognition within brains.

One obvious merit of the distributed cognition theory is the fact that it shifted the attention from the brain to the contextual factors that shape our knowledge building. Moreover, sociocultural elements are according to Hutchins not the product of our individual thinking, but at our collective interactions. "The emphasis on finding and describing "knowledge structures" that are somewhere "inside" the individual encourages us to overlook the fact that human cognition is always situated in a complex sociocultural world and cannot be unaffected by it" (Hutchins, 1995a). Theories such as Socially Distributed Cognition, which appeared in the 1980's, and which was based on the Vygotskyan Cultural-Historical Psychology, opened the doors for researchers to look at other elements (ex. time, space, objects, animals, artifacts, weather etc.) and not to limit the observation on persons, in order to be able to understand interactions and knowledge building.

Vygotsky's sociocultural approach

In line with the socially distributed cognition, the Vygotskyan's (1978) model acknowledged the role of the social interaction in the learning process. Although, Vygotsky used to be a cognitivist, his shift into a constructionist perspective marked his stress on the social interactions and on the past and present knowledge, in the learning process. As a matter of fact, even before the idea of social constructionism was born, Vygotsky's *sociocultural approach to cognitive development*, which is cognitivist by definition has acknowledged that complex learning always begins with social interaction, but at that time he understood learning to be an internal (or an internalized) activity. Unlike Piaget (discussed in section 2.1.2; page 30) who considered that the biological developments forge our learning capacities (Barbel & Piaget 1958), Vygotsky was the first to state that it is rather our social interactions and linguistic experience that shape our learning capacities and that the former precede the latter (Vygotsly 1978). However, *Distributed Cognition* (Hutchins' 1995 and Perry's 2003) went beyond both Piaget's and Vygotsky's works, by considering that cognition is not in the individual brains, but everywhere; and if we want to study how humans learn and communicate, we better need to widen the scope and see everywhere around. Hence, *Distributed Cognition* has marked a shift from the biological mind as a locus of control to the extended mind that involves groups, human and non human; and that is how *Distributed Cognition* meets the Actor-Network Theory outlined in the next paragraphs.

Actor-Network Theory

The French sociologist Latour (2005) along with earlier works of Callon (1986) and Law (1987) were the first to develop *Actor-Network theory (ANT)*. With the advent of ANT, scholars started to see knowledge being processed and developed everywhere and not located in a particular place. The theory does not see individuals to be the producers of knowledge, but rather the whole network, including humans and non human elements and factors. With this understanding, every element is a part and parcel of the knowledge building process. For Latour (2005), "*an 'actor' in the hyphenated expression actor-network is not the source of an action but the moving target of a vast array of entities swarming toward it"* (p. 46). More importantly, Latour pushed the philosophical discussion around

the concept of actor further and discussed the very principle of what a person is. Contrary to the understanding of the concept '*individual'* that is limited to the biological or physical manifestation, Latour considers an individual to be contingent upon others and never independent. He adheres to Jean-Paul Sartre's discussion of the concept of the individual and in particular to the case of the café waiter, who is unable to distinguish between his 'authentic self' and his 'social role'. The waiter's behaviors are constantly dependent on all other factors around, including the clients. Similarly, Latour gave the example of the stage actor, who is unable to be or behave in a way that he or she decides, but is in constant interactions with other actors, audience, and the multiple contextual factors.

By the same token, in real life situations, a person, in the physical or biological sense, who is trying to understand a piece of information or is in the process of forming an idea, does not act alone in a vacuum. Rather, he or she is in constant interaction with others, who play a crucial role in the learning process. Others are in fact substantial in the learning dynamic; and without others learning is not possible. Therefore, why have the past learning theories focused on the individual as a biological entity? Is the biological individual a separate entity? Morally, emotionally, spiritually and intellectually, is the biological *individual* independent? Based on the learning theories discussed in the previous sections, there seems to be a total neglect of the question *what is an individual?* Clearly, all the theories, including those recognizing the psychological and intellectual facets of humans, have taken for granted the idea that individuals are encapsulated in their physical or biological manifestation. And by the same token, the theories consider that a group is any number of bodies in the strictest physical sense. It is beyond the scope of this research to discuss what makes an individual or whether an individual is an entity that is independent from others and that coincides with his or her physical manifestation, but we are not either taking it for granted that an individual is just his or her physical manifestation. Similarly, this research will not discuss what makes a group, but we are not taking for granted that a group corresponds to the number of persons or bodies that share a geographic area.

2.2 Communication and knowing

In this section, I will zoom in on the inextricable relationship between knowing and communicating, through a survey of studies from different perspectives that emphasized this interrelation.

Our research- which is aiming at exploring the detrivialization practices (particularly during my interviews), as a strategy to deconstruct previous knowledge, crosses the concept of 'strategic communication' in Putnam (1994)'s terms, referring to the deliberate use of a particular process and data to satisfy the strategic goal of an organization. By the same token, the research adheres to Huff (1990) and Huff (2001)'s mapping of 'strategic thought' and 'strategic knowledge', correspondingly. More precisely, the research intends to be part of what has in the last couple of years become known as 'knowledge communication', in an attempt to examine the inextricable relationship between knowledge and dialogue (Eisenberg et al. 2010), focusing on the co-construction of knowledge, (Isaacs 1993, 1996 and 1999), as opposed to knowledge built by individuals separately and shared at a later stage.

A significant number of major Communication theorists have relentlessly stressed on the importance of the form in any given communication situation. Bateson argued that "mental process, ideas, communication, organization, differentiation, pattern, and so on, are matters of form rather than substance" (Bateson 1972; p 20). Bateson went even further in his beliefs and introduced what he called the *Theory of Logical Types; inspired by Russell's theory of types.* Bateson's (1972) theory asserted strict rules of formal discourse that he believed to be essential and if contravened, paradox will be the result and the discourse itself would become vitiated. Moreover, Bateson's *Metalogues* concretized this idea through the conversations they encompassed, where participants did not only discuss their respective problematic subjects, but also the structures of the conversations themselves. Bateson's Metalogues (cited in Bateson, 1972) represented simplistic but unique conversations between himself and his daughter, who explicitated ideas in an unusual way. Through these unique conversations, the relationship between ideas and how they can be generated was discussed explicitly by the two participants.

More interestingly, Snow (2001) suggested that thinking is but internal conversations that make use of symbols that come up out of the social processes. Through his extension to Blumer's conceptualization of symbolic interactionism, Snow (2001) emphasized the role of interaction not only in creating and recreating social reality, but also in the thinking process itself. More precisely, for Snow (2001) human beings cannot think outside interactions, be them visible or in our minds. This has equivalents in our theoretical assumptions, stating that understanding can only be achievable in the presence of the other, physically or in our imagination. Snow (2001)'s *symbolic interaction* introduced 4 elements that shape human communication, namely: *Human Agency*, *Interactive Determination*, *Symbolization* and *Emergence*. This latter characteristic of human Communication- "*Emergence"* is a key element in our present research. Because human communication emerges on the go, out of moment-to-moment situations; because it is not a retrieved substance from people's brains, we here assume that knowledge too is emergent out of conversational interactions. Therefore, for knowledge to develop and build up, we inevitably need to interact.

Similarly, Dialogic Learning (Habermas 1984, Bakhtin 1981, Freire 1970, Wells 1999, and Searl & Soler 2004), establishes an inextricable relationship between the otherwise supposedly 2 separate actions: *communication* and *learning*. From a *Dialogic Learning* point of view, dialogue is an inevitable means of learning. Whether it happens face-to-face, through writing, using telecommunication means, or in our imagination, dialogue is omnipresent. "One cannot not communicate", said Bateson 1972 from his end, to argue on the fact that communication is both intentional and unintentional, and that humans do not have the choice to stop communicating, even if they keep still and do nothing, in a vegetation mode. As a matter of fact, *Dialogic Learning* goes back to the Socratic and Platonic dialogues, in which Socrates and Plato played roles in dialogues that they enacted, using the Socratic dialectical method to build knowledge, based on critical thinking, systematic questioning, irony and more (Kreeft 2010). In modern history, Dialogic learning is a concept that was developed thanks to contributions from the following perspectives: 1) the theory of *dialogic action* (Freire, 1970), 2) the *dialogic inquiry approach* (Wells, 1999), 3) the theory *of communicative action* (Habermas, 1984), 4) the notion of *dialogic imagination* (Bahktin, 1981) and 5) the *dialogical self* (Soler, 2004), ("dialogic learning", 2015).

Bakhtin's (1981) *dialogic imagination* is of particular interest, as it claims that individuals not only create knowledge through dialogue, but they themselves do not exist outside dialogue. For Bakhtin, it is in our dialogues that we shape, negotiate and alter our own identities. Knowledge is the result of the meaning that our dialogues give to the world. Bakhtin made it more explicit, when he claims that even individual 'silent thinking' is the result of past dialogues that will have consequences on future dialogues, in a rather chain mode. Taking Bakhtin's idea into consideration, the current research considers that if we want to understand how knowledge is built, we need to examine how our dialogues are constructed. After all, where can knowledge be possibly visible? It is clearly traceable in discourse (i.e. in how it is explained). Therefore, the empirical part of this research will use discourse analysis to understand how knowledge is built according to how our dialogues / interactions are themselves shaped.

Building on Luhmann (1992)'s, I understand communication to be a *fuzzy* concept and a process that is *complex, contingent, contextual, consequential* and *emergent.* Consequently, communication (and by the same token interaction) is not a means of learning or knowledge building, i.e. we do not learn via communication. Rather, communication and learning are inextricably related and inseparable. The following citation by Luhmann triggers an interrogation on whether the process of learning is but a substantial part of communication and that the former can never exist outside the latter. Obviously, a human being needs a biological, physiological and neurological infrastructure (a body that works as it should), but other than that there is nothing but communication and it is within the scope of communication that learning happens. It is not that learning is a part that can be removed from the communication, but that it is 'substantial'.

> "There is no information outside of communication, no utterance outside of communication, no understanding outside of communication—and not simply in the causal sense for which information is the cause of the utterance and the utterance the cause of the understanding, but rather in the circular sense of reciprocal presupposition" (Luhmann 1992, p304).

Narrowing it down to the organizational level- where our current research is situated, the issue of knowledge production as such, has been tackled by Weick through his *Organizational Information Theory*. Weick (1969) examined the complexity of knowledge management within organizations and advocated the latter theory, through which he suggests a process of information collection, management and use, in order to reduce uncertainty. Weick's process of 'equivocality' reduction encompassed the 3 stages, namely *Enactment*, *Selection* and *Retention*; where the first is about information production, testing and treatment, the second about what to select from it to fit in the objectives in hand and the third is about what information to retain and what to disregard.

The empirical part of the current study is mostly related to Weick (1969)'s retention stage. More precisely, we are aiming at discussing how a piece of information is decided to be retained (i.e. played up, detrivialized) or not (played down: trivialized) during interactions, within the cancer research lab. Weick (1969) was considerably critiqued on the vagueness of his theory at this very level. For many critics the selection and particularly retention criteria were not clear and open to very subjective opinions. This is where the present research crosses Weick (1969)'s; i.e. why would a certain point be considered trivial or unimportant? Is this decision always scientific?

From another perspective, Tomasello (2008) provided a unique discussion on the origin of human communication from both ontogenetic and phylogenetic perspectives, comparing it to animal communication. The discussion outlines how human communication evolves over time (at the individual's milestone level and at the history of the species' level) and how this evolution is very tightly linked to knowledge construction and sharing. He stated that people communicate, because they need to express ideas and feelings that emerge out of the social interactions and that keep emerging all along the history of the individual (ontogenetic level) and groups (phylogenetic level). Furthermore, Tomasello argued that human communication, no matter how complex it can be, has emerged and developed over time, starting from its simplest forms of "pointing" and "pantomiming".

This primitive level of communication is similar to the onomatopoeic level of the verbal language. In this sense, both onomatopoeia on the one hand and pointing and pantomiming on the other hand reveal that human

communication is not an activity performed to express or share ideas or knowledge, but it is itself the idea or knowledge. At this primitive level of human communication, the inextricable relationship between knowledge and communication has been made obvious. Tomasello (2008) argues that human communication in its more complex level is similarly very tightly connected to knowledge construction, but this may seem less obvious, because communication has evolved into very sophisticated levels over the history of individuals and groups; which make it hard to observe ideas being shaped along with the interaction practices.

More interestingly Tomasello (2014) introduced a very interesting historical approach with regard to the relationship between "thinking" and "communication". The study considers that human thinking evolved and became more sophisticated as compared to all other species, only because historically humans happened to have a more sophisticated social life and more sophisticated types of interactions. It is the social interactions that made humans' brains more developed, rather than the opposite. Clearly, this is in complete contradiction with the innatist linguistic belief of Chomsky and others regarding how humans happen to learn their mother language (and by extension to communicate). Chomsky (1959) believes that children become able to speak a language, because they are biologically equipped with what he calls *Language Acquisition Device (LAD)* that enables them to acquire (not simply learn) the language to which they are exposed. In other words, without this predisposition (the LAD device) children would not be able to speak any language no matter how long they are exposed to it. This discussion brings us to the perpetual discussion of "nature vs. nurture" in social and human sciences, where the question about this or that human capacity is an inborn (natural) or learned (nurtured). It seems that Chomsky's LAD has been continuously challenged, and we believe that both Tomasello (2008) and (2014) happen to be more challenging, especially that Chomsky failed to argue where exactly this hypothetical LAD is located in the brain, and why it is the only possible way to explain how children learn a language.

Finally, a unique research conducted by Mondada (2005) has provided very interesting findings with regard to how knowledge emerges out of interactions. Although, we are methodologically not on the same ground with

Mondada (2005), her empirical research greatly benefits our research. The study provides passages of observed interactions collected by the author. Through conversational analysis, the author demonstrates how knowledge emerges in a consequential fashion. Through retrospective analysis, she was able to retrace knowledge that was made explicit in this or that conversation and which has roots from past conversations. Likewise, the author started from present conversations and followed their consequences on future ones; which was a prospective analysis.

The similarity between Mondada (2005) and our current empirical research is that both are interested in how knowledge is constructed in discourse. However, our present research focuses in particular on the role of the (de)trivialization practices in knowledge construction and aims at studying these practices, as a strategy to gain more insights on how knowledge is constructed. More interestingly, Mondada (2005) reported how knowledge is co-constructed, and by extension how it is co-rejected in and through discourse. To borrow Latour & Woolgar (1979)'s expression, she reported how "in the course of relatively brief conversations scientific facts are created or destroyed" (p 154).

2.3 (de)trivialization and knowing

In section 2.1, I have reviewed the relationship between *knowledge* and *communication* throughout the history of learning theories. In section 2.2, I have detailed the inextricable relationship between the two concepts; hence paving the way for the legitimacy to study knowledge as a communication practice. In this section, I will focus on previous studies that legitimize the need to specifically study the (de)trivialization practices; and how this is related to knowledge construction.

2.3.1 Knowledge and knowing in Organizational Communication

Kuhn (2014) reviewed the literature of Organizational Communication that has been focusing on knowledge and distinguished between 3 main perspectives: 1) Knowledge Management (KM) systems; 2) Networks and Knowledge; and 3) The Communities of Practice (CoPs). First, the KM perspective rather focuses on: "coordinating and controlling the generation, codification, and application of knowledge in organizational practice" (p. 484). Secondly, the Networks and Knowledge perspective focuses on "the ways networks structures and processes affect knowledge in organizing as well as literature on how connections between actors can foster the emergence of a collective level resource" (p. 486). And thirdly, the CoPs perspective advocates that "learning almost always occurs in the context of others- more specifically, in CoPs. Moreover, learning is often obvious at boundaries, such as when individuals and organizations are peripheral to a given community and seek fuller membership or when they occupy luminal positions with respect to two or more communities"(p. 488). Kuhn built the distinction between the 3 perspectives on the distinction between: knowledge, information and data: "*Data* are framed as objective facts (such as the record of a financial transaction); *information* is data linked together in a message engineered by a sender to alter a receiver's thinking (such as a statement comparing the number of transactions in a site over some span of time); and *knowledge* refers to information made meaningful and valuable with respect to evaluation and action (such as the insight that the number of transactions in a given site are likely to change because of what one has ascertained about a competitor's actions)" (p. 482).

The CoPs perspective (which best matches our own understanding of knowledge and communication) is not only concerned about *knowledge* more than *information* and *data*, but also prefers the use of the gerund *knowing* to the noun *knowledge*. The use of the term *knowing* emphasizes: the dynamism of the activity (processual and changing) and the interactivity (it occurs with others; between individuals and groups rather than inside them). Kuhn & Jackson (2008) have emphasized the usefulness of the CoPs perspective in exploring the organizational knowing as: 1) an action (a problem-oriented action) rather than a stable objects, facts and dispositions; 2) a community co-construction (being deeply social); and 3) a problem-solving activity rather than independently justified as true. However, Kuhn & Jackson (2008) also emphasized that the CoPs perspective has faced criticisms, mainly due to the following concerns: 1) that organizational knowledge or knowing is still vague and not operational; 2) that the practice-based approach is not accompanied by methodologies of sufficient complexity; and 3) that existing models for investigating knowing are built on simplistic assumptions about intracommunity consensus. To address these issues, Kuhn & Jackson suggested an "analytical framework that can guide examinations of knowing, defined as situated problem solving at the micro level" (p. 457).

Kuhn & Jackson (2008)'s proposed framework suggests that in order to study how knowledge and knowing functions in organizations, we need to start from *problematic situations* and observe how participants solve problems, in different situations on a continuum of *determinacy* (from highly determined situations to low determined or indeterminate situations). Using different examples and scenarios (mostly from a call center of a Help desk they observed), Kuhn & Jackson (2008) distinguished between 4 types of knowledge deployment: information transmission, information request, instruction, and improvisation. In order to respond to the necessity of the problematic situations suggested through this framework, I wish to state the following: While Kuhn & Jackson's (2008) case study (call center) enables them to observe the problematic situations immediately as they occur (i.e. when a caller asks the telephone operator to solve a technical computer problem), in our own case study (which examines a cancer research lab), problems are not that observable. In our observed lab, our participants work on different and separate experiments that are well designed in advance and, if problems occur, they do not necessarily

discuss them in front of me (as an observer). Usually, the post doctoral researcher thinks about solutions and implements them on their own, or instructs the technician to do something. At least in this cancer research lab, problems tend to be thought of without observable interactions taking place, or are discussed behind closed doors between the lab director and one of the post docs. Additionally, being unknowledgeable about Biology, I would not be able to figure out whether this or that discussion is of this or that level of determinability. More importantly, in our own case- a cancer research lab (unlike the call center case, where the participants are obliged to engage in verbal talk, describe and solve problems, in open conversations with their clients and between staff), problematic situations may be avoided or concealed for various reasons (i.e. groupthink (Janis 1971 and Whyte 1952); dispute settlement (Latour & Woolgar 1979); cognitive dissonance avoidance (Festinger, 1957 and 1962) etc.

Consequently, my use of Garfinkel's (1967) *breaching experiment* is a necessity to be able to dig down the covert problematic situations; thereby "to make the commonplace scenes visible" (p. 36). In section, 2.3.2, I will start by developing more on the need to use the breaching experiment in order to explore the trivializations that are otherwise unobservable (i.e. implied; not necessarily discussed). In section 2.3.3, I will establish a link between the collective trivialization and Groupthink (Whyte 1952 and Janis 1971) and argue that the *detrivialization* that my questions will trigger may constitute a unique opportunity to achieve *a real dialogue* (Eisenberg et al. 2010). Finally, through section 3.3.4, I will outline the need to explore *(de)trivialization* as a sociopsychological area of interest, mainly through the studies of Simon, Greenberg & Brehm (1995) and Festinger (1957 and 1962). Hence, sections 2.3.2, 2.3.3 and 2.3.4 will pave the way for our case study's methodological choices.

2.3.2 Breaching experiment to make commonplace scenes visible

From an Ethnomethodological perspective, Garfinkel (1967) coined the term *breaching experiment*, to refer to the deliberate violation of the social rules with the aim of observing and studying people's reactions. For Garfinkel, such violations and the reactions they trigger constitute a unique opportunity to understand the underlying assumptions that people make about the rules they use unnoticeably. He specified that breaching experiments should target the "seen but unnoticed" practices that people engage in. It is through this technique that a researcher would be able to access the underlying knowledge and explore how group members have in the past constructed, inherited or acquired what they now imply or take for granted. By the same token, in the case study of the present research (in a cancer research lab), I will use the breaching experiment technique to target the ‘‘commonplace’’ and make the underlying assumptions (that I would not be able to see or understand otherwise) visible. In other words, I will trigger discussions over the commonplace, specifically for our research, and will not be contented by observation. Interestingly, ‘‘commonplace’’ (and also ‘‘mundane’’) is by definition (see section 1.3) a synonym for trivial (in the second sense of the term). Therefore, I found it quite judicious to target the trivial (what is considered to be commonplace) in order to understand what has been trivialized (in the sense of what has been considered to be insignificant or of little importance to re-evoke). Obviously, digging down the commonplace presupposes that the informants share a set of habits, which I need to understand how they have been constructed. I do not want to call these habits ‘‘practical knowledge’’ beforehand, for two reasons: 1) because *practical knowledge* is still controversial in the literature (Lumer, 2010); 2) because it is hard for me as an outsider to identify practical knowledge in a context that I ignore (cancer research); and 3) because *commonplace* can also be related to other sociopsychological phenomena, such as *Groupthink* (Janis 1971 and 1982; Whyte 1952). Therefore, I will remain inductive and faithful to interpretivism; hence I will stick to the expression: ‘‘commonplace’’ and will not go beyond it beforehand; i.e. what I will be able to identify as commonplace, through my own ethnomethodological observation.

2.3.3 Trivialization from "Groupthink" to "real meeting dialogue"

Groupthink is a well established concept in social psychology. It refers to the "excess" of group cohesiveness, which causes limitations to knowledge building. Whyte (1952) was the first to use the term, but Janis (1971) was the first to conduct a research within this framework: "in the spirit of Parkinson's law […] the more amiability and esprit de corps there is among the members of a policy-making ingroup, the greater the danger that independent critical thinking will be replaced by groupthink, which is likely to result in irrational and dehumanizing actions" (p. 43). Thus, in our case study, once I will identify instances of trivializations in the cancer research lab, I will not be contented by "clear cut" answers, such that those usually solicited by structured questionnaires. Rather, I will explore the trivializations by opening detailed discussions with the lab members and comparing and contrasting the different responses and accounts, simply because, if the lab members happen to be avoiding controversies, I may not be able to understand the complexity of the trivialized issue. Interestingly, the relationship between Groupthink and trivialization is already established, at least from Janis (1971)'s point of view: in the citation mentioned in the present paragraph, he referred to *Parkinson's Law of Triviality* (Parkinson, 1958), which I already introduced in section 1.4 (page 13).

As a psychological phenomenon, Groupthink may be responded to by engaging the lab members in *real meeting dialogues* (Eisenberg et al. 2010) over the trivialized issues, through open interviews. From there, detrivialization is triggered, and it is through our initiated (de)trivializing discussions that the underlying (past) knowledge can be called to surface and become thereby studiable. Eisenberg et al. (2010) have distinguished between four types of dialogues: *the mindful communication dialogue*, *the equitable transaction dialogue*, *the empathic dialogue*, and *the real meeting dialogue*. The writers emphasized the need for the real-meeting dialogue in learning, because it is "not merely for the purpose of exchanging ideas or information in order to learn, it is engaged in for the purpose of revising how the parties involved actually understand something" (p. 46). Although the empathetic dialogue can be essential in achieving cohesion, harmony and understanding between group members within organizations, the real meeting dialogue "involves far more than simply getting someone's "take" on a situation for

the sake of information; it involves being open to changing an opinion as the result of soliciting the perspective of another" (p.46). Because they enable us to avoid empathizing with others at the expense of being authentic (ex. Being able to say what we really think and not what others want to hear), real-meeting dialogues can lead to new and better ways of doing things within organizations, Eisenberg et al. (2010). Daring to rethink what has otherwise been trivialized or taken for granted is that key moment where we shift from an empathetic to a real meeting dialogue.

2.3.4 Trivialization: a socio-psychological phenomenon

Within the framework of the *Sociology of Scientific Knowledge* (SSK), a number of scholars have emphasized the importance of focusing on triviality and trivialization in scientific contexts. Latour & Woolgar (1979) who have observed a biology lab, reported that what interested them as sociologists was described by the biologists (their participants) to be "unsurprising if not trivial" and that such "reaction was a nice confirmation of the accuracy of our [their] observations" (p. 274). Moreover, Latour & Woolgar who provided examples on how "facts can be created and destroyed during brief conversational exchanges" (p.154), have also reported the difficulty that group members face to contradict one another, to avoid controversies; at the expense of accuracy: "the standing of one scientist might be such that when he defines a problem as important, no one feels able to object that it is a trivial question". Thus, building on Latour & Woolgar's (1979), I found it important to address the group dynamic that would be mobilized, when I as an outsider would point out a contradiction between the lab members that I am studying in the current research case study. To do so, collective interviews were also conducted and analyzed, after individual interviews had already been conducted and after a number of controversies had been identified. The interest is to examine, how the controversies will be settled. Latour (1987) insisted on the need to open "Pandora's black box"; i.e. urging scientists to engage in thoughts and discussions about how things that are otherwise taken-for granted actually work inside (example machines) and not to just assume that they are working properly. For the same reason, Latour put the stress on the need to follow scientists during their construction of knowledge (in situ) not when they present their 'nicely packaged' findings. More importantly, Latour & Woolgar (1979) found that in the lab they observed, "reality was the consequence of the settlement of a dispute rather that its cause" (p. 236), suggesting that the lab members

avoided disagreements at the expense of scientific accuracy, and regardless of the consequences of compromising knowledge building. Based on these findings, Latour and Woolgar have stressed the importance of distinguishing between what is scientific and what is not in the scientists' discourse.

At the individual level, a number of socio-psychological studies have described in different ways the psychological hassle that an individual can experience and which makes him or her trivialize a version of a story at the expense of another; in order to achieve a level of *comfort*, even at the expense of *logic* or accuracy. Simon et al. (1995) have conducted 4 different studies and concluded that their participants use trivialization (reduction) when they face the psychological pressure of cognitive dissonance (being seen as contradictory). Rather than changing of opinion, or admitting the contradiction, participants played down the importance of the dissonance. These studies support a number of other studies that illustrate that people do not want to be seen as contradictory or inconsistent, otherwise they suffer from mental stress (Festinger 1957 and 1962). Hence, in our own case (a cancer research lab), I will also need to examine how individuals would account for when I point out their contradictions (whether or not they will compromise knowledge, in order to avoid being seen as inconsistent) and what linguistic tools they will use to achieve that aim.

2.4 Literature review summary

The literature review was divided into 3 main parts: The first part (section 2.1) discussed the interrelated concepts of *knowing* and *communicating* in the dominant contemporary *learning theories* aiming at providing a discussion on how the two areas of interest started to be completely separate and how they have along the history converged. The second part (section 2.2) introduced a number of studies that emphasized the inextricable relationship between *knowing* and *communicating*. Finally, the third part (section 2.3) surveyed a number of studies that cross our current research and legitimize the need to study (de)trivialization as a communication practice and to relate it to knowing.

In section 2.1, through a survey of the dominant learning theories in the last century, I discussed the consequences of the fact that, prior to the emergence the *social constructionism*, learning and knowledge theories have given very little attention to the role of *communication* in knowledge building, which goes beyond the mere transfer or exchange of information. Based on that, I have accounted for the vital role of interactions in knowledge building. I then argued for the role of Communication as a discipline, in bringing a new perspective to knowledge and knowing. Until the emergence of the idea of *distributed* cognition (i.e. Hutchins 1995), knowledge and learning have on the whole been understood to be a cognitive or cerebral work, in most theories. Using various studies of different perspectives in section 2.2, I understand *knowing* and *communication* to be inextricably related, and from that perspective my critique of the literature was made. In section 2.3, (building on Kuhn 2002, Garfinkel 1967, Latour & Woolgar 1979, Janis (1971 and 1982), Whyte (1952), Whyte (1989), and Festinger (1957 and 1962) etc.), I concluded that observing interaction is observing knowledge being constructed (knowing on the go). From there, I argued that examining (de)trivialization can be heuristically strategic in gaining insights about knowledge and knowing at the (inter)organizational, group and individual levels.

Based on the literature review, I wish to argue that (a) knowing is a communication activity and ought to be studied as such; especially that knowing in modern societies is taking a myriad of formats and is no longer limited to

a classical classroom setting. Even classical classroom settings are more and more democratized and taking all sorts of formats and interactions. Perhaps settings that foster more the need to look at learning as a communication activity include scientific research labs, multimedia learning designs, on-line courses, Wikipedia as a unique concept, etc. Studying learning at a cancer research lab, for example, needs to be approached inductively and as a communication situation, rather than deductively through the orthodox learning theories. Secondly, (b) I understand trivialization to be a communication practice that can potentially hinder knowledge building. Finally, (c) I wish to study (de)trivialization as a strategy to gain insights on knowing.

Now, the case study, which I first introduced in section 1 and which I built using section 2 will start in section 3 (page 55). I will start by briefly introducing my approach (in section 3.1) and outlining the methodology (in section 3.2) to pave the way for the analysis (section 3.3). Section 4 will introduce the research findings followed by a discussion. In section 5, I will wrap up the current research, establishing a link between the theoretical and the empirical parts of this paper. Figure 1 (on page 54) provides a graphic summary of the theoretical part of this research and portrays how our case study emerges from it, while figure 2 (on page 59) sketches the different methodological steps of the case study.

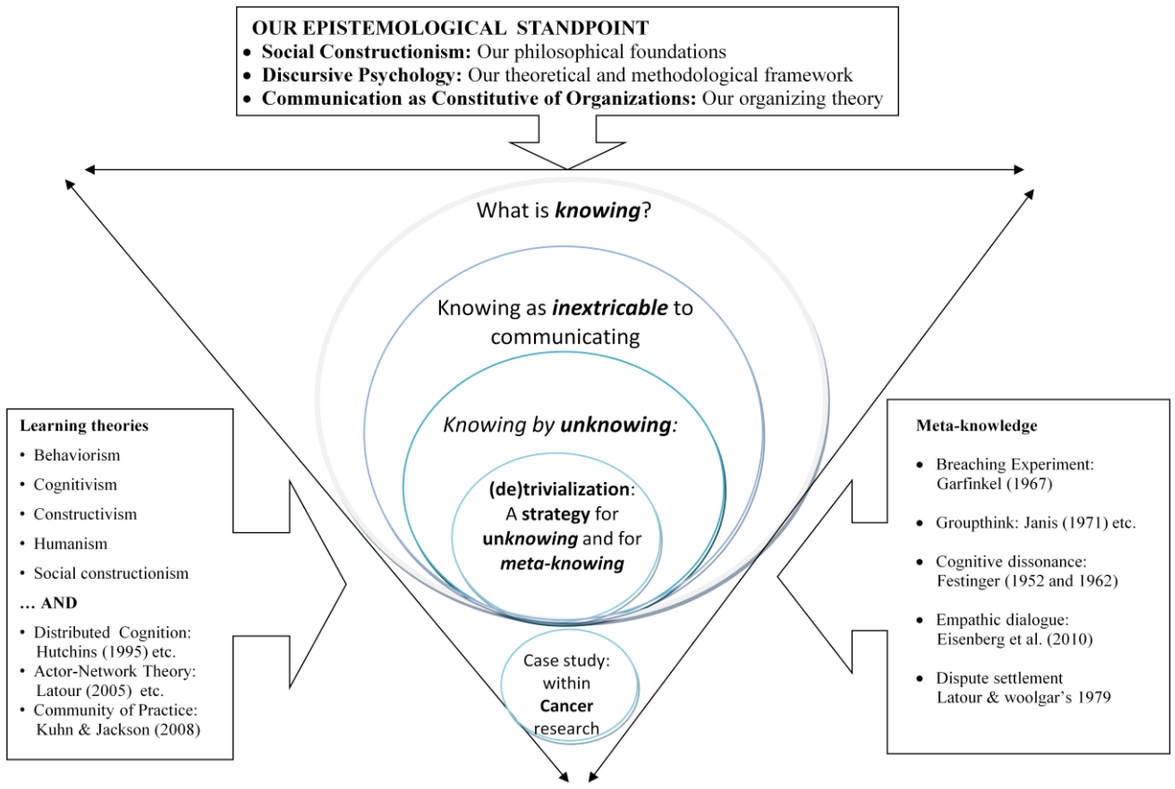


Figure 1. From global thinking to local acting [1]

[1] Compare with figure 2.

3. Case study

3.1 Approach

Epistemologically, the research is well nested in the interpretivist paradigmatic realm, the major theorists of which come from different origins: Schutz (1962), Cicourel (1964) and Garfinkel 1967 (phenomenology and sociology), the Chicago School of Sociology, such as Blumer 1984 (sociology), and Malinowski 1967 (anthropology). From this stand point and unlike the positivist or cognitivist paradigm in particular, two main assumptions are made: first, that reality is perceived inter-subjectively through the meanings that we share experientially and socially. This situates the research in the *constructivism* from an ontological stand point. Second, because humans cannot detach themselves from what they try to know, the relationship between the knower and the object of thought is inextricable and consequently the understanding is partly contingent upon who the knower is and how he or she interacts with the object of thought (Potter & Wetherell, 1987).

The research question will be addressed from a socio-psychological point of view. The Social psychological approach is a tradition in Communication research since early 20th century, although the word *Communication* first appeared in the literature of the experimental Social Psychology in Hoveland (1948) according to Craig & Muller (2007). More precisely, our research question will be examined from the *sociological social psychology* perspective rather than the *psychological social psychology*. The main reason behind that is the fact that the former approach targets the group as a main subject, while the latter targets the individual. Because our research question is focused on the collective (group) knowledge building, it is counterintuitive not to adopt the sociological social psychology approach for such a research question. Thus, the research is almost entirely qualitative, observational and discourse analysis based. From this perspective, studying communication practices entails studying their corresponding social contexts. Communication is seen as a process of social interaction that shapes and is shaped by causal forces of social influence. Finally, the research will be based on field observation. The field – a cancer research lab- will be described in the methodology section below.

3.2 Methodology

Our case study is guided by *Discursive Psychology*, both as theory and method. While the theory was already introduced in section 1.2 above, in this section I will detail our 10-step method, based primarily on Potter & Wetherell 1987. But before that, it is important to mention that *discursive psychology* is one among other well established methods of Discourse Analysis. Therefore, I will first: (a) distinguish between the other 3 major Discourse Analysis methods and argue for the reasons behind my choice for *Discursive Psychology* in particular; and (b) clarify our use of the term discourse, distinguishing between *Discourse Analysis* and *Conversational Analysis*. Finally I will detail our 10-step method.

(a) With regard to the choice of the Discursive Psychology method, I will here briefly compare it mainly with *Laclau & Mouffe's 1985*, *Fairclaugh's 1995 and 2001,* and the *Foucauldian (through Kendall 1999)*. The *Laclau & Mouffe's Discourse Analysis* (1985) was founded through a unique combination and deconstruction of two major theoretical traditions: Marxism and Structuralism, where the first serves as a socio-economic ideology and the latter as a theory of meaning. This blending of the 2 stances oriented the method to the understanding "of the social as a discourse construction, where all social phenomena can be analyzed using discourse analytical tools" (Jorgensen & Phillips, 2002, p.24). The *Fairclaugh's 1995 and 2001 Critical Discourse Analysis* is influenced by Gramsci's post-Marxist theories, as well as by Bakhtin's, Halliday's and Bourdieu's works on social power. The method is said to be critical, because it is clearly oriented into the oppressed social groups and is calling for a fairer society (Fairclaugh, 1995 and Fairclaugh, 2001). The *Foucauldian Discourse Analysis* as its name suggests, is inherently influenced by Michel Foucault's major philosophical works, but also by Jacques's Derrida's as well as the larger scope of psychoanalysis and critical theory (Wooffitt, 2005). The method is specially characterized by a focus on power relationships as reflected in language use and behaviors (Kendall, 1999). With regard to why I chose Discursive Psychology as a theory and method, there are 3 good reasons: first, because DP is clearly less ideological and clearly less 'obsessed' with or less focused on 'power' and hence, at least from this posture, it is less biased. Second, DP is oriented into socio-psychological factors, less than political and is therefore more suitable to study issues such that I am studying (i.e. trivialization as a practice and how it is related to learning).

Thirdly, as I will detail in the next subsection, DP is particularly effective in analyzing scientific discourse, which is also our case, because our data is scientific (cancer research data, collected through observation and interviews).

(b) It is now essential to be clear with what we mean by *discourse*, because according to Potter & Wetherell (1987), the term has been used with a variety of interpretations across disciplines such as linguistics, psychology, anthropology, literary studies, philosophy, and communication studies. Therefore, from our standpoint, "discourse is understood to cover all forms of spoken interaction, formal and informal, and written texts of all kinds" (p. 8). All along our case study, I will refer to discourse from that standpoint, unless I specify otherwise. Now, because Discourse Analysis and Conversational Analysis can be easily confused and are used interchangeably in the different literatures across social and human sciences domains (Potter & Wetherell, 1987), I wish to distinguish between the two in the following way. While both methods study the language in its own right, conversational analysis is typically focused on authentic and everyday conversations, which is not necessarily the case for discourse analysis, which studies spoken and written texts, formal and informal, naturally occurring, or designed, including the highly specialized or technical discourse (Wooffitt, 2005). It is for this very reason that discourse analysis is more suitable for our research; particularly that our analyzed texts are (a) interview-based and (b) scientific (around cancer research topics). Additionally, conversational analysis focuses more particularly on *how* discourse is constructed (example through turn-taking, interruptions, overlaps etc.), whereas discourse analysis (particularly DP) can focus on *what* the participants' accounts construct, stressing on the different roles each account can play separately (Wooffitt, 2005). DP too is interested in studying how the whole conversation functions, but is also interested to compare accounts of the same participant across different conversations and not only accounts made during the same one. The aim is not to stick to the form and functionality of the conversation as such, but to study the variability of accounts and what motivates it. Finally, both discourse analysis and conversational analysis adhere to the Ethnomethodological views, particularly with

reference to how “the social action is accomplished through the competencies of the participants” (Wooffitt, 2005, p.73).

Figure 2 on page 59 portrays the 10 Discursive Psychology’s methodological steps that I undertook in my case study. Each step will now be detailed, following the theoretical assumptions that I make about it.

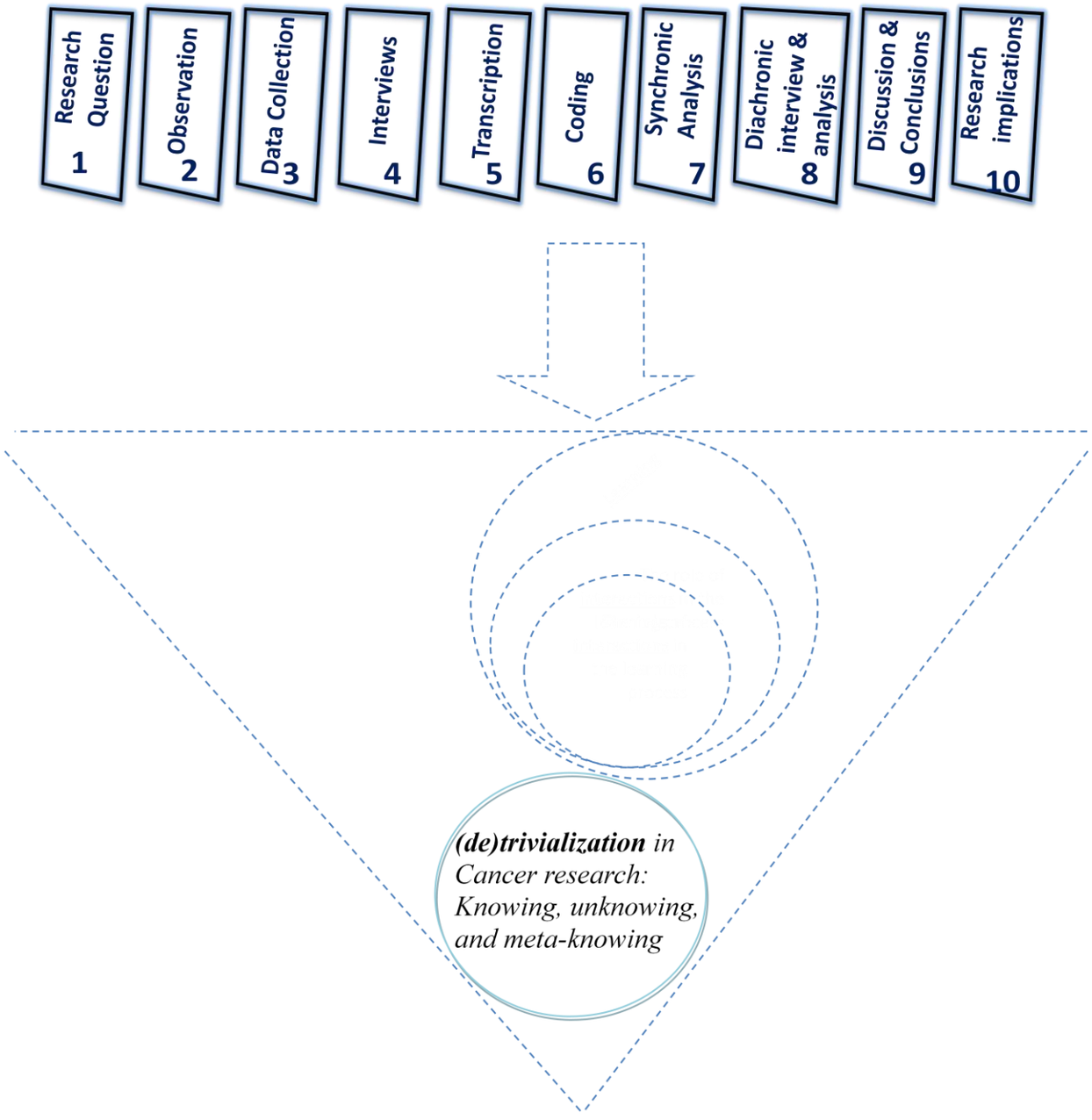


Figure 2. The Discursive Psychology's 10 methodological steps[2]

[2] Compare with figure 1

3.2.1 The Research Question

The research question is the following: *How can the (de)trivialization practices be useful to know about knowing?* From the main question, I generated 3 corollary questions: (a) What is the role of trivialization in the lab's knowledge construction? (b) What is the role of the detrivialization- triggered by our research- in the lab's knowledge construction? And (c) how can we know about knowing, by studying (de)trivialization, from our perspective?

Building on the following past studies, the research presupposes that trivialization is a practice that does exist in cancer research labs: 1) Garfinkel's (1967) breaching experiment ("to make the commonplace visible"); 2) Groupthink (Janis 1971 and 1982; Whyte 1952 and Whyte 1989); 3) Empathic Dialogue (Eisenberg, 2010); 4) Latour & Woolgar's (1979) findings regarding the practices of constructing and destroying scientific facts: 5) Cognitive Dissonance (Festinger 1957 and 1962). From a broader philosophical perspective, our research also addresses Feyerabend's (1978) concern regarding the dogmatic and sectarian practices of modern sciences. From an epistemological perspective, the research hinges on the main idea that we have detailed throughout the theoretical section, based on Potter & Wetherell's 1987 Discursive Psychology assumptions; i.e. that knowing is a discourse activity and that studying how knowledge is constructed can be achieved through the analysis of the discourse practices, in all their forms.

3.2.2 Observation

I conducted the participant-observation at a cancer research lab in Brussels, Belgium. Throughout the observation period, which lasted 18 months (from June 2014 to December 2015) with sporadic absences, I was able to access the lab on a daily basis. I could enter any room and speak to anyone without major difficulty. I incessantly observed, thought about how things work and asked questions.

The lab is one of a 4-lab pole. It is situated on the 5th Floor of an 8-floor building. The pole combines 2 main sections; each of them comprises 2 labs: the one section is concentrated on cardiovascular studies and the other is on cancer / tumor research. The lab that I am observing is part of the tumor section. It works on 2 main topics: Tumor Hypoxia and Tumor Metabolism. Both topics are divided into 2 research categories: Basic and Translational.

On the lab web page, 28 publications are listed. The title states that the list comprises the publications of the past 10 years. The publications range from 2003 to 2014. All publications have multiple authors (from 5 to 15 authors per publication). The lab director's name appears on all the publications, but never the first. 10 of the publications were on an American journal, the first was in 2013 and the last in 2014. The other 18 publications were on other journals, on the basis of 1 to 4 publications per journal. The same webpage lists 15 reviews, dated from 2002 to 2013, each of them is of multiple authors (usually 3, but a couple of them have 5 and 6 authors). On the same webpage, 9 books are listed as having been published by the lab, between 1995 and 2012.

The lab also has a rubric instructing people on how to donate for the lab's ongoing experiments, in order to support research. The rubric leads to a PDF document in French dated 2013 and states that the next aim from collecting donations is to be able to find a molecule (name of molecule removed), which requires hiring a new researcher, which in turn requires 92.500 Euros. Appendix A.1 includes more information about the lab[3] and appendix A.2 information about the 10 lab members – our research participants[4].

The observation was typically conducted as follows. When I arrive to the lab, I immediately place my belongings in the cafeteria, take my journal and a pen and start strolling around until I spot someone who seems to be available and willing to speak with me while conducting his or her operation. Typically, I used to sit next to any informant who was doing his or her work and opened discussions using questions such as: what are you doing now? Why are you doing it this way? Is there other ways to do it? How long have you been doing this? Who else does this

[3] Information adapted from the lab website.
[4] Information adapted from the lab website.

here? Do other biology labs do it as you do? How often do you do this? Who taught you to do it this way? How can you make sure that this operation enables you to know what you said you wanted to know? Additionally, I walked around the lab and observed machines, interactions, operations and artifacts. I asked myself questions and immediately asked anyone who got in my way questions such as: what is this for? Why is it so and so? Then, I addressed the same questions to more and more people. The major aim was to facilitate my understanding of the space, and to explore how participants explain things to outsiders (myself). These conversations also helped me to spot covertly trivialized matters and analyze them in view of how they are commonly and continuously vs. uncommonly and sporadically accounted for. Hence, my observation was designed to both follow Potter & Wetherell's (1987) discursive psychology's method and use Garfinkel's (1967) *breaching experiment*.

Background information

Typically, the lab members work on cells[5] (usually cancerous[6]) that they purchase from external providers. Cells can be primary or immortalized. Primary cells are taken directly from humans or from animals (usually rats). But most of the experiments in this lab are conducted on immortalized cells or what they usually call cell lines. Cell lines[7] are cells that have been modified in such a way that they do not die easily, which will enable the researchers to conduct a number of experiments without having them die in the middle of the research. The different researchers in this lab usually aim at either understanding a particular aspect of the cell metabolism[8], or testing drugs on cancer cells to check its therapeutic[9] efficiency. Any cells are immediately stored in the freezer, as soon as they are delivered by the provider. If they leave the freezer the cells would soon die.

When a researcher would want to start working on some cells, he or she would need to prepare for what they call cell culture[10]. Culturing cells is providing them with the adequate conditions to get them to grow as they would in the human body. The cell culture starts by the preparation of the medium[11]. The medium is a liquid made of a number of nutrient ingredients that are supposed to substitute for what the human body consumes in terms of food and drink. The medium usually has the same ingredients for the different experiments, but if the researcher is working on a particular aspect, he or she may intentionally remove or add some or part of an ingredient. Once the medium is ready, then the cells can be taken out of the freezer and placed in small plastic petri dishes[12] where medium is added. Consequently, the cells would be constantly exposed to the medium to consume from it. Cells are alive and they are supposedly functioning more or less as they would in the animal or human body.

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[5] The smallest structural and functional unit of an organism, which is typically microscopic and consists of cytoplasm and a nucleus enclosed in a membrane (Oxford dictionary).

[6] *Affected by or showing abnormalities characteristic of cancer:* a cancerous tumour, If stem cells are unstable, they can become cancerous. (Oxford dictionary).

[7] *A cell culture developed from a single cell and therefore consisting of cells with a uniform genetic make-up (Oxford dictionary).*

[8] *The chemical processes in living things that change food, etc. into energy and materials for growth (Oxford dictionary).*

[9] Designed to help treat an illness (Oxford dictionary).

[10] A preparation of cells obtained by culture (Oxford dictionary).

[11] The substance in which an organism lives or is cultured (Oxford dictionary).

[12] A shallow, circular, transparent dish with a flat lid, used for the culture of microorganisms (Oxford dictionary).

The cells, which are now covered by the medium, soon get glued to the wall of the petri dish taking the shape of a very thin whitish to colorless layer, which we can see with the naked eye. They should very quickly be placed in the incubator[13]. The incubator is a machine that resembles a small fridge in shape. Its role is to provide to the cells the same conditions that the human body would need. Therefore, all incubators in the lab are set at 37° Celsius and 5% of co2 level[14]. According to the lab members, these 2 parameters are set like that, everywhere else. The researchers change the medium from time to time, usually once every 3 days, because they say that the cells consume from the medium which becomes impoverished from a number of ingredients that the cells need, and also because the secretions let out by the cells need to be evacuated in order for the cells to consume cleaner medium. To do so, they suck the old medium using pipettes[15] and immediately inject the new one. These operations and all others are performed inside the hood[16]. The hood is a device used to suck the ambient air up, in order to avoid that bacteria[17], viruses[18] or fungi[19], if they happen to be around, to fall on the cells. Bacteria, viruses or fungi are said to be able to kill the cells and they need to be avoided, through strict rules, such as wearing gloves, sterilizing equipments and every tool or hand that gets in contact with the cells or near them.

Because the cancer cells grow and proliferate[20] fast, the researchers need to regularly throw some of them. And because they are glued and cannot be easily detached from the dishes, they use what they call Trypsin[21]. Trypsin is a liquid that gets the cells away from the petri dish wall. Then they can throw half of the liquid (which is now a

[13] An apparatus used to hatch eggs or grow microorganisms under controlled conditions (Oxford dictionary).
[14] Co2 is A chemical formula of a colorless, odorless gas produced by burning carbon and organic compounds and by respiration. It is naturally present in air (about 0.03 per cent) and is absorbed by plants in photosynthesis (Oxford dictionary).
[15] A slender tube attached to or incorporating a bulb, for transferring or measuring out small quantities of liquid, especially in a laboratory (Oxford dictionary).
[16] *An extractor hood*, one that removes particles usually by sucking them up (Oxford dictionary).
[17] A member of a large group of unicellular microorganisms which have cell walls but lack organelles and an organized nucleus, including some which can cause disease (Oxford dictionary).
[18] An infective agent that typically consists of a nucleic acid molecule in a protein coat, is too small to be seen by light microscopy, and is able to multiply only within the living cells of a host (Oxford dictionary).
[19] Any of a group of unicellular, multicellular, or syncytial spore-producing organisms feeding on organic matter, including moulds, yeast, mushrooms, and toadstools (Oxford dictionary).
[20] Reproduce rapidly (Oxford dictionary).
[21] A digestive enzyme which breaks down proteins in the small intestine, secreted by the pancreas as trypsinogen (Oxford dictionary).

mixture of Trypsin and the cells). Thus, they end up with fewer cells. If cells are not reduced they are said to reach a level where they burst inside the dish and die, just as cancerous organs would do in a metastasis[22] phase.

Depending on the aim of the research, researchers typically study what they call gene[23] expression or protein[24] expression, which means that they want to see if this gene or that protein is related to this or that condition that they are testing. Others are testing drugs that are supposedly thought to be effective for curing cancer, according to some literature review or based on previous experiments of the lab.

So far, all that I have described is the in vitro[25] phase. After in vitro, researchers may move to in vivo[26], if they get positive results, but if the results in vitro is not promising then they usually would not go to in vivo. In vivo typically starts by performing the same test that was performed in vitro, on rats. If they find that it also worked well on rats, then they can go further, by conducting toxicity test. Toxicity tests are performed in the clinical cancer research, not in the fundamental research labs (such as our lab). If a drug passes the toxicity test (decided not to be toxic), it can then be administered to humans, before pharmacologists may seek authorization to commercialize it.

Lab Space

As we enter the lab, on the left we first find the secretariat and 2 offices belonging to 2 principal investigators of 2 other labs that belong to the pole. At the end of the corridor, there is a door that leads to an area where on the left we find an office of 2 post doctoral researchers belonging to our lab and on the right there are 2 offices that also belong to post doctoral and doctoral researchers, from our lab and from the cardiology lab (a number of office switching has happened, though). Also on the right side there is a space arranged to make experiments on rats. Most of the time, I observed experiments on the rats' hearts, performed by a technician from the cardiology lab. I

[22] The development of secondary malignant growths at a distance from a primary site of cancer (Oxford dictionary).
[23] A unit of heredity which is transferred from a parent to offspring and is held to determine some characteristic of the offspring (Oxford dictionary).
[24] Any of a class of nitrogenous organic compounds which have large molecules composed of one or more long chains of amino acids and are an essential part of all living organisms, especially as structural components of body tissues such as muscle, hair, etc. (Oxford dictionary).
[25] (Of a process) performed or taking place in a test tube, culture dish, or elsewhere outside a living organism (Oxford dictionary).
[26] (Of processes) performed or taking place in a living organism (Oxford dictionary).

witnessed experiments on rats for cancer research, on a couple of occasions. At the end of the first corridor, if we turn right we would first find the freezer room. After the freezer room on the left there is a room, where researchers usually prepare the medium (if it is not purchased) or to add more ingredients to it. Next to this room comes the lab director's office, and then the cafeteria where I usually sat, particularly to conduct my interviews. The cafeteria is where the personnel have lunch together. Each of them usually had a sandwich or a whole meal that they bought or brought from home. Then there is the meeting room. When we turn left, we immediately find 2 offices for post doctoral and doctoral researchers from our lab and from the cardiology lab together. On the right side there is a room equipped with electronic microscopes connected to computers that enable researchers to observe cells and to take color photos. Also on the right side, there are lockers belonging to a couple of researchers. After that, on the left side there is a room dedicated to perform special operations; for example, removing the DNA[27] or the RNA [28] from the cells, for different reasons and experiments, after destroying the cells membranes. The operation can be achieved manually or using what they call the Maxwell machine. The next room is prohibited for persons not conducting research, because within it usually experiments causing hazardous radiations are conducted. Next to it on the left there is a room that makes the end of the corridor. The room is used to conduct experiments involving what they call western or southern blot. Western blot[29] is a process that enables researchers to determine the protein expression. Because proteins are otherwise unobservable, the process enables researchers to detect them and identify their density. Southern blot[30] is a process that enables researchers to analyze the RNA. At the end of the corridor, if we turn right, there is a storage room on the right hand side and a small animalia, where rats are stored in cages. On the left hand side there is the culture room, in which cell culture works described above is performed. After the culture room there is an office for the technicians from the 4 labs of the pole. If we keep going past the technicians' office, we will get back to the entrance again, because the shape of the corridor is circular.

[27] Deoxyribonucleic acid, a self-replicating material which is present in nearly all living organisms as the main constituent of chromosomes. It is the carrier of genetic information (Oxford dictionary).
[28] Ribonucleic acid, a nucleic acid present in all living cells. Its principal role is to act as a messenger carrying instructions from DNA for controlling the synthesis of proteins, although in some viruses RNA rather than DNA carries the genetic information (Oxford dictionary).
[29] An adaptation of the Southern blot procedure, used to identify specific amino-acid sequences in proteins (Oxford dictionary).
[30] A procedure for identifying specific sequences of DNA, in which fragments separated on a gel are transferred directly to a second medium on which assay by hybridization may be carried out (Oxford dictionary).

3.2.3 Data collection

Out of the indefinite number of questions that I asked to my informants, I finally identified 3 cases of trivialization. As explained below in this section (through observation), I suspected that these 3 cases can make promising research tracks that enable us to address the current research's main question. The 3 cases were then singled-out and taken to the next step, which is interviewing (as outlined in section 3.2.4 below). Ultimately, these 3 cases or research tracks have evolved into 3 chapters, in the current research. Each of the 3 chapters will provide a synchronic (cross-sectional) and a diachronic (longitudinal) discussion on the corresponding case. The 3 cases, which will be thoroughly discussed in section 3.3, are encapsulated in the following 3 questions: (a) why 5% of co2 for cell culture? (b) How appropriate is the medium usage? (c) Do you own your research? The remaining part of this section will address the following 2 concerns: (a) why are these 3 cases considered to be instances of trivialization? And (b) why those 3 cases in particular, were selected out of the rest of questions?

(a) Why are these 3 cases considered to be instances of trivialization?

I suspected these issues to make instances of trivialization, because prior to my raising of the related questions, the lab members did not seem to have discussed these issues. Details on how each issue came up to the surface will be provided in the background sub-section of each of the 3 cases: subsections 3.3.1; 3.3.5 and 3.3.9, respectively. In these sections, I described the participants' reactions illustrating that my questions constituted real instances of Garfinkel's (1967) breaching experiment. It is perhaps quite logical to expect trivializations in areas of matters that have never been discussed in the past (unnoticed conditions) more than those discussed (possible or partial neglect). More precisely, I suspected these issues to be instances of trivialization; because on the one hand they seem to be very important for the lab members (as they are substantial in every research and almost never change), and on the other hand they were never problematized (at least through observation and based on the overall reactions when I first raised these questions). To go back to the paradoxical feature of triviality that I discussed in the theoretical part of this research (sections 1.4 and 1.5), I considered these 3 issues to be cases of trivialization, because they oscillate between 2 extremes: being very important and being completely neglected.

A clear distinction has now to be made between who has the 'legitimacy' to say whether this issue is or not *trivial* and who has the legitimacy to say whether this issue is or not *trivialized*. It is definitely the lab members who are entitled to say if this or that issue is or not trivial, for the most obvious reason that they are the informants of this research. They are specialized in their fields and supposedly have more competence in their domain than me. However, it is me- being methodologically well equipped and epistemologically well situated- who is more entitled to say whether or not that issue has been trivialized. In other words, it is through the method of discourse analysis that this research will find out who is trivializing what, when, where, and how? Yet, in every interview, I did directly ask the informants if the issue in question has been trivialized. As I will discuss in section 3.3, the informants' different responses to this question will be taken into consideration in the analysis and will be discussed, but will not be taken for granted.

(b) Why those 3 cases in particular?

Before I ended up with 3 cases, I initially started with 6 semi structured interviews on 6 issues (added to an introductory interview that was aimed to be used for the consultancy services that I gave to the lab in return for giving me access to their work space). I managed to interview the 4 post doctoral researchers on the 6 issues, but I later decided to drop 3 and keep 3 for 3 reasons: the first reason is that I knew that the 3 cases would be just enough for a dissertation type of research. The second reason is that not all the participants, especially the lab director would be available to finish the 6 of them. I also realized half-way through that my interviews were time consuming for my informants. Finally, the third reason is that the other 3 dropped issues encompassed a number of questions that are not realistically applicable to the technicians and the master's student; and to a lesser degree to the PhD students. Here are the 3 other issues that I abandoned half-way through: 1) is cancer a disease? 2) why does cancer need to be researched at the cell level first? 3) do you publish negative results? Nevertheless, the issue number 2 did not disappear completely, but was evoked in the case number 2 of this study.

Now, with regard to why these 3 cases in particular, my choice was based on several criteria or reasons (also in line with Yin 2003). The most obvious reason is that we anyway have to make a choice out of a number of many other valid issues that I suspected to make promising research tracks. But the main criterion was the need to identify cases that are transposable, i.e. that we can later investigate in other labs, to see if they also apply elsewhere. It is always important to understand if one's research findings hold true in other settings. The second criterion, behind this choice is the participants' first reactions to my initial raising of the issue. Out of the indefinite questions that I asked, among the 3 cases that I singled out, the first (case 1) seemed to have triggered surprise and or particular excitement, right from the beginning. The second question (case 2) was said to be not trivialized and that it is one of their main and daily concerns; hence my question did not seem to be important. Case 3 was received with remarkably dissimilar reactions. Relations that made me think that the participants did not understand the aim of my questions. Hence, I tried to vary my choices based on the informants' different reactions. Finally, in a way or another, I happened to like the subsequent discussions around the 3 issues that I identified, and I felt that they were likewise very appealing to the informants, which made me decide to take them to the interviews. As a matter of fact, the conversations around those 3 issues were, at least to me, very enriching. They were in Eisenberg et al.'s (2010) sense, unique 'real-meeting dialogues', where 'mindful' discussions are practiced. More detailed information on how each issue first emerged and how it evolved into a whole case will be provided in the corresponding 'background' sub-section of data analysis; section 3.3.

3.2.4 Semi-structured interviews

I designed the interviews to respond to the research questions outlined in section 3.2.1 above. I interviewed the 10 lab members introduced in subsection 3.2.2 and detailed in appendix A.2, in the lab cafeteria, face-to-face, and using tape recording.

For every individual interview, I sit in front of one informant, at a time. A collective interview was also conducted with 4 post doctoral researchers and the 2 PhD researchers, at a time. Each time, I open my semi

structured interviews that I prepared in an excel sheet document, which I rename every time, using the name of the corresponding informant. I then start recording and write down in the space provided to the answer of the corresponding question, whatever I found it useful to deepen the interview discussions. Sometimes, I type a couple of key words that would remind me of ideas that I want to bring back to the discussion again, and sometimes I just write down anything as an excuse to avoid eye contact with the informants, whenever I felt that the interviewee was uneasy or intimidated.

Individual interviews were conducted partly in May and June and partly in September 2015. The collective interview was conducted in January 2016, after I have finished analyzing the individual interviews. While the individual interviews were meant to understand the (de)trivialization dynamic synchronically, the collective interview was initially meant to understand the dynamic diachronically. Almost 75% of the interviews were conducted in English. The two technicians (Marie and Dalila) as well as two post doctoral researchers (Dilara and Daniel) do not master the language enough to be able to do it in English, so they spoke in French. However, even for these latter cases, the questions were always asked in English first and then the rest of discussions were continued in French. In the appendices, transcripts were left in the original language, but every time I use or refer to a verbatim account, I translate it from French into English.

3.2.5 Transcription

Transcribing the interviews was performed using the Jefferson system of transcription notation, detailed in Attkinson & Heritage 1984. The full transcriptions of all the interviews around the 3 cases identified in section 3.2.3, are in appendices B1 and B2 (case 1), C1and C2 (case 2), and D1 and D2 (case 3). Following is the list of symbols, from Attkinson & Heritage 1984, used in our transcriptions:

(.) A full stop inside brackets denotes a micro pause, a notable pause but of no significant length.

(0.3) A number inside brackets denotes a timed pause (in seconds) that is significant to appear in the transcription.

[Square brackets denote speech overlap.

= The equal sign represents a continuation of the sentence.

< > Arrows like these surrounding utterances denote that the pace of the speech dropped.

> < Arrows like these surrounding utterances denote that the speech pace speeded up.

( ) Space between brackets signals that utterances were unclear and were inaudible.

(( )) Double brackets with a description inside mark some useful information that cannot be transcribed using the conventional symbols.

:: Colons mark that the speech or a particular sound is elongated or stretched.

→ A rightward arrow marks a particularly interesting utterance.

↑ An upward arrow marks a rise in intonation

↓ A downward arrow marks a drop in intonation

(h) A bracketed 'h' inside a word such as "certain(h)ly" marks a laughter within the talk

Underlining a word marks a rise in volume or an emphasis.

CAPITALS mark that an utterance was said either shouted or said loudly.

3.2.6 Coding

My interviews were already focused on the samples that I had selected (the 3 trivialized cases introduced in section 3.2.3 above). At this stage, I worked on squeezing my data into manageable parts, in a way that is crucially related to my research question: *How can the (de)trivialization practices be useful to know about knowing?* Therefore, I focused on textual signals of the different trivialization practices. First, I paid attention to verbal signs of trivialization such as the use of words similar to these: "no that's minimal"; "I don't ask myself these questions"; "it's not trivialized, it's something that's ignored" etc. Second, I categorized these practices into direct and reported trivializations. Direct trivializations are practiced by the speaker who expresses his or her own view (such as: "this is minimal"). Reported trivializations are those that are cited by a participant and said to have been practiced by others (such as: "people ignore these things"). Third, I focused on the speaker's account with regard to the instance of trivialization that he or she is citing. Fourth, I traced the trivialized matter throughout the text to see if it is

consistently or unpredictably trivialized. And finally, I traced the concomitance of the verbal trivializations with the non verbal practices, and reflected on the correlation of the two.

Based on the coding process, I ended up with a list illustrating the variability of accounts, across the lab members. The variability of accounts is an important axis in the analysis of trivializations, because if a matter is considered to be trivial and consequently there is no need to discuss it, we assume that the discussion around it would not result in any significant knowledge. But, if the interviews reveal that accounts are different from one person to another, then we suspect that what was considered to be agreed upon is in fact not. Hence, the trivialization appears to be hiding a disagreement, and therefore its scientificity may be at stake. Consequently, it is through this axis that we can access the scientific discourse (which is otherwise exclusive and highly selective) in order to spot non scientific accounts embedded within the scientific discourse.

By the same token, the coding ended up with a list of synchronic inconsistencies. These are contradictory accounts made by the same person during the same interview session. Similarly, synchronic inconsistencies are important to better understand how individuals construct knowledge or adapt their accounts on the go, no matter if they say that their opinion is educated or scientifically based. Contradictions were in cases admitted by the informants (if they step back and express that they are now figuring out another aspect of the issue and wish to modify their previously stated idea), but in other cases they were denied. Both contradictions are important, especially if relevant non verbal practices accompany them and are significant to understand what is going on. The Coding of the collective interview, which provides a diachronic analysis, is focused on how individuals changed their accounts over time, by comparing their initial account during the individual interview with their accounts in the collective one.

The variation of accounts (from one person to another) and as well as the synchronic and diachronic contradictions (made by individuals both in the same individual interview session and over time) were the 3 key

elements that enabled us to dig down the hidden dynamic of trivialization in context. Therefore, we were able to ask and answer the following questions: who is trivializing what, when, where how and why. Additionally, I paid a special attention to signs of power (covert and overt) that may be behind the different instances of trivialization. In line with the Discursive Psychology guiding principles, our subsequent analysis can borrow from the Foucauldian theory of discourse analysis, which focuses on power and the subjects, as we suspect that trivialization may partially be shaped by the hierarchy, statuses or other forms of power.

3.3 Analysis

This section contains 3 chapters. In each chapter, I will first synchronically analyze the series of the individual interviews I conducted throughout May, June and September 2015 around the 3 issues, identified in the sampling section 3.2.3 above. Then, I will diachronically analyze the collective interview that I conducted in January 2016, and which revisited the same topics of chapters 1-3 based on the obtained results of the synchronic analysis. I will open each of the 3 chapters with a brief background description on how the issue came up to the surface and how it evolved before it was taken to the interview stage. Then, for each chapter, I will analyze the corresponding individual interview series (each time, 1 from the 3 series included in Appendices B1, C1, and D1) and the corresponding collective interview (each time, 1 from the 3 included in appendices B2, C2, and C2). The analysis was based on the Discursive Psychology guidelines, particularly as described in Potter & Wetherell (1987).

Before moving to the actual discourse analysis (cases 1-3), we will now introduce the methodological tools that we used to approach our data. These methodological tools, which draw from the theoretical assumptions that we detailed in section 1.2 (in particular, subsection: Discursive Analysis), are the 4 major tenets of Potter & Wetherell (1987)'s Discourse Analysis, (a) through (d) below:

(a) Function: the analysis will be oriented into what people do with discourse. Words, sentences and utterances accomplish a function or a number of functions. Whether or not a particular function is intended by the speaker is not the focus of the method. Rather, the analysis focuses on the function discourse plays in context. People's intentions are from our standpoint virtually impossible to determine, even if they tell them themselves. Therefore, the analysis will shift the attention from the causality concern (the 'why' participants say what they say) to role that the account plays in context. Regardless of the formulation of discourse, people's accounts will be analyzed on the basis of what they effectively do. For example: order, request, persuade and accuse. Additionally, the analysis will take into consideration that a function is not always explicit, and that people are partly indirect (Drew, 1986). They may prefer not to say things, but to make them understood. They may also "want to head off undesirable acts, like rejecting, before they happen" (p. 33). The analysis will therefore depend on my own reading of the context, but

I will aim to be coherent and transparent, and I will use factual elements that can hardly be denied. Additionally, the analysis will not pretend that it is the only possible scenario, but will definitely enrich the discussion to understand the dynamic in question. In line with the functionality of discourse, the analysis will assume that "a person may wish to present himself or herself in a favorable light and to present someone else they dislike in a poor light" (p.33). Moreover, people may mean to be implicit rather than explicit, because explicitness may be less persuasive: "to present yourself as a wonderful human being to someone, you perhaps should not say 'I am a wonderful human being', but you might slip into the conversation at some 'natural' point that you work for charities, have won an academic prize, read Goethe and so on" (p.33).

(b) Variability: a person may vary his or her account over the same object and is not expected to be constant or consistent, because at different moments, he or she has different functions to do with words. Consequently, my analysis will be attentive to these variations of accounts during the same interview session (made by the same person) and over time. Variation is a very delicate issue in our analysis, because we on the one hand see it as an expected human practice, and on the other hand if the account is regarding a scientific matter (cancer research, in our case), then that would signal an issue for the scientificity of the account. In other words, if the issue that I raise, turns out to have been considered as trivial prior to my research, because "it's an already known thing", then we would expect a precise and invariable scientific answer. But, if variability comes up, then that is either a sign that the object "is not an already known thing" or that participants want to achieve different functions at different moments, even at the expense of scientific exactitude. For both cases, this has a consequence on the research that they are doing, and on how they collectively build knowledge.

(c) Construction: in line with social constructionism, the method assumes that people use existing linguistic resources to construct their accounts, in the way they do to "construct houses out of bricks, beams and so on (Potter & Wetherell 1987, p. 33-34). The construction implies active selection of the linguistic elements, which means that people include and exclude, favor and avoid, use and omit things. Therefore, produced accounts are

expected to have a potent "consequential nature" (p.34), and this is how they contribute to the construction of reality. The construction is not necessarily deliberate or intentional, but can be emergent. In other words, ''a person may be just 'doing what comes naturally' rather than intentionally decide that this than that form of language would be appropriate" (p.34). Consequently, the relationship between function, variability and construction is the following: people use discourse (a) as a tool to achieve functions; (b) functions vary according to the contextual factors; (c) the variability requires that discourse be constructed in such a way that it serves the targeted function. Furthermore, if 2 people construct their language in the same way, then this may be due to the fact that they are targeting the same function. Hence, just like variability is an expected practice, the consistency of accounts is not "a sure indicator of descriptive validity" (p.34).

(d) Discourse as topic: our analysis is not meant to read and foretell what is going on in people's minds, nor their beliefs and cognitive processes. We do not aim at finding out if the participants are or not telling the truth. We distance ourselves from the idea that there are underlying 'attitudes' that are constantly out-there to govern a person's 'behavior'. Rather, we are interested to analyze what is going on in discourse, in what context it is produced, how it functions, how it varies, how it is constructed and what consequences it makes (Gilbert & Mulkay, 1984; and Potter, Stringer & Wetherell, 1984). The analysis may be supported by the non verbal linguistic elements such as: repetition, interruption, overlapping, volume raising or dropping, speed raising or dropping, hesitation and the like. Again, the analysis will not use these non verbal elements (and neither the verbal ones) to establish cause effect relationships, but to describe how discourse is constructed. The expected result from this analysis is the emergence of linguistic patterns that would enable us to understand what is going on, and hence better understand situations and phenomena. If the patterns would be tested elsewhere, through transposition, then that would make a confirmation that our findings have some validity and reliability for future research addressing the same concern as ours.

Case 1: Why 5% of co2 for cell culture?

This chapter is closely related to Appendices B1 and B2. Appendix B1 consists of the series of the individual interviews' transcription; and appendix B2 consists of a collective interview transcription. The transcription lines are numbered, for convenience of reference. Verbatim accounts that I will use in this chapter will be referenced using their respective line numbers.

3.3.1 Background (case 1)

One afternoon, during my early weeks in the lab, I walked in the culture room and found Sophie, Stephan and Marie[31]. Sophie is not a lab member; she is a member of the cardiology lab that is using the space with our lab, as detailed in section 3.2.2. Sophie is a medical doctor and had worked as a physician in hospitals for 6 years before she joined the lab to do another PhD in cardiology. As I had the impression that they were very busy and not ready to hear questions from me, I decided to silently scan the room with my eyes piece by piece and try to figure out what each device, equipment or tool was for, starting from the door and ending at it. When I stood right in front of the incubator,[32] I noticed that it displayed the following 2 figures: 5% co2 and 37°C. I said to myself it seems that this 37° C is to maintain the cells[33] that are inside in the same condition as a in healthy human body; but what is this co2 level? I was trying to see how the gas tank was linked to the incubator, assuming that it was there to automatically feed it with the quantity of co2 each time the pressure within the incubator drops.

Sophie, who had generously been willing to explain things to me, looked at me and said in French: "that's for co2". I said "why these cells in the incubator need co2?"; she said "as in our body". I said "but why 5%?". When I said that, she looked away and I felt that the mood changed in the room, but could not tell what was wrong. It was just a matter of feeling. I later said that maybe my question embarrassed her in front of her colleagues, for a reason that I ignore. I had no clue to know in advance what kind of questions would be embarrassing for my participants. I

[31] Lab members are identified in appendix A, section A.2.
[32] An enclosed apparatus in which premature or unusually small babies are placed and which provides a controlled and protective environment for their care (Oxford dictionary).
[33] The smallest structural and functional unit of an organism, which is typically microscopic and consists of cytoplasm and a nucleus enclosed in a membrane (Oxford dictionary).

moved next to Stephan, who is usually very attentive to what I say and is always willing to talk with me or respond to my questions even when I do not address them to him in person; but this time he seemed too busy with his ongoing operation. I decided not to ask, not to disturb anyone. After some silence, Sophie asked Stephan, who said from his corner "I have no clue why 5%". Then, I deduced that in fact the silence in the room has something to do with my question, and possibly they were trying to find an answer to the question. Then, Sophie looked at me and said in French "it's shameful". I said "what is shameful?"; she said that they don't know why the incubator is regulated at 5%. I said to myself, if I will continue to embarrass people with questions, they will be annoyed and may be they will not facilitate my job anymore. But what to do when my job is to ask questions? I told her, "but no one knows everything". She said "yes, but this is important". I later realized why Sophie in particular was upset of the fact that she does not know the answer. Being a physician, she probably found that she more than anyone else should know how much of co2 human bodies would have.

I left the culture room and started to ask the question to each person I came across. So, after Stephan who was in the culture room, I asked Brian, then Daniel; that would make a total of 3 post doctoral researchers. Then, I asked Joanne (a master student) and Yassine[34] (a BS student). Then I asked Dalila, which would come to a total of 2 technicians (considering Marie who was initially in the culture room). The responses to my questions were quite similar in that they all first said something like the following: "I don't know", "I have no clue", "ah, that's a good question, but I don't have an answer, right know", but whenever I kept the discussion going they also evolved in a rather similar fashion. So after "I don't know", they say things like "but that is important for the equilibrium of the cells", "it's to maintain the cells in good condition", 'it's important for the phenotype" etc. When I insisted on the why and how, explanations started to be confusing and sometimes off topic. At that time, I did not use to write what they said immediately in front of them. I preferred to establish good rapport with the team members, even at the expense of providing precise accounts. But once alone, I used to write down what I could recall from previous conversations.

[34] Yassine is not in the list of our informants, because he is not a regular member.

Bottom line, on the first day where this issue was identified, none of the lab members was able to explain why the incubator is regulated at 5%. At night at home, I looked for answers, in several websites and specialized forums.[35] I found that no matter what the exact answer was, the co2 was said to be used in the incubators to maintain the acidity[36] or the pH[37] level, in the medium,[38] where the cells are cultivated[39]. The next day, I reopened discussions with everyone I came across, and surprisingly many of them happened to provide the same answer that I found on the internet, the previous night. I started to hear things such as "yeah that's normal, because without co2 the pH level would drop", "yeah it's related to the acidity of the medium". It was embarrassing for me to ask the participants whether they found this answer on the internet the previous night or it was a sudden improvement. Instead, I pushed the discussions further by saying "ok, but why 5% in particular?" and "who decided that?". At this level, all of them said in a way or another that it was tested in the past and that was part of the basics that research at this level should not rebut.

By and by, discussions started to become interesting for me and apparently for most of them. Stephan and Brian in particular seemed to like my questions, as they gave me much of their time. Every time they saw me in the cafeteria, where I usually sat, they came and opened discussions that could easily take more than one hour. So, after why 5% of co2 was responded to with utterances such as "it is related to the ph level", my question evolved into why 5% in particular and not more or less? And from there the issue exploded into a number of other questions, especially "does the human body also have 5% of co2?" and "Are there other side effects that could happen to the cells, if the 5% is not the best choice?". After a couple of months of discussions over the same issue, I had a feeling that none in the lab had exact answers to my questions, that each and every person was building knowledge over this issue while discussing with me and that answers were never constant, but were evolving all the time in discourse. It was a unique

[35] http://www.researchgate.net/post/Why_cell_lines_are_preserved_in_the_co2_incubator , http://molecularbiology.forums.biotechniques.com/viewtopic.php?f=15&t=13007 http://www.biology-online.org/biology-forum/about19373.html

[36] The level of acid in substances such as water, soil, or wine. Acid is a substance with particular chemical properties including turning litmus red, neutralizing alkalis, and dissolving some metals; typically, a corrosive or sour-tasting liquid of this kind (Oxford dictionary).

[37] A figure expressing the acidity or alkalinity of a solution on a logarithmic scale on which 7 is neutral, lower values are more acid and higher values more alkaline (Oxford dictionary).

[38] The substance in which an organism lives or is cultured (Oxford dictionary).

[39] To cultivate: to grow or maintain (living cells or tissue) in culture (Oxford dictionary).

case, where I could experience and observe discussions over issues that were initially said to be unknown, and in particular to see knowledge being built and strengthened in discourse. However, nothing was traceable without tape recording. It was an uneasy and awkward experience to genuinely discuss such very deep issues and write at the same time. At that time, I preferred to totally live the experience of discussing and to dive into conversations, rather than be bothered with writing and obsessed with “accuracy”. Therefore, I decided to conduct tape recorded interviews with all the lab members over the issue of the co2 level, in the incubator.

3.3.2 Interviews analyses (case 1)

I initially designed a semi structured interview, which I conducted with each participant individually, between June and September 2015 (Appendix B1). Then I conducted a collective interview in February 2016 (Appendix B2). While the individual interviews will be analyzed synchronically in the subsections 3.3.2.1 and 3.3.2.2, the collective interview will be analyzed diachronically in subsection 3.3.2.3. The individual interviews, have 9 questions each (numbered from 1 to 9). Depending on the participant's accounts in each question, further questions were asked to and responded to. The different interview questions were designed to invite the participants to account for the following issue: *Does the 5% level of co2 have negative consequences on research?* The collective interview (which also goes through the same 9 questions) is rather less structured, because the participants anticipated many questions. Nonetheless, I made sure that I went through all of them. The following is the list of the 9 questions:

1. Why is the incubator regulated at the level of 5% co2?
2. Have you heard this question before?
3. Do you think that this question has been trivialized or neglected?
4. Do other biology labs in Belgium and elsewhere also regulate their incubators at 5% co2?
5. Do you think that this question is important for your research?
6. Is 5% of co2 the same condition in the human body?
7. How do cells intake co2 within incubators, when they are constantly locked in containers or wells?
8. How true this is: "incubators are regulated at 5% co2, because that is how the desired pH level can be maintained"?
9. What other negative consequences can happen to the cells, due to this 5% level?

3.3.2.1 Accounts variation (case 1)

For the convenience of reference, the participants' accounts were reordered by question, not by participant. The aim is to easily spot and compare all the participants' accounts on each question. If we want to reorganize the

interview per participant, we can do so, by first selecting the participant initials (example RM) and second trace his or her numbered answers from 1 to 9 (example. RM's answers are from RM 1 to RM 9).

Why is it important to illustrate the variability of the participants' accounts? The shortest answer is because they reveal the disagreements over agreements. In other words, what is supposed to be largely agreed upon, at least in practice (i.e. the usage of the 5% level of co2) turns out to be problematic and disagreed upon (i.e. giving different accounts with regard to the consequences of taking this level for granted and trivializing its potential to skew experiments). What I wish to achieve through this section, is how the trivialized matters in practice hideaway issues of very important considerations for research, using the researchers' own words. The variation that interests us in this regard will be revealed through how the different lab members responded to the question about the usage of the 5% of co2 level, and in particular: (1) how they explain this practice (i.e. where it started, how it was decided and for what reason); and (2) how they account for the consequences of using this level in particular.

At the interview threshold

The first remarkable variation is related to how the participants accounted for the same matters before and at the beginning of the interviews. Brian, who had in the past not only said that he did not have a scientific explanation to the question on why incubators are regulated at the level of 5% of co2, and who had many times said that choices in the experimental sciences are sometimes made randomly and that many practices are taken for granted; provided almost opposite accounts during the interview. When I asked the first question during the interview, he laughed and looked at me in a way that I could only interpret as "again, you are asking this question?", which provoked my laughter as well. Then, he said in a rather determined way that there are different reasons "I think" (line 12): (a) "one thing could be to be a standard to have in every lab", (b) "the other thing is that they found this like a compromise or average concentration of co2 in the body", and (c) "with this concentration of co2 it keeps the pH at 7". Brian did not account for: (a') why standardizing something that is still problematic and is not suitable for all types of cells? Especially that all team members including himself agree that the human body does not have 5% of co2, everywhere.

He did not mention: (b') who "they" are those who "found this like a compromise"; especially that none of the lab members was able to tell me where I can find a reference (e.g. a journal article or a specialized book) on how this practice had started. Finally, as we will discuss in the next subsection, (c) if 5% regulates the pH at 7 in particular, what about the cells that should be kept at the pH level of 6.5 for example? Obviously, this very last question is also valid for all other lab members, but unlike the other members, Brian at this level was the only one who sees this condition as unproblematic, contrary to his pre-interview accounts. Brian, who also used expressions such as "I think", "could be" and "I think"; was unlike others arguing that there is a rationale behind the choice of 5% co2. He ended his response with "these are my (.)" without completing the sentence with something that could be "my own explanation" and restarted the sentence with a vague one "that's why" and ended his response there. In this case, *omitting* (to finish the sentence) is as significant as *telling*. Avoiding the sentence completion and restarting with "that's why", serves Brian to say that such explanation is a valid one, independently from his own opinion.

Stephan's response to this question also came as a surprise, not only because he drastically changed his account during the interview, as compared with his previous accounts, but also in the intonation he used to answer the question. He said "why at 5% co2? Huh (.) that's a good one" (line 3), as if he heard the question for the first time from me. More interestingly, when I told him that the recording was not on and that we had to restart the question, he repeated the same response in the same way "huh (.) that's a good one" again, as if he were an actor who had to repeat the same movie sequence and keep the same way of acting, especially in being surprised with the question that he actually heard me asking repeatedly, during the previous weeks. As we have detailed in section 3.3, and in line with our methodological assumptions, when people are asked a question, they may not only answer the question, but have their own goals to achieve. Therefore, their account has to have a function, and in order to accomplish the function they construct discourse contextually, which is why versions of discourse vary and differ. Lambotte & Meunier 2013, also discussed this variability, arguing that the narrator (a scientist or not) is not only telling the story, but also "acting" it, and hence co-constructing it in context. Consequently, the narrated version may be at any distance from the scientific physical reality. "The narrative does not mirror reality. It is a co-construction of it,

formulated by narrators, in accordance to their interactions with scientist-interviewers and in a given context" (p. 88). Epistemologically, we are not able to tell "why" Stephan 'acted' the surprise twice (in the first and second recording), but we can tell what function this surprise fulfills in the context, if we go deeper in his discourse. We will suggest a function for the acted surprise at the end of the next paragraph.

Although Stephan tried to provide a scientific answer to my question, he repeated "I think", "I'm thinking", "I guess", and "I'm not 100% sure, but I guess that's the main purpose". That is a way to say that he did not necessarily agree with the rationale of the answer. He rather was mentioning possible reasons that may be behind the choice of the 5% level of co2 in biology research labs, in general. Therefore, the account formulated as such enables him to explain things and list reasons, but not to adopt them. I recall that in our early discussions long before the interviews, Stephan (and also Brian) had to step back from what he had previously stated. Now, (a) acting the surprised (which tells us that his account is improvised and that he is trying to think aloud and therefore he should not be severely critiqued for any mistakes or false starts) and (b) not adopting the answers that he gives (by ways of "I guess" etc.), puts Stephan in a nice and comfortable position. Willingly or unintentionally, Stephan is using the strategy of explaining, but not adopting the rationale of the explanation, in order not to make contradictions or feel obliged to change his position during the interview, as he did before. But probably willingly, acting the surprised will give him more room to make mistakes without concerns, because after all anyone who is asked a question for the first time is allowed to make mistakes and to think loudly. He ended his response with "I guess that is the main purpose". The word "main" suggests that there are other purposes that he chose not to deal with at the moment; and the word "guess" enables him to say that he does not necessarily agree with this practice, but he is rather guessing why others decided it to be so. Hence the closing sentence: (a) leaves the door open for more elements of answers that he can later; and (b) confirms that his answer was just a matter of explaining without necessarily agreeing with the explanation.

Daniel, who said in his response to question 2 that he never heard the question before (not even from me), provided a firm answer to the first question on why 5% of co2, foregrounding that it is a norm that has been decided internationally “c'est la norme qui a été fixée au niveau mondial” (line 24). Intentionally or unintentionally, this answer puts him on the safe side, because if it is a norm that has been set internationally, no one would critique him on the fact that he did not consider this parameter for all the research that he has been doing. In other words, even if 5% turns out to be a wrong decision, it is actually not his decision, but an international convention that he has to consider. Paradoxically, Dilara will now mention an example where it was possible to use another level and not to maintain the 5%. In her response to question 1, Dilara- with whom I never discussed the issue prior to the interview and who does not conduct experiments in this lab- said that she does not know why incubators in the lab are at 5%, and that the incubators that she had used in a previous lab were at 10% and not 5% co2. Interestingly, although she said that she does not know about the current lab’s incubators’ co2 level: “u:::hh (.) ici je ne sais pas (.) forcément (.) je travaillais avec 10% de co2 ((laughter)) donc ((laughter)) (.))” (line 32), in her response to question 4, she said that she later knew that other labs in the world including the current lab use the 5% level: “aux états unis (.) ils étaient à 5% (.) ici c'est à 5% (.) et u:::hm (.) où est ce que j'étais encore? La culture cellulaire? À Paris” (line 524). Moreover, when I asked her why all incubators in the world are regulated at 5% she said (always in French) “I suppose it’s a way to standardize research”: “une façon comme (.) je suppose de standardiser la recherche (.) n’ayant j'ai jamais fait de (.) n'ayant jamais fait de 5% (.) je ne sais pas répondre (.)” (line 75). Thus, while Daniel- who was prepared- confidently said that the practice is due to an international standard, Dilara was clearly trying to figure it out as a standard.

Marie provided a rather technical answer to the question, and said that 5% of co2 has a role in the regulation of pH level in the medium (“uhm (.) ça joue un rôle sur le pH, ça maintient le pH ça tampo::: tamponne le milieu dans lequel les cellules vivent”). It was a crystal clear answer. Similarly, Dalila said “ça veut dire voilà ça évite les variations de pH’’ and “oui ça stabilise (.) ça évite les grands sauts de les grands sauts de pH (.) les grandes variations” (line 176), which I translate as “it avoids fluctuations in the pH level; it stabilizes the medium and avoids

the big shifts in pH". Likewise, Cédric and Gaelle have provided clear cut technical answers: "it's really linked to pH of the medium", and "it's to regulate the pH in the medium of the cells", respectively. On the other extreme, Joanne responded to the same question as "(laughter) THE question (laughter) (.) I don't know (.) I really don't know" (line 213). Joanne did not only say that she did not know, but said "THE question", which I could only interpret as "there you go, you are asking the same question again". Although I had discussed the issue with Joanne many times before, and she was present several times during my discussions with Stephan over the same issue, she preferred not to say anything about why 5% and insisted that she really does not know anything.

My surprise was bigger when most of the lab members replied to question 2 (did you hear this question before?) with "no", although my question had long circulated within the lab, before I started the interviews. Dalila had mentioned that all lab members are talking about me and my question, saying "qu'est ce qu'il veut avec son 5%?" (line 317), which I translate as "what is he after his 5%?". Joanne has told me outside the interview that she heard people discussing this issue. Even the lab director, who was always in his office, told me one day: "are you sure that your research is within the scope of Communication studies? I heard you are nearly going to do a PhD in cancer research, not in Communication". But the lab members said "no" to this question in different ways. Cédric, whom I interviewed only 3 days after he joined the lab and with whom I have never spoken about the issue before, put it like this: "no, I mean (.) yeah (.) I mean I'm not sure (.) I've never thought about it" (line 293). Marie said in French what is equivalent to "not (.) really (.) no": "non (.) pas vraiment (.) non" (line 325); but, because I told her "did you hear me asking this question the other day?" she said "yeah" I replied "You heard me, right? And it was the first time?" she said "yes" and laughed (line 333). Daniel, just said "no". Again, although I was sure that I spoke with Gaelle about the issue of 5% before and that she said that she did not know the answer (I had written this in my journal a couple of months before), when I asked her during the interview, if she heard this question before, she mentioned that it was a couple of years ago from a student that joined the lab for training. During the transcription, I kept repeating over and over again and again her sentence "when you arrived in the lab" (line 266), I replied with "when I arrived?"; she then replied with a noticeably higher pitch "ah NO NO NO". So I had to tell her "did you hear me ask the question to your colleagues before?" she said "no". Brian first admitted that he first heard the question

from me “yes with you” (line 227), but when I told him “no I mean before me” he said “u::hmm (.) during my PhD we had a discussion like this but it was not exactly this question but we were talking about […] = co2 and this (.) co2 and the regulation of pH and the medium (.) so it was important to keep the ph at 7.4” (line 229).

5% of co2 for a 7.4 pH or for any desired pH?

Paul, with whom I had not spoken about the co2 issue or any of the other trivialized issues that I identified, but who was the only one who knew in advance that my research was going to be about *trivialization*, was considerably and noticeably cautious in answering my questions. He spoke very slowly and put his words very carefully. He said that 5% of co2 is used “in order to buffer the medium so that we are sure that the medium is always around 7.4, which is the physiological pH” (line 114). It was a problematic account, because when I reported this answer to Joanne, she told me “I don't know if it is real or not, but we (.) like here we are culturing cells no[t] only in pH 7.4 but also 6.5 (.) so if they’re saying that 5% co2 is desired for cells with pH 7.4 (.) so what about the cells with 6.5? See what I mean?” (line 1351). Joanne insisted on 2 matters: that she does not know why 5% and that 5% cannot be used to maintain the 7.4 pH level in particular, because 5% is also used with cells of 6.5 pH. She repeated “so, if it is required for 7.4 (.) pH 7.4 so I don't think it's good for cells of pH 6.5 (.) see (.) but if it required to just maintain the pH in that level stable so I think (.) I think this is the right” (line 1377). When I rechecked the different answers, I found that Paul the lab director and 3 out of the 4 post doctoral researchers (Stephan, Brian and Daniel) were the ones that answered that 5% of co2 was to maintain the pH level at 7.4, whereas the 2 technicians, the 2 PhD students, in addition to Joanne (the master’s student) were the ones that said that 5% was to maintain the pH level no matter what the level is and not necessarily 7.4.

Added to Paul’s answer mentioned in the previous paragraph, following are 3 more responses that explicitly considered that 5% of co2 was to maintain the pH level at 7.4, in particular. Stephan said “I'm thinking it's a value that is chosen (.) it makes the conditions in such a way that the culture medium is maintained physiological (.) pH which is normally around 7.4 something like that” (line 6). Brian said “with this concentration of co2 there is the

bicarbonate in the medium that u::h (.) with this concentration of co2 it keeps the pH at 7" (line 17). Daniel said almost the same thing in French: "donc 5% de co2 combiné à (.) là en fait avec la concentration bicarbonate dans le milieu (.) ça fait un pH qui est physiologique (.) un pH 7.4" (line 25).

Conversely, in line with Joanne's idea (that 5% of co2 maintains the medium's pH at any desired level and not specifically at 7.4), following are more responses that were made by the 2 technicians (Marie and Dalila) and the 2 PhD students (Gaelle and Cédric), respectively. Marie said that it plays a role, it maintains the pH, it buffers the medium where cells live: "ça joue un rôle sur le pH (.) ça maintient le pH ça tampo::: tamponne le milieu dans lequel les cellules vivent''. I wanted to ensure that I understood Marie very well, so I asked her explicitly if the 5% level is to maintain the pH at 7.4 specifically or at any level; she confirmed "at any level". Following is the original exchange in French: "mais est ce que c'est pour maintenir le niveau du pH quelque soit le niveau ou bien le niveau de 7.4?" ; she replied: " Quelque soit le niveau" (lines 1322-24). Dalila said it was a percentage used generally to keep the medium buffered "c'est un pourcentage généralement utilisé afin de garantir uhm (.) que le milieu reste tamponné" (line 168). I asked Dalila if the level is to maintain any desired level; she said yes (lines 1299-1300). Gaelle said "It's to regulate the pH in the medium of the cells" (line 85). Finally, Cédric said: "at least I'm confident it's really it's really linked to pH of the medium. The cell are growing (.) so if (.) if the pH change in the solution then the cell can shift in metabolism or just die (.)" (line 100). Now, we clearly have 2 opposite accounts, the one (made by the lab director and 3 post doctoral researchers) states that 5% of co2 is used to maintain the pH level at 7.4 in particular, and the other (made by one master's student, 2 technicians and 2 PhD students) states that the 5% of co2 was to maintain the pH level, and not necessarily at 7.4, but at any desired level.

Further discussions with Paul have made the following come to the surface: the 5% level of co2 regulates the pH at 7.4, but because tumor cells are usually at different pH level, there are other ways to regulate it and not only through the co2 level. He said "a tumor pH is usually measured below 7 and it's usually between 6.5 and 7 so the culturing cancer cells at a pH which is buffered (.) which at 7.4 like in many labs you you you (.) you probably lose part of the part of the of the of the information (.) so it is clearly uh uh underappreciated (.) and this is why we have shifted the way we we do we regulate the pH in our cells

using all the kind of buffers not dependent on co2 which allows to uh to fix the pH at 6.5 and to have cells (.) tumor cells growing in the environment which is normally the environment in vivo" (line 431). Therefore, Paul who said that the 5% is good for the 7.4 pH, realized that the level is "underappreciated", because they each time need buffers to achieve the other levels, as the 5% by itself does not solve the issue. He also said that in order to reach 6.5, they need to figure out other solutions, but not to think of changing the 5%.

Stephan started to realize the importance of the issue and said "for example we're working on colon cells, and colon cells normally at least different layers in the inside, they are growing in more acidic environment like with lower pH it could be 6 or something that is much lower than the general pH of 7.4 (.) so maybe for these colon cells actually, it'd be beneficial to have a different co2 level" (line 794). Clearly, Stephan's response shows that the only way to regulate the pH level to 6 is through another co2 level. Similarly, Brian whom I asked if it is important to rethink this level of 5% (question 5) said "there is some (.) some research that involves the changes in pH in the body or acidity or (.) I think this could be interesting to modify the co2 concentration" (line 604). Again, for Brian the way to change the pH is to modify the co2 level.

Daniel made it clear in French that because they are currently using the 5% co2, which regulates the pH at 7.4; they are not necessarily mimicking the human body. The result is that some drugs, which they are testing and which may respond to other pH levels, may not respond to 7.4. Consequently, with this level, they may be overlooking research results: "ça c'est mon sujet de post doc (.) le pH aussi a un impact sur l'effet de certaines drogues qui sont pas forcément acides à pH 7,4 dont les pH acides sont utiles (.) donc c'est clair que dans les conditions normales de cultures on ne voit pas l'effet (.) qu'on va dire la drogue n'a pas l'effet, et on met un pH acide, a un effet (.) c'est en changeant un peu les conditions de cultures qu'on va essayer de trouver (.) mais (.) clairement (.) on passe surement à coté de certaines choses dues au fait qu'on des conditions de cultures standards" (line 879).

Now, I will retrace back the variability of answers around the appropriatness of the 5% level of co2, in the following way: (a) when I first asked the question (prior to the interview), none of the informants that I asked

provided a scientific explanation. I had rather received agnostic responses such as: “I don’t know”. The next day, (b) each and everyone I came across said that it was actually related to the pH level in the medium. During the interview, (c) Carole was an exception, as she was the only one who said that she did not know why the incubators are regulated at 5% in this lab and that she had conducted her PhD research using 10 % as co2 level. Joanne, Marie, Dalila, Cédric and Gaelle said that the 5% is to regulate the pH at any desired level whereas Paul, Stephan, Brian and Daniel said that the 5% is to regulate the pH at 7.4, specifically. Then, from the latter group, (d) Paul said that the issue is underappreciated and that they are looking for other ways- not through co2- to buffer the medium for cells that need other pH levels. However, (e) Stephan, Brian and Daniel realized that the standardization of the co2 level may be negative for cells requiring other pH levels; and that there may be a need to change it. And (f) the former group (who did not know that the 5% of co2 is suitable for a pH of 7.4 only) did not know that the other pH levels are being achieved through the different concentrations of buffers. Now, in terms of consistency, all the participants immediately focused on the pH stabilization to explain the use of the 5%, and no one tried to explain this use from a broader perspective. Only when I explicitly solicited the participants to account for the appropriateness of the 5% level- regardless of the pH level (question 9)- did they start to account for such a concern. Each participant’s reaction to question 9 will be detailed in the next section (section 3.3.2.2).

3.3.2.2 Synchronic inconsistencies and contradictions (case 1)

In this section, I will shed light on a number of inconsistencies and contradictions related to the skew potential of the usage of the level of 5% of co2. I will focus on the oscillation between the two opposite views made by the same person at a time (during the same interview session). The analysis will examine what motivates this position and its opposite, each time. I have outlined in my methodological assumptions (in section 3.3) that: (1) people may make contradictory and inconsistent accounts, because they may use language to achieve different goals at different times, depending on the contextual factors of the ongoing conversation. They may not necessarily focus on truth or on accuracy more than on what they want to achieve through language use (ex. face saving). (2) Additionally, people may not want to appear as inconsistent. They may do what they can to save face and avoid being seen as dissonant or inconsistent (Festinger, 1957). Taking these two elements into consideration, our analysis will reflect on the scientificity of our participants' accounts. It will reflect on to what extent we can tolerate a scientific discourse to be variant and inconsistent? By the same token, outlining inconsistencies is a way to sketch how our scientists (participants) build logic and explain things. It is a unique way to inductively and interpretatively: 1) identify knowing patterns during explanation, 2) capture areas where scientific reasoning is interwoven with common sense opinions (Latour 1987), and 3) identify discursive strategies used by the participants to avoid revealing the inconsistencies made.

Can we forget about the pH regulation for a moment?

Paul said that he did not know where this practice started: "I could not say (.) I could not say where you could find the information" (line 122) and that he has never been asked by the students on why 5%. In his response to the question whether he has heard this question before he said: "No (.) the question is usually from the students why co2 [not why 5%, in particular]" (line 300). Furthermore, Paul said that this practice has been in place for at least 50 years and he did not even know whether it started in Biology at large or in oncology, in particular. He said "first people developed the medium that we use (.) uh they only wished to find the best medium to get the cells in culture (.) so they they (.) they manipulate arbitrarily the the medium the concentration of neutral amino acids and also pH

through bicarbonate in order to to fit the the (.) so I've never been in contact with the original publication explaining why 5 instead of 10 or the (.) or 1". This is quite enough to demonstrate that Paul has not considered discussing this issue before. However, Paul responded to my question on why 5%, saying that this level is "right", because "(.) there is an equilibrium between the co2 in incubator and the bicarbonate in the medium, which offers you the right buffer system to have the cell culture culture at 7.4" (line 117).

Paul has perhaps developed his account about this issue during the interview session, until he reached a conclusion on how this 5% level has been established. His conclusion is that the 5% level is being used, because the medium that was first made available in the market (about 50 years ago) required that level "(.) 5% is is a norm which is uh dic (.) I would say dictated by the the:: the current medium available commercially" (line158). Paul hesitated to put the word "dictated", but ultimately uttered it. He had already anticipated this idea a couple of minutes before, when I told him that Carole had worked with 10% and that all lab members were surprised that she had done so. He said: "I (.) I'm (.) I know that uh in the past they were (.) uh certainly (.) the 5% rule was not that (.) uh present I know that you could read from publication much more before than 10% was also a possibility (.) why is that now 5% in the medium is also […] they are they are selling (.) more companies with (.) selling medium now and and (.) and the way these medium are now available with strict concentration of everything (.) uh they are sold with an optimal of uh of uh of pH being obtained with 5% co2 (.) so it's more commercial prescription from the medium and the 10% may be historically able to have something which wasn't much more buffered and and wanted to have something with a stronger buffering capacity (.) and they were with 10 but I think that's the the the (.) the reason" (line 137). Therefore, up to now, we understand that 5% is dictated by the medium available in the market and again if it is suitable for buffering, it is so only because it is compatible with the medium that is available in the market. The next inquiry is now whether the 5% can have other negative effects. I understood that this level could be suitable for the available medium (again only when it requires a pH of 7.4), but does it have other side effects?

At the level of question 3, I had already asked Paul to account for the appropriateness of the 5% level (line 427); and he expressed his concern that their "core" business is to mimic the in vivo conditions: "well it's (.) it's part of the core business of our research to consider that specifically in the field of cancer (.) uh a tumor (.) in (.) a tumor pH is usually measured below 7 and it's usually between 6.5 and 7, so the culturing cancer cells at a pH which is buffered (.) which at 7.4 like in many labs you you you (.) you probably lose part of the part of the of the of the information (.) so it is clearly uh uh underappreciated" (line 430). However, Paul did not mention the side effects of the 5% level itself and whether or not this condition is the same in the human body. He rather went back to the issue of the pH mentioning that with 5% we would have a pH of 7.4, which is not what we always need for research on cancer cells.

Now, because I noticed that every time I mentioned the 5%, every lab member immediately linked it solely to the pH regulation, I had to formulate my question in a different way (line 1667). I had to say explicitly that I understood the role of the 5% level for the pH regulation, but that I wanted to know about the appropriatenss of the 5% level as such. This question triggered the longest answers and finally it yielded to almost completely opposite accounts (compared to the one mentioned in the previous paragraph; line 430). Paul, after 22 seconds of silence, responded to question 9 with what he called a "preamble to the response" where he mentioned that the most important thing for them is to keep the cells alive and to provide them the environment to split and proliferate. Then, he said that "this being said I think that 5% co2 is something which is part of the (.) part of a model not being perfect uh I may assume that uh that the the the co2 (.) could (.) uh the co2 concentration could (.) influence the (.) could nuance the the the (.) uh the way that the tumor cells behave (.) uh if you compare them with with what it would be in the (.) in in vivo" (line 1667). So, for Paul, the 5% level could "influence" the way the lab's tumor cells behave (because they are not being given the same level as they would within the human body), but we have to remember the priority that he mentioned in the preamble. In order to satisfy the priority that he mentioned in the preamble (keep them alive and get them to proliferate in the medium available commercially), we have to accept the constraints of what is available commercially, as he said in his response to question 1.

Paul ended his answer with: “so for me it’s not contributing to a big shift in the phenot[i]pe in the phenotype, but the influence is that can we consider there is a minimum uh to be acceptable at”. The conclusion that Paul made is that there is a negative consequence, but we have to accept it, because that is what is available in the market. Then, I asked if this “minimum” that they accept would be more serious if it is used everywhere in the world and for every single research on cancer: “there is no doubt that this can be causing some sort of bias for all the research?” (line 1684). At this level, Paul said that “it’s minimal (.) it’s minimal” and justified his answer by mentioning that drugs that work in vivo would work in vitro under the 5% condition: “so if you take a drug, which is in the same way as when this chemotherapy is uh is administered to a patient so (.) if you take a drug which is not working uh in a in a patient because it does not express the right receptor against which this drug is is uh directed (.) if you do the same in vitro (.) in cells incubated at 5% co2 (.) the drug will not route”. Yet, the problem is actually not for the drugs that are successful in vitro and get later tested in vivo (as I will explain this in detail with Daniel’s account, starting from the next paragraph). The real problem is in the drugs that usually do not pass to in vivo, because they were not successful in vitro. So, if the 5% condition would cause drugs to be unsuccessful at the in vitro, they would not go to in vivo, as they would be reported to have failed in killing cancer cells. It seems that Paul too has noticed this later idea during the explanation, which made him finish the explanation, before completing the opposite case (drugs that do not work at the in vitro). He dropped the explanation and said that “so this has been validated and therefore for (.) 90% 95% of the research on going this kind of model (.) uh with 5% co2 not reflecting the reality uhm (.) is (.) uhm can be considered as uhm as sufficient to to to draw a conclusion” (line 1692). Clearly, at this level, Paul is using the legitimacy of the figures: because 95% of researchers do that, so we have the legitimacy to do that.

How can we accept to standardize a potentially skewing condition?

To begin where Paul ended, *Daniel* provided an opposite account. He said in French that the 5% of co2 level itself (regardless of the pH regulation) may cause that the research would fail to find the therapeutic effects of the drugs they are testing and to mimic the physiological conditions of the human body: “ben une conséquence négative c'est que peut (.) on en a parlé (.) on peut passer à coté de l'effet de certaines drogues (.) on peut être à coté des

conditions physiologiques du corps humain (.) voilà c'est un biais de la culture (.)" (line 1556). This is the other half of the story, which Paul dropped and which we referred to in the previous paragraph (i.e. when experiments fail and are quit at the in vitro level). On another occasion, Daniel said again that if a specific type of cells on which they work would happen to have another level of co2 in the human body, there will certainly be losses, and they would fail to achieve findings: "de toutes façons on peut dire plus l'un dote d'un taux de changement dans l'environnement ne serait ce que le co2 (.) le pH aussi (.) ça c'est mon sujet de post doc (.) le pH aussi a un impact sur l'effet de certaines drogues qui sont pas forcément acides à pH 7.4 dont les pH acides sont utiles (.) donc c'est claire que dans les conditions normales de cultures on ne voit pas l'effet (.) qu'on va dire la drogue n'a pas l'effet (.) et on met un pH acide (.) a un effet (.) c'est en changeant un peu les conditions de cultures qu'on va essayer de trouver (.) mais (.) clairement (.) on passe surement à coté de certaines choses dues au fait qu'on a des conditions de cultures standards". And on a third occasion, he said that we would clearly conclude that the drug has no effect: ''mais non clairement fin c'est sûr qu'on va conclure que la drogue n'a pas d'effet'' (lines 873 through 887).

The discrepancy between Daniel's account and that of Paul is not just in the fact that the former admits that 5% of co2 has negative consequences while the latter said that it was minimal. It is way beyond that contradiction. Daniel, who said towards the end of the interview that 5% can have serious negative consequences, he himself said earlier that we should not change the 5%, because we need to standardize research: ''oui c'est en fin (.) je pense que c'est quand même important de savoir pourquoi on place à 5% (.) uhm donc la discussion après uhhhm de toutes façons là on ne changera pas notre façon de faire la culture (.) voilà c'est placé à 5% on sait pourquoi tout le monde utilise un pH à 7.4 pour les cultures sauf (.) des conditions particulières (.) oui ça c'est bien de [''. At the begining I doubted that Daniel meant that it is not him who considered that the 5% condition has to remain in place, when he said: ''Je pense c'est (.) c'est vraiment des conditions standards que (.) voilà pour les gens (.) c'est un incubateur à 37 degrés 5% de co2 (.) voilà c'est des conditions que tout le monde utilise (.) voilà, il ya plus de remise en question entre guillemets de ces conditions (.) c'est accepté partout''(line 385). When I asked him if this condition is everywhere in the world he again said that yes and that it was a good idea because it helps research to avoid the

unnecessary “bias” or variability: “c'est standardisée donc ça évite des biais qui sont dus juste aux conditions de culture” (line 513).

Because Daniel insisted on the importance of standardization in their research, I tried by all means to tell him that I did not understand why he was interested in standardizing a condition that he himself said to be quite negative (lines 888 through 924). He said that they need to culture cells in the same way, in order for another researcher to repeat his experiment and have the same conditions: “oui mais après il faut aussi (.) qu'un moment donné il faut se dire (.) ce qu'on utilise (.) ces conditions là (.) c'est que tout le monde applique on peut pas (.) nous après bon on sait plus comparer à ce qui a été obtenu dans le laboratoire d'à coté ou chez nous, ça devient difficile par ce qu'il y en a un qui ont travaillé sur 5% de co2 l'autre à 2.5 l'autre à 10% (.) et après c'est difficile de (.) s'il y en a qui disent ben nous on a une drogue qui qui agit (.) nous on va tester (.) ben non elle réagit pas (.)” (line 889). Basically, for Daniel it is better to use 5%, although he thinks that it leads to wrong results (as we have detailed in the previous paragraph), just for the sake of allowing other labs to reproduce the same experiment using the 5%, if they want to check his own results or replicate his research.

Morevover, what he failed to explain, is that even if he does an experiment with any other level of co2 (that he would decide to be suitable for his cells), the other labs can still replicate his experiment, using the level that he would mention in his research protocol. Obviously, Daniel is right in one thing when he said that one lab or one piece of research cannot test everything in one single research: “ben (.) c'est un moment (.) on peut pas tout tester” (line 910). I told him that I understand that and that I totally agree, but that I am not suggesting that he or the lab should do that. I said that we are speaking about what is better and feasible for research in general. He insisted again that standardizing is better than working at different co2 levels, without justifying the standardization.

Standardize or not standardize: that is the question

In his response to question 3, Stephan was already unfavorable to this idea of standardizing conditions that may be inappropriate for all research: “well it's like standards so you have to put it there (.) so you don't want to: (.) people don't want to I think to waste their energy by spending and thinking on something as this because it’s (.) it’s not the key issue that they want to address at the moment (.) it’s just not one of the parameters that is required (.) It's some standards I think (.) It’s always like that so why would you change it (.) And if they don’t change it (.) they don’t have to think about it” (line 354). Yet, when I told him that Daniel preferred standardization than to think of another co2 level, although he (Daniel) mentioned the drawbacks of the 5% level, Stephan tried to defend Daniel’s account, but then changed his account dramatically. He first said: “yeah because people like to (.) hhhhh how to say that (.) in research (.) the world like (.) because everybody used it as this one (.) it's a kind of standard setting. (.) and then if you have results (.) then other people can repeat those things which you made (.) because everybody (.) pretty much everybody is using 5% (.) so if they want to repeat the experiment they can just put it in the incubator and they don’t have to change anything except for a few parameters (.) so they can easily reproduce those studies (.)” (line 1438). When I said that I just cannot understand why people standardize what they think is or might be wrong, he said: “but in most research they normally find it much easier than say (.) well we (.) based on that idea we continue like this and use the same settings (.) so we don't have to revalidate these findings (.) it's much easier in that sense (.) Much easier to explain things if you find something of course against to quote other (.) it's a kind of (.) I don't know if this the word (.) may be it’s like laziness (.) maybe not exactly the right word (.) but it's just easier than to do it (.) to have it comparable against other people's findings (.)” (line 1447). Finally, and after I said this issue of standardizing is worrying me, because it causes that researchers make the same mistake over and over again, he stepped back and said: “you don’t have to keep necessarily on standardizing (.) or you can also just repeat your same experiment at two different settings and just compare and just report on that option (.) it’s just (.) often people they just copy and go with things that is already done (.) but I agree it’s not always the best thing to do” (line 1466).

Stephan had said that this issue of co2 is important, even before I asked him about the negative effects of the standardization itself. In question 2, when I asked “have you heard this question before” he said “a few times (.) sometimes (.)” and then added on his own: “I know it's important because if co2 is not functioning like the culture become effective (.) that's more like people know the necessity (.) but the exact role often they don't consider into detail” (line 221). When I asked him directly, if he thinks it was important, he said: “Yeah I guess so (.) it's an important parameter (.) everything that you're doing is (.) All your culture cells that you put in the incubator are affected by the parameters that are set (.) and may be your cells are (.)

requiring something different (.)" (line 593). And on a third occasion, when he answered question 9, he was even more willing to talk about the seriousness of this issue: "you might find if you set it at a different level (.) you can find some slightly different outcomes of your studies (.) so yes it could (.) it could have big implications (.)" (line 1387). Stephan was consistent all along the interview, with regard to this issue. For him: (a) this issue has big implications on research; (b) it has also been ignored "it's just something that's ignored (.) it's that people take things for granted"; (c) people do not question it again out of "laziness". Furthermore, Stephan went into deeper causes, by saying that (d) researchers avoid to deal with these general conditions and prefer to build on what is already there so that " (.) we don't have to revalidate these findings (.) it's much easier in that sense" (line 1456). This last explanation provided by Stephan is perhaps a reality that is related to how research labs are organized at least in this lab. Researchers, be them doctoral or post doctoral are all upset about findings, publishing and more importantly about their statuses. We will discuss this in case 3, but for now we wish to hint on the fact that perhaps sometimes researchers do not necessarily own their research and do not genuinely think about it. Rather, they prefer or feel obliged to follow the stream and not to take the risk to evoke issues that are not part of the mainstream debate.

Oscillating between important and trivial

Brian admits that this question is "now" important to reconsider. For him, this issue is now trivialized, but "in the past (.) they were discussing a lot a lot" (line 362), even though he did not say where this practice started. Furthermore, in explaining what could be behind such a trivialization, he said "you think that not all the people is wrong and they are just in the wrong (.) co2 concentration" (line 368). Basically, because everyone has been using it everywhere, researchers happen not think of re-questioning it again. This is in a way similar to Stephan's explanation, in the previous paragraph, with a slight difference: Stephan called this practice "laziness" or avoiding revalidating one's research protocol, whereas Brian somehow defended it using the justification of "not all the people is wrong". Furthermore, Brian, preferred to use "you" instead of "we" in his justification. To wrap up Brian's position: a) this issue did not start from nothing, but rather was discussed "a lot a lot" in the past (without explaining where and when); b) it is now trivialized, because now it is time to rethink it, but in the past there was no need, because these are "like a standard that is well established everywhere and in every lab you have always this set up" (line 365); c) we should not put the blame on those who do not re-question such practices, because we cannot think

of people to be all wrong; and finally, d) Brian used the second singular personal pronoun “you” instead of “we”, which may serve him to mean that such a logic is also true for “me” and not only him.

In responding to the question 5 on whether the issue is of any importance, Brian was increasing the interest little by little. From “could be” and “it depends” to “yeah sure”, here is how he formulated his first response: “could be (.) it depends on the topic you want to work on if you are interested to check the acidity or the medium or some some process that is regulated by the co2 levels in the body yeah could be an interesting thing (.) yeah sure” (line 599). When I asked why, he said that because there is some research that is now focused on understanding the consequences of changing the pH in the medium, which can be done through the modification of 5% of co2. However, when Brian responded to question 9, regarding the possible negative effects of the 5% co2 level, he said that it is as long as the cells are growing well and they are healthy, and as long as we are mimicking the human body then there is no problem: “the aim is to get them alive” and “the other aim is to be the more physiological possible”. When I reminded him that 5% of co2 is not the physiological level (and that it was everyone’s answer to question 6), and therefore with 5% we are not mimicking the human body, he said: “it’s not mimicking (.) but with the technology they had in the time it was the way” (lines 1479-1489). Although he based his account on the fact that as long as cells are alive and are kept physiological, there is no problem; when I told him that with 5% co2 cells are not even kept physiological, he then agreed that they are actually not; and that the problem emanates from the limitation of the past technology.

More importantly, Brian was in complete contradiction with Daniel, which we developed earlier in this subsection. Daniel was very sure that due to this 5% level, anti-cancer drugs may not work, and the consequence would be that much of the research would not go to in vivo and is abandoned at the in vitro level: “on peut passer à coté de l'effet de certaines drogues (.) on peut être à coté des conditions physiologique du corps humain (.) voilà c'est un biais de la culture (.)” and “(.) mais (.) clairement (.) on passe surement à coté de certaines choses dues au fait qu'on des conditions de cultures standards”. Now, Brian said “I don't think it should be a problem because you

have all the parameters are controlled (.) and for me it's not more challenging then in the animals" (line 1534), although the question was not about challenges of the instability of the variables (co2, oxygen etc.) in vivo, but rather whether the in vitro mimics the in vitro . More importantly, Brian has almost changed his account as compared to his previous account that we detailed in the previous paragraph, where he said that this issue has implications on the pH level, which is now a hot topic in their field and that "it could be important", "it depends" and that it is "yeah sure" important.

Dilara first admitted that this issue is of importance, because the aim of the cell culture is to mimic the human body, and if we do not do it in the right way, we would not know what kind of negative consequences we might have. For Dilara, if the incubator's parameters are different from those in the human body, then we would end up with different conditions, which would make the transposition to in vivo problematic: "ben pour moi la recherche sur des cellules dans des puits en état de culture c'est pour mimer ce qui se passe dans le corps humain (.) et donc si on fait varier les conditions de l'incubateur pour ben voilà on va s'éloigner du modèle animal (.)'' (line 1567). Paradoxically, she said she is still not sure if this condition would have implications on the research results: "ça pourrait être une raison (.) maintenant je sais pas si ça a un impact sur la recherche. (.) sur les résultats". Then, again Dilara repeated that this condition may have negative consequences on the transposition of the research from in vitro to in vivo: "ça pourrait avoir un impact sur la transposition des résultats à l'humain (.) à l'humain ou à l'animal" (line 1578). I said: "a negative impact?"; she said yes. When I was intrigued by her position, I told her that I do not understand how on the one hand she said that this 5% co2 level is not the same in the human body, that this condition can have negative consequences on the transposition of results from in vitro to in vivo, and on the other hand they use this condition for all the research. At this level, Dilara said that she thinks that they usually do not think about such issues unless they become problematic. She said that unless the cells die or come off the dish (to which they are supposed to be glued), we do not think about it: "et donc à partir du moment où il ya un souci là on recommence à y réfléchir" (line 1587). With Dilara's explanation it becomes more and more clear that cell culture is no longer thought of,

unless a problem emerges. Cell culture, including the 5% level of co2, is one of those taken for granted issues, within the biology research practice.

To conclude this section, I wish to summarize what I have detailed so far: 1) that the 5% level of co2 has never been discussed before in this lab; 2) that is it a tradition (a norm, a pre-existing knowledge) in the field of oncology and biology at large and that according to the lab director it was "dictated" by business not by research; 3) that the participants initially did not know how to explain its appropriateness; 4) that they all later linked it to the pH regulation; 5) that some of the participants said that it was suitable for any pH level whereas others said that it was only suitable for the 7.4 pH in particular; 6) that none of the participants tried to account for the appropriateness of the level regardless of the pH; 7) that questions 5 and 9 in particular triggered new accounts regarding the suitability of the level; 8) that all the participants oscillated between two opposite positions: to think of a more suitable level for one's own experiment or to respect standards; and finally 9) all the participants have in a way or another accounted for the possible negative outcomes of the standardization of the level.

3.3.2.3 Diachronic inconsistencies and validation (case 1)

I conducted the collective interview for 3 good reasons: a) to validate a number of conclusions that I have so far made in my previous analysis regarding the individual positions taken during the individual interviews; b) to explore the evolution or change of positions over time, regarding the different issues related to the usage of the 5% level of co2; and finally c) to compare the (de)trivializing accounts over time. The transcription of the the collective interview is in appendix B2. The semi structured interview went through all the questions that I asked in the individual interviews (listed on page 81), but in a much less structured way, because the participants have anticipated many questions by themselves and and because there were a lot of interruptions.

Indisputable reponses first

When I first asked the question that they have actually heard many times before, and which we discussed in the previous individual interviews, Stephan made a clear-cut and non problematic answer: “oh the 5% co2? u:hm (.) I think that is uh going to give the equilibrium (.) of the co2 ( ) levels that is comparable to what is happening (.) in vivo (.) it's also u:h causing u:h this balance equilibrium in the pH and (0.2)” (line 1853). Everyone seemed to agree to what Stephan said and no one took the initiative to expose the complexity of the issue, at least in the way we discussed it in the individual interviews. I asked everyone else if they agreed and they all said “yes”. I then started to unpack the complexity of the question, little by little. I started with the concern that I raised in section 3.3.2.1 (page 87), and asked whether the 5% level is meant to maintain the medium’s pH at the “desired level”, without mentioning 7.4 in particular. Again, all of them agreed to my statement by saying “yes”, “yeah” and “uhm”. When I insisted if they totally agree, Stephan added a new idea, by saying that not only the 5% is responsible for the pH level, but that there are other factors: “Well it's interplay you you also have like chemical agent in your liquid” (line 1879). At this level, I told them that in the previous interviews some said that the 5% level of co2 was to maintain the pH “at the desired level” depending on the experiment and some specified that the 5% is particularly useful for the 7.4 level of pH (see discussion on page 87). Brian responded to my concern in very ambiguous way (line. 1878). I had to reformulate my question by saying why some people said that the 5% co2 is suitable for the 7.4 pH and other people

said that the 5% is suitable for any desired level. Now Brian answered my question in a direct way by interrupting Stephan. Stephan was trying to express an idea (line 1887); he said "I think it's", but Brian raised his voice and said "to keep it at 7.4" (line 1888). Daniel, immediately agreed to Brian and said "this is a standard uh that uh culture condition". Obviously, as it was first suggested by Joanne in section 3.3.2.1 (page 87), this answer is problematic, because if the incubator which is regulated at the level of 5% co2 is suitable for the cells that should be kept at a pH of 7.4, then what about the cells that need other levels of pH and that are also stored in the same incubators (always with the 5% level)? Stephan understood my concern and said that for cells that require other pH, they should add more buffers[40]: "and otherwise I think because if you have like a medium that is different (.) different pH (.) you make your buffer in such a way that this is (.) in the same (.) and it's also stable (.) so it might be that in your liquid composition it's different but at the end because of this 5% it's still also the one you want" (line 1899).

I wanted Brian to confirm that he and the others think that the 5% was tested to be suitable specifically for the 7.4 pH and not for any other level. I asked again (line 1900) and he said "yes" but added for the first time "and bicarbonate uh buffer" (line 1906). Then, Daniel explained more and said: "Yeah two conditions which are associated the 5% co2 in the incubator and the concentration of bicarbonate in you medium (.)" (line 1909). Brian agreed and said "exactly". Daniel explained more by saying that "because the:: industry controls the percentage of co2 we know which concentration of bicarbonate we need to add in our medium (.) it's a way to change only one::: one thing in the medium (.) we know that the co2 is set up at 5 % and then if you want to change the pH in the medium you'll have to change to change only the:: bicarbonate" (line1912). Now that the claim that the 5% level was specifically made for the 7.4 pH is well established, I re-evoked the concern that I initially evoked in subsection 3.3.2.1 (page 87) and asked: "if you work on cells that are supposed to be at 6.5 of pH for example (.) what do you do?" (line 1916). Gaelle responded with a simplistic reply: "you use the buffer". Brian confirmed the answer, by saying: "we change the yeah we change the the composition uh composition of the medium". I had to ask if they keep the 5% level to cells and they said "yes".

[40] Buffer: A solution which resists changes in pH when acid or alkali is added to it (http://www.oxforddictionaries.com).

The spark

So far, none of the participants has showed any concern regarding the unsuitability of the 5% level for the 6.5 pH; so, I put the question very clearly: “how can you be sure that the 5% of co2 is also good for the 6.5 pH?” (line 1925). As soon as I put the question, they quickened the rhythm, raised the volume and interrupted one another repeatedly (lines 1920 through 1934). Brian laughed at my question, but admitted “the pH will be the same but the percentage of co2 you don't know if it's the good one but normally it should be” (line 198). Stephan almost admitted that the 5% is not known to be suitable for the 6.5 pH, but justified this practice by the fact that in order to have a suitable co2 level for different cells of different pH levels, you would need too many incubators, hence hinting to the idea that this is costly: “↑ [OTHERWISE you have you need 20 incubators if you have all these different cultures (0.2)”. Gaelle and Daniel did not respond to my question, but just described what they do in general. Now the question started to seem of great importance, although when I first asked it, the responses seemed to be brief and unproblematic. I wanted to ask them, if they themselves see my question to be of importance, Brian responded by saying: “Yes of course (.) the pH is important so (.) probably because of this addition (.) it's important”. Daniel added that he agreed that the pH is important, but nothing else: “for me (.) I agree that pH is important but I will not (0.3) [”. Daniel probably wanted to continue his sentence as he put it in the previous interview, where he mentioned that he was interested to know why 5% but not to change it (page 95). Stephan however was more skeptical and said that the question would allow them to know what is more suitable for their cells in terms of co2 level: “You need to know why this is actually the right condition for what I'm studying (.) it’s good to think about it so” (line 1959).

Now, I opened the main important part of the discussion, which is about whether or not the 5% would skew the experiments when the cells are requiring other levels and not 5%. I said: “I mean do you know for sure that co2 can change results? The level can change results?”. Stephan responded to my question with “↑Oh yeah” and Brian with “yes”. Now, we are at the heart of this case’s concern: why standardize a condition that is still not sure to be suitable for the different cells that require different co2 levels? When I put this question (line 1966), Brian responded that there are labs that are testing these levels: “I think there are labs doing tests”. I told to Brian that I did not mean

the labs that are now, after decades, starting to work specifically on this topic, and that my question is directed to all the researchers that have been and are still using the "standard" of the 5% of co2 level, even though they are not sure that it is suitable for their cells that require other pH levels. Obviously, the obligation to adhere to standards is not totally correct (as I will illustrate at the end of this paragraph); rather, the 5% is a common practice and researchers do have the possibility to use another level. I said: "Why standardizing a condition that you are saying it can change the results?". My question clearly triggered defensive accounts: Gaelle said that they cannot test everything (line 1984). Brian agreed and repeated the same idea. I was trying to tell them that I agree that a researcher cannot test everything (although I said this too many times, before), but he or she at least should not defend what they never tested. But Daniel, bounced off another idea, which he had mentioned in the individual interview. It is about the need to get the 5% fixed everywhere in the world in order to compare replicated experiments conducted in different labs: "it's also a way to be able to compare (.) our results with the results of another lab or (.) we need to: to work with uh […]= almost the same conditions" (line 1998). I responded to Daniel, by saying that as long as an experiment has a research protocol, it can be replicated with whatever co2 level it mentions and not necessarily the 5%; especially that they had confirmed that the 5% can skew their results. I was very curious to know their positions with regard to this trivialization (or the playing down) of the skew potential of using an unsuitable co2 level and with regard to the emphasis (playing up) that Daniel put on the need to replicate experiments. I said: "why? (.) people who need to replicate your research (.) they would look at your research protocol and see what what level you used and replicate it (.) not necessarily 5% (.) particularly if you think that the type of cells that you work on don't have 5% (.) so why necessarily use 5% when you think that these cells need 2% for example?" (line 2002). At this level, Daniel tried to answer: "you need to use [", but Brian interrupted him, by saying "then you use 2%" and all of a sudden all of them, including Daniel agreed that it is fine then to change the co2 level to 2%, while they had already said that they should not play with this standard.

That the participants said that they can change the co2 level to a suitable one for the cells does not solve the problem, simply because they never did so, in practice. I put the question directly and said: "but you don't change the

level of the co2". Brian admitted that they cannot do so: "no because we cannot change" (line 2013). I repeated the question to make it general for the entire lab; and Brian said again: "no I think never". Now, that the participants are faced with the contradiction of saying that the co2 level should be changed to be suitable to the cells under experiments, and the fact that it actually never changed in the lab, even though they work on different cells, Daniel will give another justification. It is now a matter of means and constraints: "yeah it's also because (.) ok it could be interesting to change u::h co2 concentration but it means that you will keep one incubator for your experiment because you'll change the co2 (.) and we're not only one to use the incubator (.) we share the incubators so (.) we need to keep it at 5% co2 so::: we cannot change like that". No matter whether Daniel's reason is true or false, he is no longer defending that the 5% level should be kept at all times, as he did at the beginning of this interview and at the previous interview too. He shifted the discussion away from the scientific terrain and oriented it to the fact that their lab lacks the proper means and resources (i.e. limited number of incubators). Daniel used the word "also" to switch the discussion from the scientific aspects of the issue to an unaffordability consideration. He also, switched to the use of "you", when he was describing such unaffordability. Immediately after Daniel, Gaelle returned the discussion to the idea that 5% of co2 was "for sure" suitable for the 7.4 pH, although our question is no longer about this suitability, but rather for its suitability for the different cells (especially those that do not require 7.4 pH). Additionally, Gaelle said she was sure of something that she even did not know when it was established (between fifty and one hundred years ago). Furthermore, our discussion is not about the effectiveness of the 5% to achieve the 7.4, but rather of the skew potential of the 5% in itself; Gaelle: "[but for sure there are study before may be like u::h fifty I don't know one hundred years ago that said that 5% co2 it was necessary to maintain the:: the pH at 7.4" (line 2028). I told her: "how do you know for sure?", she replied by basically saying that it should be like that, again using the "you" pronoun: "because you cannot say ok now we will put the incubator at 5% co2 and you give uh good results ". Clearly, Gaelle's response is not an answer to my question. She kept saying the same thing (lines 2028 through 2038), although our discussion was supposed to evolve into "how" things should work and what is better for research, instead of redescribing what she guessed to have happened fifty or one hundred years ago.

Existing knowledge

When I repeated that they are constantly building all their research on a condition that they do not know where or when it started (a paper, a journal etc.), particularly that they said that it can potentially skew their experiments, Stephan responded by saying: "= you're continuing on existent knowledge that has been validated" (line 2039). Stephan's response now revealed a new factor behind this trivialization. It is the power of the "existing knowledge", or the practical knowledge. From my standpoint, scientific researchers- who are supposed to think thoroughly and systematically more than any other professionals- should be skeptical about the practical knowledge that they are using. Stephan again said: "and if you want to continue and expand (.) do new findings you have to start with (.) some part of existing knowledge to continue from this point" (line 2041). One more time, Stephan used the "you" pronoun to make me involved in the need to use existing knowledge, when the idea is his own and clearly not mine.

Yes 5% can skew

Now, because Stephan was defending the "existing knowledge", which is in our case the standardization of the 5% level of co2, I wanted to go back and make sure that I clearly understood that this standardization is a problem for their research. For me, it is just inconsistent to defend something and to say that it is causing a problem at the same time; so I said: "are there other negative consequences with maintaining the level at 5%?" (line 2055). Brian responded by saying: "yeah sure (.) I'm sure of that". Daniel interrupted him, but I interrupted Daniel to better understand what Brian meant and I asked him: "sure of what?". Brian said: "that there are some genes that responding to this concentration of co2 for sure". At this level, Brian already accounted on a precise and serious problem with this practice of standardizing the 5% concentration. That genes respond because the co2 level is set at 5% in vitro (unlike in reality) will make things different when the experiment will move to in vivo. So what works in vitro will not work in vivo and vice versa.

Diversions

Daniel again interrupted Brian to say that there are other elements that can skew the experiments and not only the co2 and he mentioned the glucose. I told him that our topic is the co2 not the all elements that can skew: "→ Yeah but let's keep talking about this in particular" (line 2089). My request triggered a big laughter by everyone; I assume it was because they understood that Daniel wanted to take the discussion off topic. When this attempt failed, Gaelle took the discussion to another direction. She said that as long as the 5% level is used for both the "control" and the "test" samples, so there is not problem: "but you have the control condition the condition that your experiment (.) so:: the two conditions are exposed to the same level of co2" (line 2070). I was ready to answer Gaelle, because she was clearly wrong, because she was at least talking about the reliability not the validity of the experiment, but Brian responded to her by saying that the 5% level coud "mask" the experiment (for example the drug effect): "It could be masking may be if you do a less co2 percentage you will see difference but at the same co2 percentage (.) at 5% you don't see difference" (line 2080). Gaelle responded by saying: "yes but me I don't want to see the effect of co2 (.) I just want to see the effect". Clearly Gaelle did not get the point. She basically said that as long as her experiment is not about the co2 level effect, so it does not matter if it is at 5% even though it is not the appropriate one for her cells. At this level Daniel, Brian and Stephan corrected her to confirm that even her research is not on the co2, it would have an effect on her research, because it is not the appropriate one. Brian told her: "it has an effect", and Stephan agreed: "exactly" (lines 2081 through 2087).

From an awkward trivialization to detrivialization

After the last diversion initiated by Gaelle, and after admitting that it is not appropriate to generalize the usage of the 5% level of co2, Stephan will now try to trivialize the skew, but his use of the words "risk" and "gamble" are quite surprising: "it's it's it's (.) you have to make a calculated risk more or less that you make (.) it's like a risk (.) you take a gamble" (line 2089). I wanted to dig down more and bring the discussion back to this risk, so I asked them again to tell me what kind of negative consequences this situation can have. Brian responded with almost the same idea, by restating that if the level of co2 is inappropriate, it can make a difference in the research results: "depending

on the level of co2 you will see difference or not (.) between the two conditions". Then, I addressed the same question to Daniel, who admitted this skew potential, by saying that this condition would change the phenotype[41] of the cell: "so if we increase the co2 concentration in their medium at least they'll start to produce co2 (.) so you change a bit the phenotype of your of your cells" (line 2107). Then, I reminded Daniel of what he had told me in the previous interview (that this level can cause them to miss the drug effects, page 95). He admitted this, but added that not only the co2 can cause these skews, but there are other biases in their experiments. Then, I closed this part of the discussion by asking: "so it's a condition that can change the the how cells behave?". Now, all of them confirmed: "yeah", "yeah", "yeah", and "of course". (lines. 2116 to 2119). Now, I am left without any doubt that the generalization of the usage of the 5% level of co2 is a considerable risk of skew for the cells that do not require this level.

After the multiple diversions, I will now focus more on understanding why this risk is trivialized. I put my question in a different way this time, by asking why this situation is so, but I foregrounded the idea that I am not talking about them in particular, but about the whole research practice (line 2120). Stephan responded by being critical to the idea of replicating research and standardizing a condition that should not be standardized: "because there are for sure there are groups they're like we have to culture we must just replicate standards and look and may be they're not critically thinking over if it's the good condition for them (.) so yeah well then you're already biased from the start to your research is already aiming in a certain way maybe that could have been different indeed it would have an effect of co2 or not" (Line 2121). It looks like putting my question in the general form is more comfortable for at least Stephan, who grew more critical this time, but again using the strategy of "you" to describe the negative practices. Additionally, Stephan preferred to call those who over generalize the usage of the 5% and who are interested in the identical replication of experiments "groups", although all his lab members (including himself) are among them, simply because they never change the 5% level (although they work on different cells). Now, in the same minute, I put my question in a different way, by suggesting that they themselves are doing this mistake: "people

[41] **Phenotype**: The set of observable characteristics of an individual resulting from the interaction of its genotype with the environment (Oxford dictionary).

keep working on so many things but they are standing on this standard (.) like ok this is a standard we don't question it unless this is my topic and you're saying that it can skew the totality of research" (line 2141). This time, the participants will get back to the trivializing accounts. Stephan, will only consider this situation a limitation: "yeah (.) I think you're (.) in your study if you have to work from a starting point then you based on some model and this just part of this model (.) part of the conditions (.) and if you look at like the key identifiers that you know or at least expect to be changing or not (.) as long as you can see this effect (.) it can be enough and only if you suspect (.) may be there is a role of co2 or something (.) that you would take it into the:: consideration but otherwise you just focus on this part and you have to accept limitation [" (line 2153). Clearly, the discussion will now switch again into the trivializing positions.

Back to square one (case départ)

Brian, will now restate again the idea that they need to replicate the experiments, although I had answered Daniel a while ago that we can replicate any experiment by following its research protocol with whatever level of co2 it mentions; Brian: "[and you need to standardize because when you go to a lab in another place the co2 concentration will be different then you have the right buffer and then the right pH (.) it's also a way to give constant" (line 2156) . They all agreed with Brian again that this standardization is important to replicate experiments. This time, I expressed my astonishment by saying that "the only thing that I can't understand is how we standardize something that is already problematic (.) still just ok we want (.) because you want to replicate so you need to keep this in order to be sure that we're duplicating the same thing (.) but at the same time you're saying it's problematic it can change results (.) this is [" (line 2171). But this time too, I was interrupted by Stephan who will now play down this skew risk, by using 2 reasons: a) that the parameters are too many and they cannot change them all, and b) that they still can address their research even though they would miss other things: "I think you can still address the question that you have (.) even just accepting that you have some parts that you don't know but at least the question (.) you:: you can do some your findings or new data or research done (.) so may be to address the same question that you have it's sufficient to find something (.) may be you'll find additional stuff (.) if you would change this parameter but you

cannot change everything (.) otherwise it's like millions of parameters that you have to change then then you're never (.) you have to accept some lack of" (line 2178). Now, Stephan's account is not only irrelevant to my question, but also in clear contradiction with his previous accounts. He trivialized the skew risk of the co2 level because there are so many other parameters to think about, whereas the co2 and the temperature are the only two parameters that are systematically repeated in every single experiment, without exception. So clearly, the co2 is not one among "millions" of parameters. He also trivialized this risk, because "you can still address the same question" as he said. What he called a minute before to be "already biased from the start" is now no longer a big problem: "it's sufficient to find something (.) may be you'll find additional stuff (.) if you would change this parameter" (line 2176). More importantly, Stephan's response is irrelevant to my question, in fact. My question again, was about why they need to replicate experiments with 5% and not with the suitable level, which they anyway indicate in the research protocol.

After a long discussion about the usefulness of standardization, the participants- who on the one hand said that this level may blind their own research- never responded to my comment: if any lab wants to replicate any other lab's research, it can just follow the research protocol. They never answered me with regard to this concern (which I made using their own words); and every time I mentioned it, they switched the discussion to another argument. Additionally, the co2 level is not one like the unlimited number of cell culture parameters, because there are only 2 parameters that are systematically repeated in every single experiment (unless it is the topic of the research): the co2 and the temperature. The other parameters can vary from a research to another, even though the majority of them are most of the time unchanged (the medium components, the Trypsin [42], the plastic of the dish etc.).

From science to common sense

I will now tell them that Paul has told me that this 5% level actually "was dictated by the medium that was available commercially" (which I detailed in the previous subsection, page 92). Obviously, this idea suggests that this standard did not come from research, but from business and is therefore profit-driven: "but what I meant is that this

[42] **Trypsin**: A digestive enzyme which breaks down proteins in the small intestine, secreted by the pancreas as trypsinogen.

this level came from the commerce from the providers not from research". When I said that, they all spoke at once and almost shouted, to the extent that the audio recording became inaudible and untranscribable (line 2243). Then Stephan mentioned that he did not agree by saying that if research does not buy the medium, the providers would not be able to make profit: "they can produce but nobody buys" (line 2248). Gaelle added that even if providers are from commerce they use researchers. I told her that their ultimate aim is business, anyway. Stephan took the floor to say that it is actually the researchers that asked for a medium of 7.4 pH, so the need came from research not from business: "because there is a request for something that is keeping things at 7.4 (.) so they make it (.) and they sell it (.) so it's like (.) it's way". Yet, Stephan forgot that we are not talking about the 7.4 pH but about the 5% of co2. I told him that if the research requested a medium of 7.4 pH, the business was able to satisfy this need, but "dictated" the usage of the 5% of co2 with it, as suggested by Paul. Brian interfered and disagreed that this 5% came from business, altogether. He said "it's not like the company said no no you have to use 5% (.) the scientists found that 5% co2 with this bicarbonate composition leads to pH 7.4 (.) so the company created this medium to supply" (line 2271). Basically, this account is contradictory with Paul's, although when I reported to them what Paul said (line 2212) they agreed with the idea. Brian: "yeah we agree" (line 2216). Stephan also said: "yeah we agree with that" (line 2224). But now Brian is getting back to the idea that the 5% did not come from business but from research, without mentioning any basis to back up his idea. At this level, I said I will put my question in a different way and said that the company that first produced the medium dictated the usage 5% of co2 as suggested by Paul and that this 5% was not what research asked for. I then added that if this company discovers during the manufacturing of the medium that the 5% is not good for their cells, would they mention it? Or would they ignore this condition, because they want to sell the medium that was asked for? Now, Daniel will destroy everything by saying: "but why they would 5% is not good" (line 2280). I was surprised to hear Daniel ask me why, although he and his colleagues have already described the negative consequences of generalizing the usage of 5% level of co2, when cells require other levels. I told him "like YOU said" (line 2281), not me. Daniel did not reply, but Stephan took the floor to divert the discussion again into the same point that he had already mentioned before: practicality and cost: "so because also practical (.) I think when they choose ok let's use co2 (.) something (.) relatively comparable to environmental and 5% it's (.) something

to do (.) I think it's it's because also like really what research wanted practicality and it was historically somebody arrived at this and then people say HEY what your (.) nice it's easy let (.) I'll also do it I'll also do it and you get a market and then produce and then everybody uses it" (line 2291).

Our discussion ended there, after I asked everyone if they wanted to add anything, but they did not. The discussion was like a hide-and-seek type of game. The contradictions that were made in the previous individual interviews were repeated here, as if they never discussed this issue with me before. I deduce from the number of the inconsistencies I discussed here that the participants developed a defensive position regarding this issue. Nevertheless, I was able to detail how the standardization of the 5% level of co2 is- according to the participants' description of the negative consequences of using unsuitable co2 levels- potentially skewing their research. I also illustrated that such a potential skew is trivialized both in practice and in the participants' accounts, during the interviews. While the trivialization in practice can only be explained by the power of the structure or "the existing knowledge", the trivializing accounts made during the interviews were mostly accompanied by: a) the switch to the "you" pronoun, b) the use of "people" to refer to practices that they themselves do, and c) the use of "also" to lessen one's contradiction made by citing two incompatible ideas.

3.3.3 Exploring the (de)trivialization dynamic (case 1)

Based on the 3 previous subsections, in this section, I will zoom-in on the (de)trivialization dynamic related to the case of the co2 level in the incubator. Following are the questions, which I will address in the subsequent paragraphs: (a) what is (de)trivialized regarding the issue of the co2 in the incubator? (b) Who is (de)trivializing it? (c) Why is this (de)trivialization occuring? (d) How serious are the consequences of trivialization? When and where does it occur? How is it practiced or accounted for or practiced? As a matter of fact, the (a) through (d) questions happen to be inextricably related and can hardly be dissociated, but I will discuss each concern in a separate subsection, for the convenience of dismantling the dynamic into distinguishable elements. These elements are summarized in table 1 (page 129).

(a) ***What is (de)trivialized regarding the usage of the 5% level of co2 in the incubator?***

With regard to the most basic question: "*what is trivialized regarding the co2 level in the incubator?",* the shortest answer is: its skew effects on the research results. I will detail 2 opposite elements: the first element details that the 5% co2 level has been taken for granted and that its skew effects have never been discussed prior to our current research; the second element details that the 5% co2 level does not mimic the co2 level in the human body and that this condition can have negative implications on cancer research.

The lab members have directly answered my second question "do you think that this issue has been trivialized?" with varying degrees of "yes". Stephan said: "I think. I don't know if it's trivialized or neglected (.) it's just something that's ignored (.) it's that people take things for granted" (line 348). Brian said "may be now yes (.) because it's a it's a long time it was set up and we don't question these things (.) but I think in the past they were discussing a lot (.) a lot" (line 363). Daniel said "oui (.) oui (.) oui parce que d'une part en fin d'une part je me suis posé plein de questions pourquoi 5%? et je pense qu'il ya des gens qui viennent au laboratoire et je pense qu'il n'y en a pas beaucoup qui savent répondre (.)" (line 382). Dilara said almost the same thing : "u:::hm je pense qu'elle est importante parce que c'est une façon de mettre le doigt sur le fait qu'on maitrise pas tous les paramètres de la

recherche (.) c'est quelque chose de trivial (.) tout le monde travaille à 5% et tout le monde fait ça (.) en fin dans ce cas ci (.) mais finalement j'ai l'impression que peut être peu de gens savent pourquoi on travaille à 5% de co2" (line 393). Gaelle said: ‘‘Yes we could uhm (.) ahhh "approfondir" (deepen [our understanding])’’ and then ‘‘because we can't say ok (.) to regulate the pH (.) but how it works?’’ (line 414). Cédric said: "yeah may be I don't know (.) for my part yeah (.) I never asked myself that question about the 5%" (line 417). Paul said: "you probably lose part of the part of the of the of the information (.) so it is clearly uh uh underappreciated" (line 434). Dalila said: "oui je pense qu'on ne s'arrête pas dessus " (yes, I think we don't stop at this question). Marie basically said that incubators are sold to them like that, so that is not a matter that they think about in everyday life ‘‘uhm (.) je pense pas que ça était négligé mais on se la pose pas tous les jours (.) quoi (.) puisque c'est comme ça (.) l'incubateur on nous (.) enfin (.) on nous le vent comme ça’’ (line 457). Joanne said: "I don't know (.) I've never asked them that or I've never hear about this from them (.) so I don't know" (line 474).

Now the lab members will account for the importance of the issue: Stephan said: "Yeah I guess so (.) it's an important parameter (.) everything that you're doing is (.) All your culture cells that you put in the incubator are affected by the parameters that are set (.) and may be your cells are (.) requiring something different" (line 595). Brian said: "could be (.) it depends on the topic you want to work on (.) if you are interested to check the acidity or the medium or some some process that is regulated by the co2 levels in the body yeah could be an interesting thing (.) yeah sure" (line 601). Daniel first said that it is important to know why 5%, but just to know and not to try another level for cell culture: "oui c'est en fin (.) je pense que c'est quand même important de savoir pourquoi on place à 5% (.) uhm donc la discussion après uhhhm de toutes façons (.) là ne changera pas notre façon de faire la culture" (line 614). Daniel later changed his position when he answerd question 9 and said that the 5% condition may actually cause them to overlook the effectiveness of the drugs they are testing: "de toutes façons on peut dire plus l'un dote d'un taux de changement dans l'environnement ne serait ce que le co2 (.) le pH aussi (.) ça c'est mon sujet de post doc (.) le pH aussi a un impact sur l'effet de certaines drogues qui sont pas forcément acides à pH 7.4 dont les pH acides sont utiles (.) donc c'est clair que dans les conditions normales de cultures on ne voit pas l'effet (.) qu'on va dire la

drogue n'a pas l'effet (.) et on met un pH acide (.) a un effet (.) c'est en changeant un peu les conditions de cultures qu'on va essayer de trouver (.) mais (.) clairement (.) on passe surement à coté de certaines choses dues au fait qu'on a des conditions de cultures standards" (line 877). Dilara said: "u::h je pense que dans la vie de tous les jours (.) c'est pas important parce que comme tu l'as dit finalement la majorité des gens font comme ça (.) donc on se pose pas question (.) mais par contre je crois que c'est important de se poser une fois la question" (line 643). Gaelle said : "Yes it's important because otherwise the cells can (.) may be not (.) are not in the right condition so it can have different consequences on our research" (line 658). Like Daniel, Cédric made it clear that it is important to know why but not to try to change it or anything of the sort: "of knowing why yes it's important to know what (.) why you're doing this stuff (.) uh (.) Yeah" (line 671) then he said "I said just that but it's you know it's (.) I guess it's established for really long time you know (.) it should have been done and redone (.) a lot of time (.) by now (.) so I guess it's ok" (line 690). Paul said: "It worths to reconsider the the (.) the the the pH which sets our culture (.) clearly (.) but we know that it's not by changing the co2 concentration that we'll have the (.) it will reach the (.) for instance the cancer cells the expected pH mimicking the in vivo condition (.) so yes it's important to consider the pH (.) uh but not from the co2 (.) through other means of regulating pH" (line 698). Marie said that all the parameters are important and if we modify this parameter we will end up with different results: "oui ben oui oui ben tous les paramètres sont importants si on modifie ce paramètre là (.) on aura pas peut être les mêmes euh les mêmes résultats". Joanne said: "Yes (.) I think it's important (.) because we have to consider uhm how the cells and in which the cells (.) in which conditions they are living in reality like in the body and to try to incubate them in the right condition" (line 767).

The participants' overall accounts reveal that the issue of the co2 level is important on the one hand and that it has been trivialized on the other hand. Even in the case of those who said that it is important to know why 5%, but not to revise it or try to test other levels, they are paradoxically confirming that it is actually important to rethink this issue. This is the case of Cédric who never changed his position and Daniel who changed his position later in the discussion. Cédric (quoted in the previous paragraph) who said in the same utterance that it is important to know *why*, also said that it is not important to consider rethinking it. If he does not know why this level was established at

5%, how can he decide that it is not important to rethink it? Similarly, Paul who had answered question 1 (line 104), saying that the 5% level is important to maintain the pH level around 7.4, also said here that it is not important to rethink the 5% level; he said what is important how to monitor the pH level through other ways. What is clearly contradictory in Paul's account is that he stated: (1) that 5% is important for the pH level they need and that there is no need to rethink or revise the 5%, which means that he is satisfied with this condition; but then he stated (2) that it is important to find other ways to fix the pH level, which means that he is not satisfied with this condition (i.e. that the 5% of co2, which is there to maintain the right pH level, is not functionning its work properly). In sum, no matter what any lab member position is, it is undoubtedly true that this issue of 5% is an important issue, with varying degrees and justifications. Likewise, no matter what the lab member position is, it is quite clear that the issue has long been trivialized.

(b) ***Who is (de)trivializing the usage of the 5% level of co2?***

In this subsection, I will address the "actants" of trivialization (actants in Latour 1987's sense); i.e. *who is (de)trivializing this issue of 5% of co2 level?* I will detail the idea that actants of (de)trivialization are multiple, interrelated, human and non human, and that they are both inter and intra organizational. Furthermore, the actants will be identified in relation with the following 3 distinguished stages: stage (1) the trivialization as a practice that is independent from and prior to our current research; stage (2) the trivialization followed by the detrivialization and the reverse direction, during the individual interviews; and stage (3) the recurrent oscillation between trivialization and detrivialization during the collective and diachronic interview. Figure 3, on page 304, depicts the interaction between the long established trivialization and the detrivialization initiated by our research along the 3 stages (observation, synchronic interviews, and diachronic interview). The actants are classified into 3 main levels (organizational, individual, and group). The figure aims at portraying the trivialization actants that compromise knowledge construction by interweaving scientifically based ideas with ideas that are not necessarily scientific, but rather a common sense type of ideas. This distinction has been made by Latour & Woolgar (1979) who provided a number of examples, which "point to scientific reasoning as not being dissimilar to common sense reasoning". The authors

concluded that in their laboratory too "whether evidence is accepted as such, depends on factors that are not necessarily scientific" (p. 178). In the current study, it is through the participants' accounts analysis that I am identifying the actants of the (de)trivialization over the co2 issue. More particularly, it is through the distinction between the scientific and non scientific discourse that these actants emerge, or become apparent:

Level one actants of trivialization (see figure 3, page 304) is more a condition than a particular person or group of persons. It is the condition or the practice of taking things for granted. At this level, it is very hard to distinguish between the practice of trivialization and that of taking things for granted. Even though in research, scientists are expected to scrutinize the different elements of their research, they may inevitably or intentionally trivialize other elements. The lab members happen to trivialize the need to think about this matter, because they take it for granted, as we detailed in subsection (a) above. And they take it for granted, because they assume it to be unproblematic or may be not worthy of investigation. The cause-effect relationship between *trivialization* and *taking-for-granted* is not totally clear to us, although the lab members have explicitly accounted for it (subsection (a) above), but I can at least confirm the correlation. In this sense, there is "a yet to identify condition" that is underlying the trivialization. Perhaps, this condition may correspond to the duality of "structure-agency" (Giddens, 1984), when the participants refer to it as a "standard". When it is a standard, this condition structures the research practices in the labs, hence it urges them to know something (that they need 5%) and unknow other things (the skew potential); but the standard can also be an agent personified by the participants when they defend this choice (even without being able to explain it). Hence, at the level of this lab, the trivialization over this issue is mostly an uncontrolled condition than a deliberate practice, because it is visibly inadvertent. Perhaps a systematic analysis of power, would unveil a specific "microphysical power" that is behind this practice (Foucault, 1969).

From another perspective, trivialization can also be considered as the participants' own practice, no matter how inadvertent it may seem. It is their own practice, because they have taken other options and thought about other parameters in any of their past research experiments, without considering the parameter of the 5% level of co2.

Trivialization, in this sense, consists in the practice of avoiding; no matter how unintentionally it has been done, or how passive it may seem. Thus, all the lab members without exception have been to some degree active in the trivialization around the 5%. What I call here unintentional group practice comes along with what is known to be "groupthink"; a phenomenon introduced by Whyte 1952, Janis 1982, and many more to illustrate the idea that groups tend to converge and do things in the same way, for the sake of avoiding disagreements or misunderstandings. However, it is beyond the scope of this research to determine, if this convergence or groupthink situation is a choice motivated by the avoidance of disagreements. In terms of power exerted on the lab members, and which is behind their unintentional trivialization, we can comfortably cite the power of "standards". Standards have been cited by the lab members to be dictating the level of 5%.

Now, I wish to distinguish between the trivialization that preceded our research and the trivialization that persisted during the conduction of this research. More precisely, we wish to draw a line between the lab members that only trivialized "by default" or prior to this research and those that continued to trivialize the issue of the 5% level of co2, explicitly during the interviews. As a matter of fact, all the lab members have trivialized this issue prior to our research, because none of them has reportedly tried to consider the consequences of the 5% co2 level. Then, except Cédric, Paul and Brian, all other lab members have stopped trivializing this issue at least verbally at the threshold of the interview.

To start with the trivialization that preceded the interviews, I understand that a researcher cannot think about all possible variables and parameters, simply because they are too numerous; and in experimental sciences a researcher reportedly needs to make a choice and take options, supposedly and ideally based on a literature review and using past research findings. From this standpoint, trivialization is understandable and clearly explicable. However, because cell culture (which involves the 5% co2 level, the 37° Celsius and the usage of the medium) is a process that is undertaken in every single cancer research in the world no matter what the aim of this or that piece of research is, I here assume that a special attention should have been given to these parameters over the parameters that

are proper to a particular piece of research. In other words, if the 5% level of co2 turns out to be negatively impacting the research, then it would be skewing all cancer research in the world. From our stand point, the impact of the co2 level on research is in principle (not necessarily in reality) significantly bigger than any other parameter, because it affects all cancer research in the world. Bottom line, I understand that a researcher can be overwhelmed by the unlimited parameters and factors that he or she has to take into account and that this situation would make him or her prioritize parameters and trivialize the importance of others. But, I do not agree with the lab members who once they realized the importance of the co2 level kept considering it as any other parameter that a researcher might or not want to work on. That ends the level one actants of trivialization portrayed in figure 3, page 304, i.e. the barricade of the actants that I call a combination of a "general condition" and an "unintentional group practice".

With regard to the trivialization that survived even during the individual interviews (level 2, in figure 3, page 304); Cédric, Paul and Brian were the 3 actants that continued to practice it, with varying degrees and up to different moments in the interview. All other lab members have at the beginning of the interview or half way through it expressed that they realized the importance of rethinking the 5% level of co2. As outlined in the previous section, Cédric was clearly the one who said that this issue has no significance and that they do not need to waste time about it. Cédric was consistent all along the interview. He never changed his position, in this regard. Even, when I raised the concern about the fact that human bodies do not have 5%, he still said that that condition has no importance. He recognized the importance to know why, but he said beforehand that it would be just to know and not to try to be critical about it. Again, even when I asked about the consequences of this level regardless of the issue of the pH level, Cédric persisted that the issue is not worthy of investigation. It is perhaps important to add here that Cédric was interviewed on the third day after he joined the lab. All other lab members have at least heard me say a word about my research, but not Cédric. I do not wish to establish a cause-effect relationship between the fact that Cédric was the only one who was unaware about my research scope and the fact that he was the only one who continued to trivialize the issue up until the end of my interviews. Yet, there is probably some degree of correlation between the two facts.

After Cédric, comes Paul with a lesser degree of trivialization. Paul also trivialized the issue of the 5% level of co2 even during the interview. Although he said that he does not know where this practice comes from, Paul said that the 5% level is not problematic and that we should not be worried about its impact. During the interview, Paul realized that the decision to make the co2 level at 5% was dictated by the medium that is "available commercially". In other words, the 5% level was not a research finding, but a commercial decision. As we have detailed in the "synchronic inconstencies" subsection in the present section, Paul who first considered that the 5% has to be kept, because if we try to change it, we then risk to damage the cells; when I told him that Dilara in fact did all her doctoral research with 10%, he said that that was an old habit. Dilara's research was actually concluded in 2014 (one year before our interview). Dilara, did not even know that cells are everywhere else cultivated at 5%. She did it because she did not know it was impossible, in Mark Twain's ironic terms. Moreover, Paul considered the negative impact of the 5% level to be minimal, contrary to all the other researchers, except Cédric.

Finally, Brian was the third person (after Cédric and Paul) who continued to trivialize the issue of the 5% level of co2 up until the interview, unlike the rest of the lab members, who realized the importance of the issue prior to the interview. As we have mentioned in the previous subsection, Brian was trivializing with significantly lesser degree compared with Cédric and Paul. Additionally, Brian made it clear that the issue is not of significance "before", but not now. He mentioned that because the decision of doing culture cells at 5% co2 has been taken for long time, it is now important to rethink it: "may be now yes, because it's a it's a long time it was set up and we don't question these things (.) but I think in the past they were discussing a lot a lot" (line 363).

With regard to the (de)trivialization that was practiced diachronically (level 3 of figure 3, on page 304), I have provided an in-depth discussion, in section 3.3.2.3 where I have detailed the recurrent oscillation between trivializing and detrivializing accounts, depending on my questions. The participants took clearly defensive positions, which resulted in a number of paradoxes. At the beginning of the interview, the agents (the participants) defended the structure (i.e. the standard) and nearly became one with it. Then, each time I pointed to a contradiction, they altered

their position momentarily by detrivializing the issue; and then trivialized it again. They repeatedly mentioned justifications of the trivialization: 1) the need to replicate the same experiments, 2) the need of standards, 3) the possibility to catch up at the in vivo level, and 4) affordability, which are all structural in nature. I repeatedly responded to these justifications, but they systematically shifted the topic to one or the other justification, without responding to my own responses. Additionally, I detailed a number of discursive strategies used to: 1) sometimes defend the usage of the 5% level (i.e. to trivialize its skew risk), and 2) sometimes minimize the flagrant contradictions and then be able to switch to detrivialization. Bottom line, the participants embodied the structure and defended it. They mobilized their own agency against their own freedom of knowing outside standards, rather than the opposite.

(c) **Why is this (de)trivialization occurring?**

While I have already detailed the organizational factor underlying this trivialization (i.e. a standard that is taken for granted) along with the discussion about the actants in the previous subsection, I will now address the trivialization as an individual practice, in an attempt to understand why the participants trivialize what they otherwise consider to be important. Yet, “why” questions are quite awkward when they entail a cause-effect relationship between an observed practice (discourse) and an intangible interpretation (possible motives, conscious or not). I not only think that peoples’ intentions are changing and hardly graspable, but that being oriented directly into them can be misleading, because my ultimate aim is not to understand intentions as such, but rather how they were accounted for and what roles these accounts play, in context. In the analysis of the accounts made by the 3 participants that continued to trivialize during the synchronic interviews (Paul, Brian and Cédric) I was not oriented into what they “intended” to achieve, but at exposing how their discourse functions in context.

Paul happened to be the only one who started by detrivializing the issue detailing the skew potential of the 5% level of co2 and using scientific words and explanations, but at the end of the interview he dramatically shifted into rather trivializing accounts, without any explanation (see discussion in section 3.3.2.2, page 94). His accounts shifted

from “you probably lose part of the part of the of the of the information (.) so it is clearly uh uh underappreciated” in question 4 to “I think that 5% co2 is something which is part of the (.) part of a model not being perfect uh I may assume that uh that the the the co2 (.) could (.) uh the co2 concentration could (.) influence the (.) could nuance the the the (.) uh the way that the tumor cells behave (.) uh if you compare them with with what it would be in the (.) in in vivo (.) in an in vivo context (.) so for me it’s not contributing to a big shift in the phenot[i]pe in the phenotype (.) but the influence is that can we consider there is a minimum uh to be acceptable” in question 9 (line 1680). Paul first said that he does not know how the level of 5% of co2 was decided to become a standard in their field, then that this condition could make them lose part of the information and that it is underappreciated; then that it could influence results; then reduced the term “influence” to “nuance”, to finally and suddenly say that the “influence” is “minimal”.

Following are Paul’s 5 major accounts regarding the usage of the 5% level of co2. He said: 1) that in the human body cells are not at 5% level of co2 and that levels greatly vary from organ to organ and depending on the type of cells and other factors; 2) that he did not know where the practice of cultivating cells at 5% of co2 started and he did not know where I could find a reference; 3) that he did not know what the negative consequences of cultivating cells with a co2 level that is different from the levels that different cells in the human body usually have; 3) that 5% is a well established standard and has to be kept, because it is important to maintain the pH level (although Dilara has conducted a research with 10% a couple of years ago, which was a surprise that all lab members who only discovered it through my interview); 4) that the 5% is a condition dictated by the medium that is available commercially; and finally 5) that he has never asked himself or was asked by students about the level. Based on the 5 elements, I conclude that Paul failed to provide any scientific answer regarding why 5%, and that his ungrounded trivialization at the end of the interview (by saying “it’s minimal”) functions as a barrier to whoever wants to problematize this issue. Additionally, Paul was the only one who was able to account for where this 5% comes from: business: “it was dictated by the medium available commercially”. Consequently, regardless of Paul’s intentions, his accounts were clearly contradictory and manifestly ungrounded. Based on the sudden shift of positions and particularly on the synchronic analysis of Paul’s accounts (see page 94), I deduce that Paul was surprised by the idea that he was

formulating by himself and found it hard to accept that all his past research can possibly be built on a potential skew. He also might have not found it comfortable to see that someone else from another field (me) can incidentally discover something as consequential as this. Bottom line, he might have found it too hard to acknowledge the significance of this matter.

Brian's accounts altogether help him argue that their choices are grounded and that their logic is justified. Brian said: 1) that the co2 is at 5% for different reasons and he mentioned that it was a standard, that it was an average level, that it was a compromise and that it was appropriate to maintain the pH level in the medium at 7.4. He also said 2) that he had heard this question before but not exactly in the same way I formulated it (rather how co2 is related to the pH level). Brian added 3) that this question is trivialized now, but not in the past, because in the past "they discussed it a lot a lot" without mentioning any reference or who they were those who discussed it a lot. 4) He also said that this level is a standard and that we cannot think that all others are wrong "you think that not all the people is wrong and they are just in the wrong (.) co2 concentration" (line 368). Additionally, he said 5) that the level of 5% co2 should not be a problem and would not blind the research; contrary to Daniel's account who said that if we test drugs on cells with co2 level that is different from what we have in the human body, then we would be blinded, and we can miss the results (page 89, third paragraph). Finally, Brian said 6) that he did not change his position with regard to the importance or triviality of the 5% level and that he did not remember his previous discussions with me about it, and if I think that his accounts changed, that is because in the interview I was asking questions in a different order compared to our past discussions.

Right from the synchronic interviews, I was able to detail that Brian's account number 5 was in contradiction with Daniel's position (see page 89). Furthermore, through the diachronic interview, I was able to detail how Brian's accounts were incomplete and sometimes wrong, but the way he mentioned them enabled him to seem to have logical reasoning. In fact, I was surprised by Brian's answers, because during our discussions that preceded the interview, he acknowledged the importance of the issue in almost the same way as Daniel's. The difference is that Daniel tried to

legitimate the trivialization, by saying that they needed to standardize research and opt for a co2 level that all labs share, while Brian did not legitimize the practice. Furthermore, Brian has on many occasions asked me about how I would analyze his positions and discourse. He said that his spouse is also in the social sciences field and that he keeps telling her that her domain is very "subjective". After the interviews, Brian repeated many times that he would be curious to see how I would interpret his talk. In short, Brian left me with a feeling that he was very skeptical about the methodology I would use to analyze his accounts. Consequently, I am more prone to conclude that Brian's discourse practices were oriented into fostering that their logic is rather strong and beyond reproach and that they are sure of what they are doing. In the diachronic analysis, I detailed a number of defensive accounts along with inconsistencies, which made me conclude that Brian was less focused on the topic than on managing me and guessing what I would think about him.

With regard to Cédric (see discussion on page 116), his initial accounts were clearly oriented into trivializing the issue rather than detrivialize it, but he was not able to justify his position. I concluded that Cédric was rather not prepared to answer my questions, which explain the discrepancy between his strong position (that we should not change the level) and the weak justification (that he does not know why exactly it is 5% and that he ignores their skew potential). Additionally, his accounts suggest that he is rather interested in applying the basics of biology research and adhere to rules rather than problematize them. Being a brand new PhD student (3rd day since he joined the lab), his accounts play no apparent role other than to show that he is there to learn not to discuss the rules. He said that we should not question the co2 level, because it is part of the cell culture basics and of the standards. Cédric's accounts were constantly explaining the matter from his position or at his level, although I tried to tell him on many occasions that I did not mean him in person, but I meant to understand how things should work for cancer research in general. It is perhaps understandable that a PhD student may not have the time or maybe the courage to rethink what is otherwise acceptable everywhere in their field. Therefore, Cédric was rather focused to answer me, from his own perspective. From his perspective, and to be safe, a PhD student should apply the "basics", be focused on his topic and do not dare problematize what is not problematic in their field. All Cédric's accounts seem to be feeding this

position; i.e. that a good PhD student should be able to master the basics and do research according to the orthodoxy of the domain: “because, for one thing, you know it has already been optimized, so you know that's the best condition for sure and you're not going to change anything about that, but yes, it's good to know why it's like that I guess” (line 422).

(d) **How serious is this (de)trivialization?**

We will now aim at understanding the magnitude of the fact that the 5% level of co2 has been trivialized or never questioned. It is beyond the scope of our research to argue that questioning it is or is not serious for cancer research as such or that it has implications from that perspective. That the issue is important or not from a cancer research point of view was already discussed according to the lab members and using their own accounts in a verbatim way (particularly, subsection (a) above). Our aim at this level is rather to understand the magnitude of the issue, in terms of “when”, “where” and “how” it occurs. The shortest answer to these 3 questions is, respectively: all the time, in every cancer research lab and unnoticeably.

Addressing the “when” part of this subsection, all lab members have said that the 5% level is like a standard that is used all the time, unless a researcher is working on the parameter of the pH level in particular and wants to test it. Historically speaking, Paul mentioned that the practice is in the field for about 50 years: “I think this is known for (.) uh (.) probably more than uh (.) 50 years ago” (line 122). Based on the lab members’ answers, I can confidently state that this practice is used almost all the time. Stephan said “well it's like standards so you have to put it there”. Brian said “these like a standard that is well established everywhere and in every lab you have always this set up” (line 365). Daniel said “c'est vraiment des conditions standards” (line 385) (it’s really standard conditions). Dilara said that she assumes that the 5% is a way to standardize research “une façon comme (.) je suppose de standardiser la recherche” (line.75).

In terms of “where” the 5% is used, the lab members said that it should be in all cancer research labs in the world. Question 4 of the interview was “do other biology labs in the world also regulate their incubators at 5% co2?” (line.489). Stephan answered the question saying: “U::h yeah yeah all the labs where I worked were programmed at the same level, the same concentrations” (line 495). Brian said: “yeah yeah in Germany, in Spain, in all the labs like this they make the same co2 concentration” (line 500). Daniel said “oui tous” (line 506) (yes all of them). He added “oui parce que de toutes façons les firmes qui vendent les incubateurs (.) voilà de co2 (.) on peut le changer (.) mais il est placé à 5% par défaut” (because companies that sell these incubators regulate them at 5% by default, even though we can change the level). Dilara said that she knows that it is used in the USA, in Belgium and in Paris “Il ya le (.) aux états unis (.) ils étaient à 5%, ici c'est à 5% (.) et u:hm (.) où est ce que j'étais encore? La culture cellulaire? à Paris” (line 524). Gaelle said “yeah (.) I hope”. Cédric said “I guess everyone do that, except if you want to raise an issue and you are going to change this” (line 547). Finally, Paul, Marie, Dalila and Joanne correspondingly said: “yes”, “oui”, “oui” and “I think yes, but I don’t have the experience”.

Based on the fact that the 5% level of co2 is used all the time and everywhere in the cancer research labs around world, and based on the fact that the lab members have said that they have never thought about the issue before, it is clear that this issue is of huge magnitude. In other words, if the level of 5% turns out to have potentially negative consequences on cancer research, then these consequences are valid for all cancer research in the world and not only for this lab. It would potentially skew results systematically and everywhere, around the world. As we have detailed in subsection (a) the issue seems to be trivialized prior to our research, although it was reported to be important. It continued to be trivialized by Paul, Cédric and Brian, even during the interview.

I will now address the “how” part of the (de)trivialization dynamic. Prior to my raising of the question, trivialization seems to have happened unnoticeably, and the 5% level was taken for granted. In the subsection (b) above, I have already argued that there are hidden non human actants of trivialization, which surpass the individuals or the groups. I have detailed how the trivialization is rather a condition than a practice; a default situation rather than

a choice. I have also argued that prior to our research; the trivialization could be a practice that occurred unnoticeably. In the previous subsection (c) I concluded that Paul's, Brian's and Cédric's practices of trivialization were rather oriented to fulfill the detailed functions of their respective stances. Also, in the diachronic analysis, I detailed how the participants' accounts were clearly and repeatedly oriented into trivializing and detrivializing the issue, depending on how I formulated the question. Hence, trivialization is practiced in 2 ways: first unnoticeably by everyone (prior to my research) and then noticeably especially during the diachronic interviews (because the participants focused on defending this or that position, even at the expense of consistency).

Table. 1 The (de)trivialization dynamic (case 1)

What is (de)trivialized? (Object)	• The skewing potential emanating from the usage of the 5% level of co2, in cell culture.
Who is (de)trivializing? (actants)	• Prior to interviewing Trivialization (no signs of detrivialization): A condition of unidentified origin that is beyond individuals and groups. All lab members (unnoticed choice prior to our research). • Synchronic interview: Brian and Cédric first trivialized then detrivialized; Paul first detrivialized then trivialized; Everyone else detrivialized and increased detrivialization. • Diachronic interview: Everyone increased trivialization then detrivialized.
Why does this trivialization occur? (causality)	**Accounted for:** • A standard that is accepted worldwide; the need to replicate research elsewhere. • Inability to consider all parameters; practicality; a taken-for-granted practice. • "Dictated by the medium available commercially". • "Laziness", "not to be obliged to revalidate one's research". • Can be corrected in vivo, "minimal limitation". **From analysis:** • Prior to interviewing: Trivialization of ideas that do not align with standards; Groupthink. • Synchronic interview: Brian and Cédric (from trivialization to detrivialization) and Paul (from detrivialization to trivialization): unknown motives, but contradictions and signs of dissonance avoidance were clear. Everyone else increased detrivialization synchronically: unknown motives, but detrivialization increase was correlated with how the interview questions were asked. • Diachronic interview: Everyone increased trivialization diachronically, but ended with detrivializing: unknown motives of trivialization increase, but detrivialization was correlated with how the interview questions were asked.
When is this trivialization occurring? (time)	• Frequency: All the time; reported to be as a worldwide standard. • History: Not known exactly, but reported once to be over the last 50 years or more. However, at least one case where 10% of co2 was used in a PhD research, a couple of years ago; but was accounted for to be a "strange" case.
Where is this trivialization occurring? (spatiality)	• Places: In this lab, for all the incubators (observed and accounted for). • In all other labs / a worldwide condition (accounted for).
How does this (de)trivialization occur? (manner)	• Prior to interviewing: Taken for granted; unnoticeably. • Synchronic interview: Willingly (Paul, Brian and Cédric). • Diachronic interview: Willingly and defensively, with signs of dissonance avoidance.
How serious is this trivialization? (magnitude)	• It can potentially skew in vitro research results.

3.3.4 Primary findings (case 1)

In this section, I will outline the primary findings of case 1. In section 4.1, where I will outline the findings of the whole research, these primary findings will be squeezed up with those of cases 2 and 3. While the primary findings will be specific for each case, the whole research findings will be reshaped to fit the 3 cases altogether, before moving to the discussions (section 4.2).

Discoveries

First, I found that the lab participants have never discussed why they use 5% of co2 in their daily cell culture operations, prior to my raising of the question (I achieved this finding through observation- section 3.3.1; and based on their answers to question 2 of the individual interviews: *have you heard this question before?*). The participants were surprised by my question and were initially not able to provide answers (saying typically: "I don't know", "it's a shame that we don't have an answer" and the like). At the same time, the participants said that the use of this level is extremely important and that they should not change it, by any means. Later in discussions (during observation), the participants figured out more or less the same answer to my question: *that the 5% is important for the pH regulation*. With further detailed discussions, I found that the different lab members' responses were quite variant and sometimes contradictory (see details section 3.3.2.1). Likewise, accounts of the same person were sometimes inconsistent (section 3.3.2.2). During the individual interviews, new ideas emerged out of the detailed discussions; and participants shifted of positions a number of times. All the participants said that the 5% level is a standard (or a norm) that is accepted worldwide, but all of them have accounted for the negative consequences of this standardization. They said that it may potentially skew any of their experiments, which they do on cells that do not require 5% of co2 (i.e. phenotype change, shifting from in vivo model, blinding).

Secondly, I found that the use of the 5% level was *a seen but unnoticed practice* (Garfinkel, 1967) and that my question (which aligns with Garfinkel 1967's strategy of breaching experiment) triggered unprecedented discussions. Hence, more and more detrivializing accounts emerged and gained ground. Additionally, I found that one postdoctoral researcher had- to the surprise of her colleagues- used 10% in one of her previous labs, although all

the participants said that any level other than 5% would not be acceptable; because it would change the cells' pH, and therefore skew their experiments. The post doctoral researcher said that she does not know why this lab is using 5%, although she said that she already knows that this level is a worldwide standard.

Thirdly, the origin of the standard of 5% was not known, but the lab director concluded during the interview that it was initially "dictated" commercially and did not emanate from research. He said that the providers who were first able to produce a medium with a pH of 7.4 (which was solicited by research) dictated the level of 5% of co2. Hence, the 5% level is successful to reach a pH of 7.4, but its drawbacks for other considerations are at least according to our participants not known and have never been discussed. More importantly, some participants realized (initially through a discussion between one participant and me) that they are using the 5% level even for cells that do not need a pH of 7.4, but rather 6.5. Some participants said that 5% level was appropriate for any desired pH level; whereas others assured that the 5% is specifically for the cells that need a pH of 7.4. Because the lab uses the same incubators (always regulated at 5%) for all types of cells (those that need a pH of 7.4, 6.5 etc.), the participants who said that 5% is only suitable for a pH 7.4, clarified that they add buffers to cells that require 6.5 pH.

(de)trivialization

With regard to the individual (de)trivialization practices, during the synchronic interviews, some participants already started with some degree of detrivialization and increased their detrivializing accounts by and by, during the focused discussions. Other participants (one of them stepped back from his accounts that he made prior to the interviews) started by trivializing the negative consequences of the standardization of the co2 level and ended the interview, by detrivializing it, mentioning the possible skews. The lab director detrivialized the issue all along the interview, justifying his accounts with details of the potential skews, but suddenly at the very end of the interview, he concluded with ungrounded trivializing accounts that the skews are "minimal". I also found that when I pointed out a contradiction between two accounts or a problem regarding a particular practice, the participants responded to my comments, reframing the dissonance or trivializing the importance of the problem. To do so, a number of discursive

tools were used: 1) Switching from the first personal pronouns “we” and “I” (also their equivalents “nous” and “je” in French) to the second personal pronouns “you” or sometimes “people” (also “tu” and “des gens” in French). While the use of “we” (and equivalents) referred to practices that emphasize that things are under control, the use of the inclusive “you” referred to weaknesses implying “you too would do what we’re doing”, or to practices that are hypothetical rather than confirmed to have been implemented; 2) Using “also”, “sometimes”, and “it depends” to join 2 incongruent ideas and to minimize the contradiction. Finally, I found that some participants used the “yes but no” while others used the “no but yes” strategies. 3) *“Yes but no”* consisted of agreeing on everything and admitting all the weaknesses of a particular practice, but disagreeing (at the very end of the interview) with the idea that the discussed practice should be revised, without backing up such position. 4) *“No but yes”* consisted of disagreeing and challenging all critiques to the practice, but agreeing that the practice should be revised.

During the collective and diachronic interview, the participants started by trivializing the consequences of the standardization of the 5% level. They initially made short and unproblematic answers, as if we have never discussed the complexity before. Furthermore, they did not contradict each other even though they had already made contradictory accounts during the individual interviews. Only when I pointed out the contradictions that I found after analyzing the individual interviews did the participants start to account for the negative consequences of the standardization of the co2 level. The same skew risks emanating from the standardization, which were accounted for during the individual interviews were then again accounted for during the collective interview.

Finally, the participants did not stick to one position, but rather restlessly oscillated between two opposite positions. When they detrivialize the negative outcome of the standardization, they provide justifications (i.e. phenotype change, shift from in vivo model, blinding), but when they trivialize it, they mention two reasons: 1) that they need to standardize, in order to align with oncology research practices worldwide, without detailing the advantage of such an alignment; and 2) that other labs if they want to replicate any of this labs’ experiments, they will do it using 5%. Furthermore, I found that the participants avoided on a number of occasions to respond to the following 2 questions, regarding the two reasons they mentioned: *1) why do you need to standardize a condition that*

you yourself say that it may skew your own experiments? And *2) if other labs are interested in replicating your experiment, why don't they use the level that you would anyway mention in your research protocol and which you would decide to be suitable to your type of cells?*

Case 2: How appropriate is the medium usage?

This chapter is closely related to Appendices C1 and C2. Appendix C1 consists of the series of the individual interviews' transcription; and appendix C2 consists of a collective interview transcription. The transcription lines are numbered, for convenience of reference. Every verbatim account that I will use in this chapter will be referenced using their respective line numbers.

3.3.5 Background (case 2)

The medium[43] is a liquid composed of a number of substances injected on the culture cells[44], on which the researchers do experiments. During the observation and also in the tape recorded interviews, all the researchers told me that depending on the experiment, they are supposed to reconsider the medium components, knowing that some components should always be there (example the glucose). The first time, I asked Brian about it, he told me that the medium is the nutrients of the cells and that "it replaces the food and drinks that humans consume". The medium is usually pinky, especially when it is fresh (i.e. not yet injected on the cells). After spending a couple of days on the cells, the medium changes of color and becomes less pink and more or less yellowish. When I asked, why the color was changing, Stephan said that it is due to the consumption of the different nutrients by the cells. He said "if it is less pink, then it is more acidic". Then he said that the medium has to be replaced regularly, in order for the cells to be in a healthy and non toxic environment. One technician from the cardiology lab told me that the change in color is also due to the fact that the cells discharge their waste and gases inside the medium, so the medium is no longer as clean as before. I understood that the medium is used constantly, in any single experiment. Cells are never without medium. I asked Sophie (the physician and PhD student from the cardiology lab) if the medium is used everywhere else, in other labs. She said definitely, because without medium cells would die, immediately.

[43] See glossary for a scientific definition.
[44] See glossary for a scientific definition.

I had two main concerns with this way of exposing the cells to the medium. First I wondered why the culture cells are constantly covered by the medium, unlike the cells in the human body. After all, humans eat and drink from time to time and not totally exposed to nutrients. When I asked this question to Stephan, Brian, Joanne and Yassine[45], they did not seem to have taken it as a serious issue. In different ways, they all more or less said that the blood in the human body is actually feeding the cells in a constant way, and that I should not compare the medium to the food and drinks, but to the bloodstream. Bottom line, contrary to the issue of the co2 level, this medium issue did not seem to have drawn the attention of the researchers. However, I had very contradictory accounts, which were reproduced during the interviews, and which I will discuss in the next section.

My second concern started to build up when I noticed that the usage of the medium is done in a mechanical way. It is perhaps the most frequent operation that one can see in the lab. Many times a day, I saw the researchers, students or technicians grabbing bottles of medium from the stock and injecting a quantity on the cells (which are placed in petri dishes[46]) using pipettes, after removing the old medium. The suction of the old medium and the injection of the fresh one are operated in the culture room, inside the hood[47]. This operation like many others is done with very strict caution, because there is a risk of contaminating the cells with bacteria, fungi or viruses. Strict rules are posted on the hood, in order to remind the users of how to avoid contamination. Basically, everything that comes in contact with the cells or near it, particularly inside the hood, has to be sterilized[48] immediately before use or has to be brand new (which is supposed to be sterilized by the provider). The medium is by excellence the substance that is constantly in contact with the cells, which also means that it has to be permanently sterile.

Because I saw the participants using the medium in a mechanical way, I asked myself the question: do the researchers think about the medium or is it one of the unnoticed elements of their research. It was not possible to answer the question just by observing or without discussing it with them, because if they may think about that

[45] Bachelor's student who attended the lab sporadically, not in the list of the lab members or of our research participants.
[46] Petri dish: A shallow, circular, transparent dish with a flat lid, used for the culture of microorganisms (downloaded from http://www.oxforddictionaries.com).
[47] Hood*: An extractor hood*, one that removes particles usually by sucking them up.
[48] Make (something) free from bacteria or other living microorganisms (downloaded from http://www.oxforddictionaries.com)

individually or during the design of the different research protocols. Therefore, I asked Brian, who told me that they do think about it very carefully and that their lab is specialized in the study of the cancer metabolism, which means that these conditions are of particular importance to them. I addressed the same question to Stephan and to Daniel who said the same thing. I then asked Joanne, who told me that the medium is usually purchased readymade, but it can sometimes be made within the lab, using different raw ingredients that are themselves purchased. I went back to Daniel and told him that I wanted to further discuss this issue of the medium use. The discussion with Daniel made me conclude that unless the research is testing a particular ingredient of the medium, researchers usually use a standard one; a readymade one, which is available in the market. I asked Daniel and then all the lab members (except Dilara and Paul) if they knew all the ingredients of the medium that they are using, by heart. They all said no.

What came to me as a surprise was that the lab members do not know the whole list of the ingredients of the medium in which the cells live, although they keep saying that in biology every molecule can make drastic changes, if we are not aware of its presence. Brian said (and he repeated this in the interview) that the list of the medium ingredients is too long to remember, but before deciding what medium to use, they think about it, thoroughly. Further discussions with Stephan left me with three ideas: a) that most of the time the medium is taken-for-granted, unless the particular research is specifically testing one ingredient; b) that the medium is only thought of, when they notice a problem such as a strange behavior of the cells; and c) that the participants are making efforts to show me that this is not an issue and that everything are under control. Bottom line, I had a feeling that the participants did not like to discuss about the medium with me. I decided to design a semi structured interview dedicated to the medium issue, because I suspected it to be an instance of trivialization- a trivialization about something that is otherwise described by the researchers to be extremely important. I was intrigued by the paradox of: 1) saying that any non identified molecule in the medium would have implications on research and 2) not knowing what the medium contains.

3.3.6 Interviews analyses (case 2)

I initially designed a 12 question-semi-structured interview, which I conducted with each participant individually, between June and September 2015 (Appendix C1). Then I conducted a collective interview in February 2016 (Appendix C2), which went through the same questions. While the individual interviews will be analyzed synchronically in the subsections 3.3.6.1 and 3.3.6.2, the collective interview will be analyzed diachronically in subsection 3.3.6.3. The collective interview was inevitably less organized than the individual ones, because the participants anticipated many questions, but I nonetheless made sure that I went through all of them.

The individual interviews questions are numbered from 10 to 21. Depending on the participant's answer to each question, further discussions were made. The different interview questions were designed to invite the participants to account for the appropriateness of the medium usage. More specifically, I targeted the two issues related to the medium usage, which I described in the previous subsection; namely (1) the fact that the medium is constantly on the cells and (2) the possibility that it can contain molecules that no one would know of or expect. The following is the list of all the 12 questions:

10. Why are the cells constantly in the medium and not exposed to it like humans take nutrients?

11. Have you personally thought about this question before?

12. Do you think that this question is an important one?

13. Do you think that this question has been trivialized?

14. What is likely to happen if we can make the medium contact the cells in the same way as how the cells in the human body contact the nutrients?

15. Do you think that the cells need to spend some time without any medium?

16. If cancer is a molecule that is causing a disorder in the regulation of the cellular reproduction, would it be possible that the molecule stay in the medium, get multiplied there, and then contaminate the others that may have been cured?

17. Do you think that the medium is too heavy for the cells?

18. Do you think that the cells that are immediately glued to the wall of the containers (dishes, wells etc.) may not receive the drug, which you want to test on them?

19. If the temperature and the co2 level that you are using in the cell culture happen to be not the same as the cells in the human body would have, does this cause the cell lines to be more resistant to drugs?

20. Do you trust that the company from which you buy the medium is providing you with the medium that you really want?

21. Are there risks that if the medium components are not exactly as they are described, they would make the cells develop resistance to drugs, or different protein expression?

3.3.6.1 Accounts variation (case 2)

For the convenience of reference, the participants' accounts were reordered by question, not by participant. Thus, we can spot and compare all the participants' accounts on each question. If we want to reorganize the interview by participant, we can do so, by first selecting the participant's initials (example RM) and second trace his or her numbered answers from 10 to 21 (example. RM's answers are from RM 10 to RM 21).

Before we start to illustrate the variation, it is important to remember that studying the accounts' variations is a way to unveil the disagreements over agreements. In other words, what is supposed to be largely agreed upon, at least in the practice (i.e. the usage of the medium in the same way) turns out to be problematic and disagreed upon (i.e. giving different accounts or explaining in different ways). Therefore, what we wish to stress on, in this section, is how the trivialized matters in practice hideaway issues of very important considerations for research (using the researchers' own words). The variation that interests us in this regard is how the different lab members would respond to the concern about skewing the research experiments due to: (1) the fact that the medium is constantly on the cells; and (2) the possibility that it can contain molecules that no one would know of or expect.

(1) that the medium is constantly on the cells

Two positions

With regard to the fact that the medium is constantly on the cells, at least two clearly diverging accounts emerged right from the first question. A group of three participants made up of two post docs (Stephan and Daniel) and the lab director (Paul) said that the system does not really mimic the human body, but for technical reasons there are no better alternatives, at the time being. Although the three of them mentioned that a new system has recently become known, the issue for this group is rather the unavailability of better technological or financial alternatives. For Stephan, it is a matter of practicality "well it's it's it's a practical issue like you can (.) provide both continuous flow of nutrients and remove the waste products but that requires like very special equipment and more complex systems to be set up and that will be (.) much more time consuming and costly in running and less practical than the system what we use now (.) so it's just a matter of choice for the practicality " (line 2299). Paul was very short and mentioned that it is "because we (.) cannot easily do otherwise" (line 2525). When I mentioned the new system to him, he took six seconds of silence and mentioned its necessity, but also mentioned that it is still not well established: "well that you you will u::h be closer to the:: the:: the reality (.) you will not start with a:: high amounts of nutrients and amino acids to be sure that after 2 days you to have enough for the cells to proliferate in your in your plate (.) u:h you you will constantly expose and to quite do the same amount as it is when blood is u:h constantly fueling new stuff from the from the nutrition (.) ↑ this kind of of setp up start to be u::h developed it's ( ) quite anecdotical right now". Daniel said in French almost the same idea as Paul, i.e. that the need is there, but the system is still not standardized: "u::hm ben d'une part il faut quand même que les cellules soient dans un milieu u:h nutritif qu'elles aient besoin de tout ce qu'il leur faut u:h proliférer donc u:h comment dire glucose acides aminés et cetera ↑ après u:h il y ait eu des systèmes un peu plus élaborés (.) c'est un système de pompe qui fait que les (.) effectivement les nutriments sont apportés un peu comme une circulation sanguine c'est quelque chose de dynamique u::h et ça clairement ce sont des:: fin des conditions qui pour l'instant ne sont pas: ne sont pas standards (.) là j'ai vu que c'était l'année dernière où ça a été publié u:h ce type d'appareil " (line 2357).

On the other side, stands a group of six participants (two post docs: Brian and Dilara; two PhD students: Cédric and Gaelle; one technician: Dalila; and one master student: Joanne). This group has accounted with varying degrees that the current usage of the medium (being constantly on the cells) functions properly: mimicking how the human body consumes nutrients. With a slightly nuanced account, Brian said that "the medium mimics a bit the: blood (.) that feeds all the cells in the body (.) but in the blood you have like a range of glucose and you have less of this glucose you will start to take fat and usually start to generate glucose or energy (.) so there is an homeostasis in the blood and in the body and I think the medium tries to mimic this uh system (.) I I I wouldn't say that in the body there are a lot of (.) very drastic change in the: in the food and in the concentration of nutrients" (line 2317). With a rather more determined way, Dilara said that this medium is doing the similar job as how it works in the human body "donc pour moi ça reste quelque chose de similaire de ce qu'on peut retrouver dans le dans le corps humain vu que on a des des vaisseaux et puis des tout petits vaisseaux qu'on appellent des capillaires sanguins qui vont irriguer les cellules de la même façon" (line 2392).

Cédric was as determined as as Dilara: "but your b:ody is quite the same (.) I mean you eat from time to time (.) so like you eat and then you take water but in your blood stream you are constant (.) nutrients level like you have in the cells like medium" (line 2475). But Dalila's account was at the end of the continuum. For her, the current system is not only close to how it works in the human body, but there is no need to be that similar: "u::h mmm poff (.) je pense que le fait de manger ça apporte des nutriments mais au final c'est quand même dans le sang d'une manière constante (0.12) […] à part à part le glucose qui fait de l'hypo glycémie je pense que le reste y a des équilibres qui se font y a des trucs qui sont transformés des machins comme ça donc au final t'as (.) et puis ça reste aussi toujours de l'in vitro quoi il y a des artefacts (0.8) " (line 2546). But perhaps Dalila's job as a technician requires her to master what is in force rather than to think about alternatives not yet made available to her.

In a less determined way than anyone among the six group members who are at the beginning of the interview contented with the current system, Gaelle said that the culture cells are constantly exposed to the medium, because

the human body's cells too are exposed to nutrients all the time. For her, the body's cells know how to get nutrients from the "stock", but since the culture cells are not able to do that, exposing them to the medium in a constant way is a necessity. She said: "Because in your body you have you can have a stock of lipids you can have stock of glucose uh other uhm glygogene glycogen (.) so you've always u::h a stock of energy somewhere (.) for the cells it's different they don't have like uh lipid uh somewhere in uh the bulk bulk? ventre? ((laughter))" (line 2442). The most noticeable account among all of the participants is Joanne's. Joanne belongs to this group of the six participants (those who first seemed contented with the system), but she is in complete contradiction with them when she explains that the culture cells consume from the medium's nutrients only when they need, just like the body's cells consume only when they need. She said: "again it's (.) in the body it's a complicated (.) thing (.) it's a complicated mechanism (.) when you feel hungry there is a message will be like trans:mitted to the brain and the brain will ask y your stomach to eat and will u:h you will you will go and eat something and when you eat another thing like we don't take u:::h the food is not different from the food you put on the cells and it's another mechanism and stuff but the cells you put always medium (.) nutrients because you you don't know when they will need it (.) when they will need to u::h to take the nutrients (.) so you put like that for for 24 hours (.) and when you will need they will take (.)" (line 2582). Basically, Joanne agrees with the second group, but explains things as the first group.

Positions reshuffled

Now, except Dalila, the two groups (the one that seemed to be contented with the usage of the medium and the one who said that they needed a better system) will collapse into one single group, as they will converge that this issue is of considerable importance. This change of positions was triggered by the question number three: "do you think that this issue is an important one?". Stephan said that: "↑ I think it's important (.) I think you have to (.) < if you are (.) every ( ) of profiling enough clean medium uhm nutrients at regular intervals is important otherwise your culture cells are affected and >" (line 2805). Paul said: "definitely (.) and I I I keep (.) every time I'm part of a jury (.) not this only in my field I keep asking the question are you sure that u:h u:h what you see is not influenced by the deprivation of your medium u:h in one nutrients?" (line 2895). Daniel was as determined as Paul and said yes this

question is constantly in “their minds”; and that in their projects, they all the time think about what they put in the medium, in terms of quantity, quality and dynamism: “oui oui oui ça c'est quelque chose qu'on a toujours à l'esprit fin::: dans nos projets (.) c'est: réfléchir le mieux possible à ce qu'on met dans le milieu à quelle quantité combien de temps (.) la dynamique ” (line 2828).

From the second group, Brian said that the question is important because “it's important you have to the: the same levels of nutrients more or less all the time” (line 2821). Dilara said that the issue is important, because we have to be sure that the changing of the medium would not skew the experiment: “Oui oui je pense que c'est important euh c'est important dans différents cas fin il faut se poser la question parce que il faut être sûre que l’expérience qu'on va mettre en place euh ne serait pas influencée justement par le renouvellement du nouveau milieu” (line 2837).

Gaelle said that it’s important, then ‘no’, then again ‘yes’, then again not important for the reason I mentioned, but because the cells need to have a constant concentration of the ingredients, which is basically what the first group (Paul, Stephan and Daniel) had said right from the beginning. In other words, at this level, Gaelle provided the same argument on which the first group built their position (of not being satisfied with the system). Perhaps, Gaelle is figuring out the idea little by little: “yes (.) important not important but it's uh yes it's important as well […] no for me it's more the::: the concentration of each stock not really the fact that it's exposed all the time or not (0.5)” (line 2880). Cédric too seemed to be figuring out the importance of this issue at the time of explaining to me, especially that he said in question two that he never thought about this issue. Cédric said “yeah it might be like when I was think for endothelial u:h cell there are in your life exposed to flow because they are inside the blood vessel and when you are cultiving cultivating it here in the lab they don't have the same flow so may be important” (line 2886). But Joanne was even more skeptical and withdrew her answer. She first said: “Yes”, but when I asked why, she said “hum::: (.) ↓ we because (.) it is important to know if we can u::h like ↓ I don't know exactly how to say that (0.5) I don't know (.) because if we: remove the medium the cell will die (0.4) I will have to think about it ((laughter))” (line 2925). Finally, Dalila was again at the end of the continuum and was the only one who said that

this question is not important. She said “non”, because as long as we know that in cell culture we don’t totally mimic the reality, there should be no problem. She also said that we do not ask such a question and that she never did: “non (.) à partir du moment où on travaille sur des cult sur des cultures cellulaires on sait que il y a des choses qui:: (0.2) qui ne sont pas:: fin que c'est pas tip top avec ce qui se passe en vrai donc:: après on se fait: on se fait une raison là dessus et puis on avance quoi et puis on se pose pas ce genre de questions (.) avant je me suis jamais posé ce genre de question” (line 2916).

So far, I have noticed that question two has triggered a slight change in the participants’ positions (except Dalila’s). The most important idea in this change is the fact that the group, which was first contented with the system, figured out that the system is at least not doing the best mimicking job needed, at the time of answering question 3. It is only when they had to account for the importance of this issue, and more particularly when I asked “why” that they provided elements on which the first group had initially used to build their opposite position (of not being contented with the system). We will now see further variations triggered by question 4 “do you think that this question has been trivialized?”

Detrivialization

For the third time, Paul, Stephan and Daniel have provided consistent accounts. They all said that the current system is not totally satisfactory, that this issue is very important for research and that the issue has been trivialized; providing coherent explanations on why this issue should not be trivialized any more. Paul was short on this, his position suggests that the fault is people’s practices: “yes (.) clearly (.) people do not (.) they underestimate this point” (line 3035). Stephan wanted to avoid the term trivialize perhaps he meant that people do not underestimate this issue, but perhaps they ignore it. During explanation, Stephan decided that yes it is actually a case of trivialization: “I think at sometimes it's (.) it's not very trivialized but sometimes people they are n::ot taking it into consideration as much as may be as it should” (line 2934). Then he said: “u::h may be it's trivialized in the sense that it's very easily to be ( ) too much and we have tricks and buffers that can u::h (.) you know compensate for the medium problems and if

you once in a while replace it (.) it's a big enough substitution (.) so I guess it's somewhat triviali::zed but also to the other extent I think in most cases in practice (.) I think that the:: effect might be marginal". Finally, Stephan accounted on the why people trivialize it: "because n (.) there is (.) it's easier to: do it in this similar way (.) it's more (.) → it's cheaper it's faster it's (.) yeah it's u:h (0.3) it's the falsely cheaper". Daniel admits that it is trivialized by many scientists, but at the same time considers that it is less trivialized in labs that are interested in the cancer metabolism as such: "u:h je dirais un peu négligée (.) uh par une grande partie de: des scientifiques j dirais un petit peu moins dans (.) les gens qui travaillent sur le métabolisme puisque ça reste à uh fin le les nutriments les choses comme c'est clairement notre uh notre domaine donc on y pense un petit peu plus (.) mais clairement fin par exemple si je compare à ce que je faisais pendant ma thèse (.) je réfléchissais pas trop à ce qu'il y a avait dans mon milieu la concentration en glucose ou en glutamine (.) pour moi c'était des conditions standards qu'on m'avait donné (.) mais j'avais pas en tête uh:: ben l'impact que ça avait sur uh sur les conditions des cellules (.) donc je pense que inconsciemment il y a quand même un: oui une partie: de trivialité: sur ces conditions de: culture et des milieux" (line 2972).

Now the participants, which we initially classified as the second group (the group of six), and which almost joined the first group at the level of the third question, will now provide even more contradictory accounts. Brian, who is now explaining how "some labs" are trivializing this issue, is finally stating the negative aspects of the current system, on which the first group built their opinion right from the first question (that the system is not satisfactory). Brian said: "but I would say labs working more in: (.) more distant from metabolism (.) it could be an issue (.) you don't take care of this u:h problem […] so if you use cells with fresh medium or you use cells with u:h medium which has already used all the glucose or the amino acids and already the cells secreted a lot of lactic and reacidify the medium (.) you would change the activity of proteins (.) activity and expression and whatever (.) it would change things for sure in the cells (.) so it's important to work always in the (.) more constant" (line 2957). Dilara said exactly the opposite of what Brian has just said. Brian (and also Paul, Daniel in particular) had stressed that their lab is actually one that pays particular attention to this issue, because it works on metabolism, as such. For Dilara, it is

not that important for a lab like this one to focus that much on issues such as the medium's ingredients, but it is important for other labs: "Je pense que tout dépend du laboratoire je crois qu'ici la question est n'est abordée parce qu'on on se focalise vraiment sur des aspects très très précis euh de la cellule tu vois avec on va vraiment très très très loin dans les processus de biologie de biochimie (.) donc on va étudier l'enzyme alors que si dans des études tu vois on étudie le passage nutrIment ou alors l'exposition d'un polluant ce genre de chose là je pense que c'est plus qu'important de se poser la question (.) mais ici finalement on étudie pas au niveau de la cellule on est bien plus profond" (line 2986). There is now, no doubt that Dilara's account considers that this lab is not one of those that should focus on the issue of how the cells consume the nutrients, while Paul, Stephan and Brian repeated many times, that their lab is in particular one of those that should pay attention to this issue. More examples illustrating the fact that Paul, Daniel, Brian, Stephan, Gaelle, Dalila and Cédric accounted on that their lab is focusing on cancer metabolism, which makes the medium and nutrients issue even more important than in other labs are in the following lines: (2871; 2954; 2972; 3473; 5491etc.).

Gaelle replied to the question whether this issue has been trivialized or not, referring to herself. She said what it means that she had never considered this issue, because she has never heard this question, but also because the cells need to be in the medium "I never heard this question before (.) […] Because::: uh:: right not uhmm (0.3), because for me we're always exposed to:: to nutrients so:: and because uh::m cancer cells need uhm nutrients to grow so they have to be exposed" (line 3020). Clearly, Gaelle is undecided; she sees the necessity to have the cells exposed to the medium, in order to grow well, but she also had accounted in question 3 that the current system is not able to provide the cell lines with the same concentrations of ingredients, as in the human body. Again, Gaelle seems to be concocting answers and building an opinion on the go, particularly that she said that she never considered this question before. However, Cédric, who too was initially in group 2 (those who first said that this system is not problematic), and who changed of position in question three, is now joining group one, once more. It seems that question 3 and four alike has triggered this change in Cédric's account. He said: "yeah yeah yes because you have some specificity when you need flow like to (.) to test for u:h (.) a lymphocyte and endothelial cell work condition (.)

if you don't have the flow they will just grow on the cells and that's all but if you put some flow they will start rolling on the cells may be stopping so you need the flow sometime" (line 3028). Bottom line, the difference between Gaelle and Cédric, is that Gaelle became more confused, when I asked her to account for the importance of this issue (question 3) and on whether it was trivialized (question 4), while Cédric became more decided that it is actually important and that it has been trivialized. Cédric ultimately mentioned that the flow rather than the constant exposition of the medium is a necessity, nuancing his account with the term "sometime" at the end.

(2) The possibility that the medium contains molecules that no one would know of or expect

Question 16 triggered the most surprising variation. The question invited the participants to account for if cancer is caused by a molecule, would this molecule stay in the medium and be behind the continuous contamination of the cells? During the discussions around this question, I also asked whether this contamination could be the cause of cancer cells resistance and aggressiveness. I wondered whether it is possible that some cells may get cured, but because they are in the same medium, they may be contaminated again by other cells that did not get cured.

Daniel was the only one who said that cancer is not a molecule, and we should not even see it as caused by a molecule. All other lab members said at least that cancer can be caused by a molecule. Daniel first said in French that he does not think that cancer is a molecule. Then, he said that cancer is more complex than just a molecule that comes to affect the cell. It is a number of modifications that happen to the cell, he said, which causes cancer and gets it to metastasize: "alors le fait de voir le cancer comme une molécule (.) ça je pense pas fin:: (.) c'est quelque chose de beaucoup plus complexe que: juste une cellule et on voit une molécule qui arrive et qui: qui affecte: la multiplication (.) c'est plus un ensemble de:: de modifications au niveau de la cellule qui qui fait qu'il va y avoir un cancer qui va être qui va se métastaser'' (line 3961). Therefore, Daniel sticks to the definition of cancer as a condition or disease and did not even accept the idea that this disease is caused by a molecule, which makes my question nonsensical, because if cancer cannot be caused by a molecule, than the idea of the medium contamination would not be a possibility. Although Daniel's account is very clear, I tried to tell him that I can understand that

cancer is not itself a molecule, but at least it is caused by a molecule, he said again no, this is not a possibility. Cancer is just a number of mutations that happen to cells, and that has nothing to do with molecules. I asked what is actually behind this proliferation, he said again it is a disorder in the cell division (see line 3950 for further details). I understood that maybe I should have said that cancer is *caused by* a molecule, not cancer *is* a molecule, but Daniel again refuted the idea that cancer is at least caused by a molecule.

All the other lab members have however admitted that cancer is a molecule or at least caused by a molecule. Stephan said “cancer? no it's not a single molecule”, but then immediately said: “Yes (.) indeed (.) some compounds produce like cancer cell that maintains their cancerous state” (line 3831). Brian said: “yes” to my question, then “it can be yes it can be a molecule and it can be a molecule that is secreted from the cells yeah could be yeah yeah” and then “there are some examples of that so yeah” and again “yes it can be”. When I told him that Daniel did not agree that cancer is a molecule he said: “cancer is the disease that (.) the disease could be caused by u:h virus and a virus is a molecule” (line 3938). Dilara said it is possible in the sense that it is a virus that causes cancer: “Dans le sens ou par exemple il y a certains virus qui causent des cancers”. When I told her what causes the prolifereation? Can it be a virus? She said “yes or a molecule”: “oui (.) ou bien une molécule’’ ; I said a molecule ? She said yes (“oui”) (line 4026). Paul said: “you mean a molecule causing cancer?”; “yes they are mutagenic comp there are mutagenic compound which will will promote the proliferation of cancer cells” (line 4279).

Gaelle said simply “why not it could be” (line 4178) to the idea that cancer can be a virus or a molecule and can even cause contamination. Cédric too agrees that cancer can be caused by a molecule: “it can be different stuff it can be u:h molecular so like you have glycogen that are u:h u:h ((breathing out loud)) increasing the risk of mutation on the genome that causes cancer” and then he added “yeah often often the:: (.) molecule that you're speaking about are environmental molecule” (line 4194). Dalila said “yes” (“oui”) to my question on whether cancer is caused by a molecule; and again “yes” when I asked if this is possible (“oui”) (line 4434). Finally, Joanne too said yes twice to the idea that cancer can be caused by a molecule (line 4461).

An unprecedented discussion

Now, the remaining part of my question (question 16) is the most confusing for most of the participants. My question presupposes that the drug that cancer researchers test on cells are supposed to kill the disease not the whole cell, but Brian and Dalila corrected me on one important matter. They said that most of the time the drugs are actually designed to kill a number of cells (some of them will anyway stay alive and keep dividing), and sometimes the drug is able to target the cancer cells specifically (leaving behind the non cancerous cells). The aim is to keep killing the cells, not the disease (randomly or in targeting way) until you reach a level where you do not see them proliferating anymore. Beyond this confusing idea, I will now address the remaining part of the question, related to the possibility of contamination through the medium. Stephan and to a lesser degree Brian and Gaelle said that the medium as it is currently used, can cause repetitive contamination, and therefore if an experiment is testing a drug on the cells, it can be concluded that the drug is not effective, when it may well be, in fact, effective. The experiment would in that case be reported negative, when the problem is not in the tested drug, but in the medium that is skewing the experiment. The problem may then be in the fact that the medium is contaminating the cells again and again. However, all the other participants do not see this idea of medium contamination to be a possibility.

To start with, Stephan initially confirmed the possibility of contamination through the medium (line 3382), which is already half of the answer, but I then reminded him that my question is not about the medium of cancer cells contaminating non cancer cells. I clarified that what I meant was the medium of the same cells contaminating them again and again, while the drug is being tested. He said: “oh yeah for sure (.) it's u:h there's compounds that are excreted and are: that are floating in the medium and it's also known that if you grow cancer cells or other cells you just only take the medium without the cells and transfer it to other healthy cells you might affect their function (.) that happens yeah that's a known hormones grow factors” (line 3850). Stephan also added that although such contamination is possible, researchers are able to know if the drug is not totally effective as a consequence of contamination: “they will the same yeah (.) so but that's then in that sense indeed you might not see the full affect but

uh normally you're canalizing like the whole culture if you are like averaging the effect of the whole (.) so if you would have some drug that would be affecting the cancer may be it doesn't kill all the cells but you should be able to measure some kind of increase or decrease depending on what you're measuring (.) as an overage it affects all of its own culture and if you see a change in that way you know it's a functional the drug" (line 3843).

Brian too agreed to the idea that contamination through the medium is possible: "Yeah it could be (.) it could be it could be but in the beginning I don't think it will be the:: (.) well it could be but the change to adapt cells to develop resistance to something ( ) you need like two months (.) so: it's not something that this immediately created" (line 3925). Basically, Brian also said that contamination is possible but it takes time and because the medium is changed every 2 days or so, the contamination may not have enough time to occur. When I mentioned that contamination in reality can happen before we can actually see it (two months as he said), he admitted saying: "ah of course it starts before yeah of course (.) even after 24 hours you can see changes". Gaelle too said that this is possible, but then said "but we change the medium also" (line 4180). So for Gaelle, if we change the medium there is no possibility of contamination, although the medium is changed after two or three days and according to Brian contamination can occur in 24 hours.

From the other side stand Daniel, Dilara, Cédric, Paul, Dalila and Joanne. All of them do not see that contamination through the medium to be a possibility. Daniel said that this idea of contamination through the medium is definitely not a possibility. He said that contamination can occur through some residue of infected cells, but not a molecule in the medium. He added that some researchers suggest that stem cells may be behind this contamination, but even this theory of stem cells is still not very well established, he said: "je vois bien là ce que tu veux dire mais u:h c'est pas une molécule qui reste dans le milieu quoi c'est […] non'' (line 3999). Daniel was confident that anyway the medium cannot be behind the contamination and the resistance. He said "no". I said it has nothing to do with resistance? He said "no" again.

Dilara too excluded the idea that the medium can cause contamination or that contamination is behind the cancer cells resistance to drugs, because the medium is changed every two or three days. This means that for Dilara, the medium contamination does not happen just because it does not have enough time to happen: “ uhm je ne pense pas parce que le milieu n'est pas tout le temps là vu qu'il est renouvelé et tous les deux jours ou tous les trois jours en fonction (.)” (line 4047). She then explained that that resistance is due to the fact that cells develop new mechanisms of adaptation against the drugs. However, when I asked her, if this adaptation is something that cancer research understands well (how it happens), she said that she herself does not know, but that it is more and more understandable: “moi je la connais pas et je crois qu'on sait de plus en plus et (0.2)”. Basically, Dilara has shifted the discussion from the possibility of the contamination and resistance to be caused by the medium into the discussion about the adaptation, which she said that she does not understand. Additionally, I said in French “so, for you the medium cannot be in any case behind cancer cells resistance?”; she said “no, I wouldn’t say in any case, because the medium components may strengthen cancer” and then she developed her idea giving the example of the glucose to be possibly one of the factors of the cancer cells resistance. Bottom line, Dilara finished her explanation with the idea that the medium can cause resistance, but due to its composition not through the persistent molecules: “Si je dirai pas en aucun cas parce que dans certain milieux tu peux avoir certains composés qui vont euh qui vont favoriser la résistance si par exemple les cellules cancéreuses ont un métabolisme qui est très rapide (.) donc les cellules respirent beaucoup plus qu'une cellule normale elles consomment beaucoup plus de glucose qu'une cellule normale fin elles sont plus euh elles ont un métabolisme plus rapide et donc du coup ben si tu mets beaucoup de glucose par exemple dans le milieu mais ça va favoriser le maintien en vie des cellules (.) maintenant si t'en mets un petit peu moins peut être que les cellules vont se ressentir moins bien et si dans le milieu là tu viens ajouter ton médicament peut être que ce sera plus facile au médicament pour tuer les cellules (.) tu vois peut être que le milieu peut avoir un impact sur la résistance mais maintenant il faut voir aussi si c'est important de modifier le milieu puisque l'objectif c'est quand même de se rapprocher de ce qui se trouve chez l'Homme (.)” (line 4107).

Cédric said that he understands me very well, but that contamination through the medium is not possible, because the medium does not contain the cancer molecule: “so it's not it's not something that you would find again in the medium (.) like you take (.) the molecule A that is u:h going out of the car (.) they are making the cell u::h a cancerous you won't find it again in the medium after (.) it just disappeared make the cell cancerous then you took the cell put in the lab and now it's cancerous because of the (.) molecule again (.) you know what I mean?” (line 4218) Paul too said that contamination cannot be a matter of a molecule that stays in the medium or anywhere else. The cancer cells are already affected with the disease and changed of phenotype, which make them proliferate and divide in an uncontrolled way, without the presence of the cause anymore. Again, for Paul cancer is a disease, a disorder, or a condition. He ruled out the possibility that this disorder is caused by a molecule that is always there. He is as confident as Cédric that the compound (or the molecule) is not there anymore.

Surprisingly, Dalila accounted that the idea of contamination is not possible, not because she does not think that the medium does not contain molecules, but because if the drug is designed to kill cancer, it would kill it in the cells and in the medium as well. Dalila was the first one who mentioned this idea, which now seems so obvious to me: “Le but des médicaments c'est de tuer cette petite molécule qui provoque le cancer et donc si ces petites molécules sont tuées elles ne peuvent pas auto-contaminer ↑ elles ne peuvent pas contaminer ↑” (line 4375). Then, Dalila stepped back from the idea that the molecule causing cancer can be in the medium. She said but for me cancer is not a molecule, cancer is the effect of that molecule on the cells: “Mais pour moi le cancer c'est pas une molécule ((laughter)) c'est pour ça que […] oui mais la molécule elle agit sur les cellules” and then she went back to say that cancer is a molecule causing the proliferation of cells “donc le l::: le cancer est une molécule qui agit sur les cellules tumorales dans notre flasque et fait proliférer nos cellules”. Joanne’s accounts were very similar to Dalila. While she agreed that cancer can be caused by a molecule, she does not accept the idea that the molecule would stay in the medium and keep contaminating the cells. She insisted that cancer resistance is not a matter of contamination, but of adaptation: “but when there is no oxygen it's not degradated and it will activate things in the (.) in the cell and those (.) m: mechanism that will be activated (.) they are related to migration and metastasis (.) this is an example so (.) this

is what I know (.) but maybe there is u:h other things surely there is other things that affecting" (line 4531). Joanne failed to explain what she means by adaptation, or how it happens, and neither had she demonstrated how we can know for sure that the molecules that cause cancer are no longer in the cells and in the medium. After rejecting my idea, Joanne stepped back and said that she does not really know: "No I don't know may be (.) you can't say for anything you don't really know".

Two points that no one except Stephan was able to explain: (1) while I understand that cancer is not a molecule, but a disorder that can be caused by many factors including a molecule or a virus, how do we know that the molecule or the virus which caused cancer has left the cells and is no always there? It was very hard for me to try to solicit an account for this very specific point. None of the participants was able to demonstrate that the molecule is no longer in the cells. Conversely, Stephan's answer is not problematic to understand, because he said that molecules that cause cancer may stay in the cells, may float in the medium, may cause auto-contamination and be behind resistance to drugs. (2) The second point is about the other causes of cancer resistance. All the other participants said that cancer resistance is due to adaptation, but they all said that they do not understand how adaptation works, and what are these mechanisms that cancer cells develop to be resistant to drugs? we will address this issue in the next subsection, within a detailed discussion about how each of the lab members built his or her accounts throughout the interview session.

3.3.6.2 Synchronic inconsistencies and contradictions (case 2)

In this section, we will focus on several inconsistencies and contradictions made within the same interview session by the same participant and detail how his or her accounts evolved throughout the interview. The aim of every interview session was to explore each participant's account with regard to how the medium is or not potentially skewing the experiments (a) because it is constant and not changing like humans would eat and drink from time to time; (b) because–or not- the molecule that may have caused cancer would always be in the cells and possibly in the medium; and (c) because the medium that they purchase readymade or that they use based on readymade substances may contain molecules causing cancer resistance to drugs. The central questions that address the elements (a) through (c) are questions 10, 16 and 20 of the interview, but the other interview questions were equally important, because they are clearly interrelated.

We will start with Stephan. Stephan was able to provide details of what he accounts for and to support his ideas with specific examples, which is not always the case with the other participants, who stepped back from their positions several times (as we will demonstrate in the subsequent paragraphs). Four main accounts and counter accounts made by Stephan will be detailed as follows- (1) through (4) and (1') through (4'):

(1) Stephan said that the constant exposition of the medium to cells may be a negative condition. He said "so why it's important to (.) because if the cells are not receiving enough fresh nutrients and if they are left too long in their waste products (.) may be the quality of the cells will become affected and they will start behaving differently" (line 2809). He also said that the constant method has negative aspects, which once obliged him to manually do the work of the new flow system: "and I understand if I didn't substitute the medium myself that they would (.) deteriorate in quality very fast so for instance like that it would be ( ) better to grow in this in a continuous system of new supplies in nutrients (.)" (line 2617). (2) Stephan also said that cancer can be caused by a molecule and that the medium can cause contamination if the molecule stays in it: "oh yeah for sure (.) it's u:h there's compounds that are excreted and are: that are floating in the medium and it's also known that if you grow cancer cells or other cells you

just only take the medium without the cells and transfer it to other healthy cells you might affect their function (.) that happens yeah that's a known hormones grow factors" (line 3850). (3) Moreover, Stephan was the only one who said that depriving the cells from the medium for a short time to weaken the cancer cells can be a good idea, and that this technique is being already used: "u::h that might be a very good thing because then you keep them nicely concentrated and more susceptible that depending on the drug may be that would be a good thing or a better it depends on u:h" (line 3870). Finally, (4) Stephan admits that the medium may contain compounds that they would not know of, and that this situation can skew the experiments: "well I guess like indeed like (.) yeah may be in you have some interaction you could have some uh ((noise)) yeah in principle you can have inter: interaction between the 2 compounds so if it's different it could have in principle an effect it's possible (.)" (line 5679).

Basically, although Stephan was the strongest advocate of the idea that the medium as it is used today can be skewing the experiments, he is paradoxically not different from others when it comes to practice, because: (1)' he exposes the cells to the medium in a constant way, like all the other participants do. He said that the alternative system is "much more time consuming and costly in running and less practical than the system what we use now (.) so it's just a matter of choice for the practicality" (line 2301). What is quite strange in Stephan's account is that despite the described skewing risk of the current system, he still favors its "practicality". Stephan does not only use the traditional system, but he also accounted for it in a rather favorable way.

(2)' Stephan does not do any special operation to check if the medium is continuously behind the cause of contamination and the resistance of the cancer cells. In the following account, he basically played down the contamination issue (despite the skew risk that he described in element (2) above) and played up the need to use a quicker process than the slow one that happens in the human body: "well in this sense it's also: important like (.) because in the labs I think you want to study something that u:h may be in the body it will take a lo::ng time may in the labs you would prefer to have something that occurs very fast (.) so if you would be removing some of those factors that are being secreted by this flow systems (.) may be you would slow down the whole process (.) and may

be then it would become more difficult to: to study it so maybe that would also hamper your study so in that sense in some cases it might be also beneficial not to use such a pump system" (line 3861).

3)' Stephan first mentioned that he does not know much about the technique that I mentioned (weakening the cancer cells by depriving them from the medium in order to limit auto-contamination), then he admitted that it is possible to do it using the "you" pronoun ("you can do these things"). However, he immediately mentioned an example using the "we" pronoun, but is paradoxically not relevant, because it is not about depriving the cells from the medium to limit auto-contamination, but about injecting a virus and make sure it is going straightaway to the cells and is not diluted in the medium. Stephan concluded his account with the use of "you" again "so yes sometimes you use this deprival state". Here is the whole account: "I don't know oh yeah you can u:h do these things for example lots of bfff not not only with drugs but sometimes (.) we just take away the whole medium for example when we want to do virus infections (.) then we just have a very little amount just enough to cover the cells with a highly concentrated virus (.) solution: and then we just let them stand for like a short time before we replenish the medium with a bigger amount so they can make it for like ( ) so yeah sometimes sure you use this deprival state a little bit (.) just to concentrate things" (line 3874). Additionally, in the example he mentioned they do not deprive the cells from the medium, but they keep using "a little amount" and for a "short time" before they replenish a bigger quantity, which may be a matter of several seconds. Bottom line, Stephan wanted to just say "yes" to my question, but did not provide evidences that this technique is possible or that it is being used; especially that all the other participants were 'shocked' by the idea of depriving the cells from the medium' (see question 15). From this example, we can conclude that Stephan is oscillating between describing what they actually do, using the "we" where he mentions a non pertinent example, and the use of "you" where he mentions a hypothetical idea. Stephan's accounts coupled with the use of the pronoun "you" here and "we" there serve him to say that he was aware of my own ideas prior to the interview, which explains this awkward oscillation between the hypothetical and the real scenarios.

(4)’ Stephan trusts the provider and does not check after him, unless he discovers inconsistent results: “well it's it's possible when I start I investigate what is in it so I don't know by heart but at that moment I know what is in it and I know it's good or not and afterwards yeah you just have to assume that the manufacture is doing a good job and producing the right product (.) but it can happen indeed that u:h after a while you discover oh there must be a problem in the medium and then you'd have to back (.) it happens but then you'll have to repeat a big part of res of your research it's possible” (line 5690). Stephan’s account illustrates the idea that he checks for inconsistency of results caused by inconsistency of the medium composition. However, the question was rather about the risk that the medium contains molecules that are not supposed to be there. The problem with this scenario is it will give consistent (not inconsistent) results as long as the molecule is consistently in the medium. Hence, Stephan would not be able to discover this problem, because his checking would only happen, when he gets inconsistent results, otherwise he trusts the manufacturer and keeps his research going.

On the other end of the continuum stands Dalila. Dalila’s accounts were on the whole completely different from Stephan’s. Four main accounts and counter accounts were made by Dalila, which we will now refer to as (1 through 4) and (1’ through 4’) respectively:

Dalila First said that: (1) the constant exposition of the medium to the cells is not problematic and that it is doing its mimicking job in a rather satisfactory way, because it is just the in vitro stage, and because it is constant just like the blood is feeding the cells in a constant way. For her, we should not compare the medium to eating, but to how the blood is functioning: “u::hmmm pff (.) je pense que le fait de manger ça apporte des nutriments mais au final c'est quand même dans le sang d'une manière constante (0.12)” and “à part à part le glucose qui fait de l'hypo glycémie je pense que le reste y a des équilibres qui se font y a des trucs qui sont transformés des machins comme ça donc au final t'as (.) et puis ça reste aussi toujours de l'in vitro quoi il y a des artefacts (0.8) ” (line 2549).

(2) Dalila also said that the issue that I am raising is not important. She said “no” it is not important, because at the level of in vitro they are not supposed to be perfectly mimicking the real body, and that they take that into consideration and move on. She said that we do not ask this type of questions and that she herself never thought about it: “non (.) à partir du moment où on travaille sur des cult sur des cultures cellulaires on sait que il y a des choses qui:: (0.2) qui ne sont pas:: fin que c'est pas tip top avec ce qui se passe en vrai donc:: après on se fait: on se fait une raison là dessus et puis on avance quoi et puis on se pose pas ce genre de questions (.) avant je me suis jamais posé ce genre de question” (line 2916). She also said that the issue was trivialized, because it is not interesting: “non pas intérêt” (line 3041).

Moreover, Dalila said (3) that contamination and drug resistance cannot emanate from the medium; and even if it does, the drug’s objective is to kill the molecule: “Le but des médicaments c'est de tuer cette petite molécule qui provoque le cancer et donc si ces petites molécules sont tuées elles ne peuvent pas auto-contaminer ↑ elles ne peuvent pas contaminer ↑” then she repeated that as long as the cell itself is supposed to be killed by the drug (i.e. drugs are usually designed to kill cells not to treat them) there is no possibility for auto-contamination to happen: “Par contre si on a un médicament qui empêche cette molécule d'aller sur notre cellule et de la pas proliférer la cellule elle va mourir (.) donc elle peut pas être auto-conta ils ne peuvent pas c'est juste que les cellules elles meurent” (line 4391). After mentioning that drugs is to kill the molecule, Dalila ended up saying that cancer is not a molecule: “Mais pour moi le cancer c'est pas une molécule ((laughter)) c'est pour ça” (line 4379). Iinterestingly, in section 3.3.6.3 (on page 168), the participants (3 post post doctoral researchers and 1 PhD student) ended up saying that cancer can be a molecule. It was Stephan who first argued for this idea, and then Daniel, Brian and Gaelle, admitted that this is quite possible.

(4) Finally, Dalila said that (a) she does not trust the company from which they buy the readymade medium, because they may vary the composition, (b) that she does not know the details of the medium composition adding that only the manufacturers know the real composition, which is kept secret, and (c) that the readymade medium can

skew results as seriously as when we buy horse meat thinking it were beef. Following are the French equivalents of (a) through (c) that I summarized in the previous sentences: (a) “non (.) il ya des variations […] puisque c'est de la production donc d'office ils produisent jamais pareil (.) après u:h j’espère que ça soit eu:h assez reproductible mais euh je pense pas que ce soit 100% identique” (line 5502) ; (b) “ce qu'on rajoute dedans? ah non non tout fait je connais pas généralement c'est secret en plus (.) pour mais de mémoire on devait chercher les compositions des milieux et les 3/4 du temps tu sais pas puisque oui ok tu utilises ça ça ça et ça mais ils te disent pas tout parce que n'importe qui peut faire la même recette que et le vend moins cher” then “tu vas sur le site de la firme tu sors la composition de ton milieu comme ça tu l'as en tête mais u:h tu sais rien faire de plus (.) que croire ce qu'on te dit” (line 5563). Finally, (c) “il y a de toutes façons on a de toutes façons une base mais c'est comme ce qu'on achète dans le super marché on peut espérer que c'est bien du bœuf et pas autres choses (.) c'est la même chose” then “ah oui comme manger du cheval en pensant que c'est du bœuf et attraper quelque chose (.) c'est la même chose ” (line 5608).

We will now look at the counter accounts (1’ through 4’): (1’) While she said that in vitro is not supposed to be that close to the real body, because we anyway will go to in vivo and that in vitro any way has artifacts that we should take into consideration, when I reminded Dalila that if in vitro does not succeed, the research will not go to in vivo any way, she agreed and said yes: “Ah oui [si l’in vitro] n'aboutie pas? […] on ne passe pas (.) peut être qu'on manque des informations importantes (.) sais pas”. She finally agreed that the fact that the medium is not perfectly mimicking the human body would be skewing the research, saying “yes I think”: “Oui je pense”.

(2’) While Dalila had said that the issue that I am raising is not important and that it has been trivialized, because there is not interest in it, when I asked her to imagine a system that mimics the human body, referring to the new flow system, she then said yes the best practice anyway is to be closer to the human body: “pourquoi pas? Le but c'est de se rapprocher quand même un maximum de ce qui se passe dans le corps (.) tout en restant u::h il faut que ça reste quand même faisable et u::h” (line 3437). As we have noticed in the previous section, Dalila here again as a

technician, never spoke about what she never tried herself, before she is invited to think about it. Furthermore, when I first mentioned the new system, Dalila's way of speaking has changed noticeably, especially with the nervous interruptions, and unjustified laughing.

(3') Dalila had said that contamination and resistance have nothing to do with the medium, because cancer is not a molecule in the first place and so that contamination would happen due to an enduring molecule in the medium is a nonsensical statement. Now, Dalila will say that cancer is a molecule. She said 'yes' ("oui") three times to me and also by saying herself that the drug's aim is to kill the molecule that's causing cancer:"le but des médicaments c'est de tuer cette petite molécule qui provoque le cancer". She also said "yes the molecule affects the cells": "oui mais la molécule elle agit sur les cellules" and then again "donc le l::: le cancer est une molécule qui agit sur les cellules tumorales dans notre flasque et fait proliférer nos cellules" (line 4388). In fact Dalila, initially agreed on three replies that cancer is a molecule (although what I meant was that cancer is caused by a molecule), she then said that cancer cannot be a molecule, because it's a disorder in the cells. But once I told her that I read an article saying that cancer can be caused by a molecule, she then stepped back and considered cancer to be a molecule (see discussion starting from line 4317).

(4') With regard to Dalila's fourth statement, she did not make apparent contradiction, but the inconsistency is with regard to her initial satisfaction with the medium as it is used now. She had said in (1) and (2) that the medium is not problematic, that the issue is not important and that it has been trivialized, because it is not worth stopping at. She also said that she is happy with the in vitro stage that it is not supposed to be perfectly mimicking the human body. Now after I invited Dalila to think specifically about the quality of the provider's medium, she then accounted for the fact that she does not really trust that the product is as it should be. She mentioned that at this level we can only hope that it is fine, and that there is no way to do otherwise or to check, because the product is a secret. Thus, there is actually no account 4', it is account 4 that is completely inconsistent with accounts 1 and 2. These

inconsistencies would not be possible to spot without a detailed qualitative discourse analysis that is based on an open interview, which has itself to be based on acute questions by the interviewer.

As a matter of fact, all the participants without exception have never accounted for their mistrust with regard to the medium that they buy readymade. With varying degrees, they all (except Dalila) said that they trust the providers. Paradoxically, all of them said that if the provider would intentionally or unintentionally add an unwanted molecule in the medium, it would significantly skew the experiments. None of the participants (including Dalila) mentioned any doubt with regard to the readymade medium. After I gave each of them significant room to account for whatever they want with regard to the medium usage and the risk of skews, I had to drive them step by step to this very precise scenario, as encapsulated in this condition: "Are there risks that if the medium components are not exactly as they are described, they would make the cells develop resistance to drugs, or different protein expression?" (question 21). Only when the participants were faced by this question did they account for the magnitude of this skew risk. At this level, they all agree that the problem is huge, but they also agree that this problem is not likely to happen, because they trust the providers.

I will now cite the different participants' accounts with regard to: (1) the fact that they trust that the providers would not add a molecule or do their job in any way mistakenly; (2) the fact that if an additional molecule would be injected in the medium, there is a considerable risk for research. I will also cite (3) excerpts where participants accounted for why they trust providers. As a matter of fact, they only mentioned that the provider's medium is consistent (for example not giving bizarre results); whereas my raised issue is clearly not about consistency here, because if the provider would add an unwanted molecule all the time, it will all the time skew the experiments in a consistent way. The conclusion that I draw from these accounts is that the participants' opinions, in this regard, are not scientific, but rather based on trust.

Paul first said: (1) “I trust them yes (.)”, but when I asked him about if such a mistake happens, he said (2) “well it can lead to misinterpretation for sure” then “you could think that what you observe is due to the kind of tumor cells you use or the treatment you have added on the cells but it could be really due to something else which could be the lack of one amino acids because the company u:h would which offer which did produce the medium forget one step in the process but we cannot control everything clearly” (line 5932). When I asked Paul how they can rely on trust, he said: (3) “there is no other way to do (.) sometimes (.) I would say in my career may be I encountered may be 2 episodes of bizarre results that we tried to understand u::h and we identify some some aspects u:h being related to u:h the medium or the serum we used as being the the likely even for explaining the difference we observed (.) so I would say in a 25 years I've b b I have been for 20 years I've been facing a problem of such a problem because something not being as it was before (.) that's the only way I could identify if there is something occurring” (line 5461). Clearly, Paul’s latter account is about inconsistent medium compositions. Only when I insisted that my question was not about inconsistent medium, but about a medium in which there is consistently an unwanted molecule that other labs too are using, because the providers are providing to labs everywhere in the world, Paul then admitted that: “if everybody is making the same mistake then you may certainly misinterpret the data and if you work on the drug development you can find a drug which is active artificially but which not which will not relieve patients because your model is too far from the reality ah if you push that at the extreme you could think about that” (line 5943). I deduced that Paul considers that the idea that providers are in a way or another constantly adding an unwanted molecule to the medium that they sell to many labs in the whole world, is an extreme case. Therefore, Paul’s position is not scientifically based, but rather a common sense type of opinion.

Brian said: (1) “yes (.) I trust”. When I asked him about the risk, he said: (2) “yes if the components are not the same (.) it can change a lot (.) a lot”. But when I told him that I am surprised that their own research relies on trusting external companies that are profit driven, he said using the “you” pronoun (3) “you trust them because you know it's better (.) with experience you know that the normally the product from the the companies work well (.) work better than what's you make for example homemade (.) sometimes (.) well sometimes and most of the time

because it's much more control (.) the quality control in this company is very tight" (line 5723). As a matter of fact, Brian did not answer my question, but confirmed that the trust has no scientific basis. It was only based on that they prefer to think that the providers are serious and reliable. Brian also said that they may check the medium in the lab, but again if they do they would check for consistency (that the components are consistently with the same amounts), not for an unknown molecule. Additionally, Brian resorted to the use of the "you" pronoun, in a rather defensive way, when I said that I am surprised that their research relies on trust.

Dilara first said in English (1) "I hope so ((laughter))" (line 5249), then she said in French that she is confident that the provider is giving her what she wants at least something within the acceptable range of error "donc je pense que le milieu que je reçois est très proche de ce que j'ai commandé (.) du moins ça se trouve dans les marges d'erreurs" (line 5256). When I asked Dilara, if the mistake happens, what the consequences would be, she said (2) that would certainly change the results "ça change oui pour euh les résultats certainement (.) oui (.)" (line 5260). Like Paul and Cédric, Dilara first added that anyway if any inconsistency is noticed, they can contact the provider and check the medium's requirement specifications "maintenant si quelque chose (.) il y a aussi soit ( ) avec soit c'est sur demande (.) mais on peut voir accès justement à ces euh à ce cahier de charges". When I asked Dilara if this check after the provider happens, she said generally speaking no and again that she does not think so and no again "On général non je pense pas (.) non". When I expressed my surprise of how they do not check after the providers, she said in the academic field they do not check, but the pharmaceutical firms do. Dilara added that it would be interesting to check, but that they do not have time and the ability to do it: "Je pense que ce serait intéressant (.) maintenant euh dans la vie on peut pas tout faire (.) et on peut pas le faire parce qu'on n'a pas les moyens et parce qu'on a pas le temps (.)" (line 5284).

To conclude this section, I wish to state the following: it was through the very precise questions that I asked, and after detailing the inconsistencies made by individuals that it was possible to ensure the following: (a) this lab (and most research labs, according to our participants) does not know the exact composition of the medium, which

they constantly use in the cell culture. (b) If the medium that they purchase readymade would contain an unwanted molecule; it would considerably skew the research. Furthermore, (c) if the providers, which sell to many labs in the world (and sometimes monopolize the international market), are permanently making this mistake, the skewing consequences would affect the experiments of all the labs that use their medium, worldwide. Finally, (d) I also found that the participants avoid accounting for their research weaknesses unless they are faced with very precise questions (see question 16 and 21 in particular). Such defensive practices played a role in obscuring the (de)trivialization practices (aka attitudes), and made the dynamic hard to demystify.

3.3.6.3 Diachronic inconsistencies and validation (case 2)

As I did for case 1, I conducted a collective interview for case 2 for the same reasons: a) to validate a number of conclusions that I have so far made in my previous analysis regarding the individual positions taken during the individual interviews; b) to explore the evolution or change of positions over time, regarding the different issues related to the usage of the medium and its skew potential; and finally c) to compare the trivializing accounts over time. The transcription of the collective interview is in appendix C2. The collective interview went through all the questions that I asked in the individual interviews (listed on page 137), but in a much less structured way, because the discussions were more spontaneous and the participants have anticipated many questions.

(1) About the condition of the medium being constantly on the cells

I first asked why the cells are constantly exposed to the medium, unlike the human body, which eats and drinks from time to time. Brian was the first to respond by saying "it's the same" suggesting that the answer is the same as the answer related to the co2 (of the previous chapter). He added "you need to find like a condition to work" and "and they work well with this uh (.) this like a stable condition" (line 5993); which suggests that this system is not problematic. In the same sentence Brian, who knows that I know about an alternative system, added: "and you can develop of course like u::h (.) a sponge or there are already **[**". The use of "and" and "you" (instead of "but" and "we") together suggests that this alternative system is something peripheral that "you" can use, but that Brian does not need. Daniel interrupted Brian to say that "**[**there are some labs developing kind of pump (.) actually the: the medium is u:h in a cycle" (line 5996), suggesting that he is also aware of this system, but they prefer the current one. Then, they immediately started to criticize the pump system, mainly because it is "expensive" and not "practical" (lines 6010 to 6019). Interestingly, after criticizing the system, Brian finished his account by saying: "but we agree maybe it's better with uh pumping your medium ((laughter))". The participants' reactions seem to be rather defensive. When a researcher wants to advance knowledge, he or she would take every opportunity to discuss alternatives in a rather welcoming and receptive way; and his or her own in a rather controversial way (i.e. by playing the devil advocate vis-à-vis his own method). The participants actually did just the opposite. Additionally, Brian

concluded that “it’s better” to use the pumping system, which means that regardless of the negative aspects that they mentioned about it, the new system is still better. This contradiction between criticizing a system and ending one’s account by saying that it is “better” reinforces the defensive position that at least Brian took. Perhaps Brian wanted to achieve one goal: that he is aware that this new system is better, but not using it is not his fault, because it is up to “others” to solve the issue of practicality and expensiveness first. Furthermore, the use of the term “we agree” (that it is better) suggests that Brian thinks that I (the researcher and interviewer) am favorable to this new system, although I did not say anything to express my own opinion, and neither am I entitled to do so. This is perhaps another element illustrating the reflexive feature of talk (Garfinkel, 1974; Wielder 1974), which I introduced on page 16. Brian is perhaps reflecting on my own thoughts and not only on the topic; i.e. he presumed that I preferred for the new system and so “agreed” with me that it was better.

Now that the participants did not discuss any negative aspect with regard to current system, I wanted to understand the issue that I raised in the previous section, with regard to the nutrients constancy. I asked if the human body’s cells consume nutrients in a constant way or from time to time; Gaelle said “all the time” (line 6034), because the blood is always feeding the cells and not only when we eat and drink. Then Daniel added that not only the blood feeds cells, but also there are some cells that produce what other cells consume: “[I think it's not only in the blood it's also: the cell contact or communication”. He added: “no it's actually it's some cells are producing:: a compound and other cells can use this compound to:: to use it as ( ) fuel (.) there is a communication between the cells” (line 6028). Gaelle explained this process as taking from the reserve “fat”: “they (.) the person they will take from fat” (line 6036). Now, after they finished detailing how the mechanism in the body is complicated and how cells are fed in a constant way, I asked about the system that they are using in the lab and which is supposed to mimic the human body. I started by asking: “so both the cells that you work on and in the body (.) they consume all the time?” (line 6049). They all said “yeah” and “uhm” altogether. Gaelle then confirmed that the cells of the lab are able to do a similar job as in the body, as they can consume from the medium in a constant way: “because there are transporters sensors so they can: take the nutrients” (line 6057). Brian added that the lab cells are also able to produce what the

cells in the body produce, using thee medium: “and the ones that there are not in the medium they can produce themselves so (.) with the things that are in the medium (.) this is hhh ((laughter))” (line 6059). Consequently, there seems to be no problem with this system, so far.

Although I have discussed with Gaelle in the individual interview that the concentration of nutrients can be problematic, she so far did not mention any negative opinion about it. So, I put the question directly: “ok but now with regard to concentrations in the medium now (.) which is more constant? is it the body that is more constant in the way that it feeds the cells or the medium is more constant? and do we need to have these constant levels?” (line 6061). Gaelle was the first one to respond, by saying that cells in the human body take orders from the brain. But when Stephan stated clearly that in the body the process is more constant and steady (line 6083), she said: “yes (.) that's why we change also the medium like every two two […] three days” (line 6085), hence again suggesting that changing the medium makes the usage of the medium satisfactory and again there is no problem with it. At this level, I am left with no doubt that the participants are satisfied with the use of their current system, or at least that is what they wanted to say to me. Especially, compared to their critical accounts during the individual interviews, the participants (so far) only showed a positive position with regard to the current system.

I put the question more straightforwardly by saying “if this way of exposing the medium to the cells is satisfactory (.) why trying (.) the flow system makes the medium flow like (.) then be permanently there? Why some people want to try this? This flow system instead of just exposing [”. My question triggered accounts that were omitted since the beginning of the interview. Gaelle interrupted me and admitted that this new system is closer to what happens in the human body, which is always the aim of in vitro experiments: “**[**to be more close than to physiological u:h yeah stuff”. Brian agreed: “exactly”. Daniel added that the system allows glucose to be moving and not constantly exposed to the cells, probably because glucose consumption was found to be related to cancer growth: “it's kind uh small dynamic system it's not only glucose is always in the flask (.) glucose is circulating and u:h so cells can get access sometimes**[**”. Gaelle mentioned that this dynamic system resembles the blood vessels (which is

not the case in the current system): “it's kind uh small dynamic system it's not only glucose is always in the flask (.) glucose is circulating and u:h so cells can get access sometimes **[**”. Stephan will now make it clear that without this dynamic way of exposing cells to the medium, cells may change of behavior (which causes them to study a mechanism that is skewed due to the current system): “maybe the cells can produce something that is inhibiting their own growth and if you don't remove it they will change their behavior” (lines 6097 to 6102). After all these accounts, I expressed my surprise with regard to how they can say all that although they had already said that they are satisfied with the current system. Stephan responded by saying “for us yeah” and then “that's our choice (.) but there are people they have other cells with other requirements and may be ah if they grow in a bottle it won't work (.) they will to (.) have this flow system” (line 6107). Stephan is now stating that only some cells and not the ones on which they work, need to be cultured in a way that is similar to the human body. Basically, he is implying that the cells on which they work do not need to be cultured in a way that is close to the human body, although they have always said that getting as closer as possible to the human body is their major aim, to be able to study “real” things (lines: 108, 766, 1490, 1497, 2530, 2738, 3081, 3410, 3420, 6107 etc.). We do not need to be specialized in biology to understand that in order to study the human cancer cells in a lab, you need to have cells that are cultured in a way that enables them to be the closest possible to in vivo (i.e. to how they are in the human body). Gaelle went to the completely opposite direction, by mentioning that it is even better to keep the medium not changing, in order to observe how cells produce things that need to stay there (in the medium) and allow them to grow: “but some time it's the opposite (.) the cells are producing something that are needed for the growth **[**” (line 6114). This is clearly a very specific type of experiment that should not be prioritized over the most important aim: to be close to the human body. Clearly, the participants are playing up an idea at the expense of a major aim, which they are implicitly playing down or trivializing (getting closer to in vivo conditions). Now Brian, who already said (in the second paragraph of this subsection) that “we agree that’s maybe better with this pumping system”, will now say that: “Yeah I don't think the pumping will be better than the other (.) you'll have also limitations” (line 6120).

Because Brian mentioned that limitations are true for both systems, I asked about the limitation of the current system: “can the permanent exposure to the medium cause a negative outcome to the research? (.) The way that you're doing right now?” (line 6153). Daniel responded by saying “Yeah we could miss u:h adaptive process uh the cells need to use in terms of (.) if they lack u::h the substrate or something they will adapt to find another way to proliferate (.) and because we're changing the medium every two three days (.) they've full access to: to the substrate or (.) they will not need to adapt to the lack of u:h **[**” (line 6155). Daniel actually mentioned a serious problem of this system; he basically said that due to this system, cells will change of behavior and would be different from how cells behave in the human body. Yet, Daniel prefers to call this condition “a limitation”, not a skew: “**[**= Yeah it's a limitation of in vitro conditions” (line 6160). Perhaps the word limitation in research is safer to use, because the research community would accept that any research has a limitation.

(2) The possibility that the medium contains molecules that no one would know

I will now orient the discussion into a specific skewing condition of this system, which we thoroughly discussed in the individual interviews (question 16, page 137). Question 16 will provoke a disagreement between the participants and will unveil clear contradictions between what was said in the individual interviews and the collective one. Daniel who was the only one who clearly said in the individual interview that cancer is not and cannot be caused by a molecule and that auto-contamination to be caused by the current usage of the medium is not possible (page 146), will now admit this possibility after discussions with Stephan. It is through this discussion that the participants will agree that the current usage of the medium can cause auto-contamination, which is a major skew risk.

I wanted to start the discussion by getting the participants to confirm or reverse a specific point: that cancer, which is the disorder of cell proliferation, can be caused by a molecule and this molecule can be always present and does not necessarily leave the cell. This discussion started from line 6173 up to line 6245 and ended where they all confirmed this possibility. Daniel said: “so so will see only the damage (.) but probably yeah the cause is still still there**[**” (line 6228). Stephan said: “could be” (line 6236) and again: “→ **[**= your cell producing its own molecule that

is causing the cancer (.) could be (.) and it's always present indeed". When I arrived at these confirmations, I addressed the question to Daniel specifically, by asking if he agrees that cancer can be caused by a molecule; and he said: "Yeah because it's already described by (.) some molecules we know". At this level, I told him that what he is saying now is not what I have understood from him in the individual interviews: "because may be I misinterpreted your what you said before u::h that it (.) I thought you told me that it's never possible (.) I thought". Daniel interrupted me and said: "no no no". I told him: "cells can be (.) become cancerous because they are exposed to some molecules?"; he said: "yeah for sure", although in the individual interview (question 16, lines 3961 to 4028) he clearly refuted this idea of cancer being caused by a molecule or that the cancerogenic molecule may remain in the cells.

Now that everyone agrees that cancer can be caused by a molecule and that the molecule that is causing cancer can still be there and does not necessarily leave the cells, I asked if this molecule would stay in the medium and auto-contaminate the cells, continuously (line 6262). Stephan was the first one to confirm that the molecule remains in the medium: "Yes still there yeah yeah it's still be there yeah" (line 6266); then Brian supported this claim: "Yeah it's possible (0.3) because they they produce (.) there are some cells that produce these molecules to keep on proliferating" (line 6267). I said: "so they themselves reproduce the molecule?" and they all said "yes". I wanted to make totally sure that what they said means that the usage of the medium can be causing auto-contamination, so I said: "wouldn't that molecule stay in the medium get multiplied there and contaminate the others that may be cured when you test your drugs? For example this auto-contamination or resistance?". Stephan admitted this: "yeah sure" (line 6285); Brian said: "yes". I directed the question to Daniel in particular and he said: "yeah yeah". Then I asked Gaelle and she said: "yeah". Because I did not believe what I heard, I said: "are you sure?". Gaelle confirmed that she understood my question and said: "that auto-contamination is possible"; Stephan interrupted her and reinforced the idea by giving a concrete proof that this is possible: "you can take you can take the medium of the cells of the culture without any of (.) uh with cancer without any cells left put in in another culture (.) there is a possibility that you induce cancer" (line 6285). I told them that I deduce that cancer is not just the disorder

of proliferation, as they always prefer to say: “so from this we deduce that there is something biological physical that is contaminating the cells”; Stephan said: “could be” and Brian said “it’s already described” (line 6292). Once they admitted this, I told them that except Stephan, none of them said that this is possible, before: “because (.) to be honest with you (.) most of you said no except Stephan (.) the only one who said yes the other time (.) that this is possible was Stephan”. Once I said that, they started again to discuss arguing that the contamination is caused by another molecule not by an initial one. I told them that I do not understand this confusion, since I never named any molecule or never said a first or a second one to be causing cancer: “I'm not talking about a particular molecule (.) did I name a molecule? I did not (.) but there is a molecule that is there and that is constantly” (line 6325). Bottom line, it took me all this time to come to an agreement that the medium can be retaining the molecule that is causing cancer and making constant contamination. Interestingly, when the participants were faced by this contradiction, they told me that the problem was in the question and that I had to be precise whether I meant the first or another molecule, whereas I never mentioned any molecule and never went into those details. My question was clearly only about contamination through any molecule that would stay in the medium.

As I did in question 20 of the individual interviews, I asked the participants here too if they trust the providers from which they buy the medium. They all responded firmly that they do (lines from 6345 to 6354). When Gaelle started to put the reason why they trust them, she said “we have quality control” using the “we” pronoun, although the control is not theirs. Brian corrected her and said: “they have quality control that we don’t have”. I asked again “who does the quality control?” Brian replied “companies”, so I told him that these companies are “the judge and the judged”. Gaelle interrupted me not to respond to my concern, which is clearly about the fact that these providers being profit driven would not be as objective as the lab wants them to be, but to say that “there are also consulting stuff”, which is too ambiguous. Daniel immediately interrupted her and said: “no no”. Gaelle again interrupted Daniel to say that “= and they are also controlled by other like federation MDA and so on” (line 6350). Basically, Gaelle insisted on giving answers that illustrate that her trust is not scientifically based. She rather trusts that others are doing their job properly. Daniel started to understand the concern, and said: “otherwise it will be too easy to

cheat" (line 6351). Then Gaelle made an account that shows that she wants to argue that it is logical to trust and illogical not to trust. It is basically a rhetorical question that she directed to me: "and why what is the interest of the company to:" (line 6353). Gaelle's question supposes that I had told her that I do not trust these providers, or that trusting them is not a good idea, whereas I never mentioned my opinion.

Because I noticed that there is a gap between what I meant by "trust" and how they seemed to understand it, I asked them to tell me how they understand trust with regard to the purchased medium: "when I say do you trust (.) what do you think about actually? You trust what in particular? What is the thing that you want to make sure that it's it's the thing (.) that's well done? (0.2) When I say Trust? You trust on what in particular?" (line 6356). After my question, Brian took the floor and put the stress on that the providers are better than the lab itself, because a) they have better equipments, b) they are precise, and c) they have more control. I obviously understand Brian's point, but I was also thinking about trusting that the providers are not cheating for example. Brian did not content himself by stating these points once, but repeated that he prefers to buy the medium from the provider than to make it in the lab. I told them that I understand his point of view and asked what else is important, but none of them wanted to go in the direction of doubting the quality of the purchased medium. At this level, I stated it boldly, by saying: "but suppose that the provider is altering the medium composition for some commercial purposes like for example to make your cells (.) addicted to their medium" (line 6378). They all attacked this idea by saying that it is not possible to happen, because they buy from different companies. I told them that these different companies may purchase the raw material from one company, and if this raw material is affected, it would affect all the companies' products: "So the different providers don't they themselves buy from the same provider at least the raw materials u::h (.) like the basic" (line 6417). When I mentioned that, Stephan admitted this possibility: "it could be yes", Daniel said: "probably yes", Brian said: "yes sure"; and Gaelle stepped back and did not say anything. When Daniel started to think seriously about this possibility, he said: "maybe at the end it would have an effect if they're change the composition (.) I will not trust them anymore" (line 6425), I told him: "But you don't know I mean something that you would not know of" (line 6428). At this level, Gaelle interrupted again to bring the discussion to and aspect that is not under the lab's control:

“but they are obliged to say” (line 6429). I started to make sure that the lab is not able to check what the provider can do with the medium. Stephan responded to Gaelle’s idea, by saying “yeah they are obliged (.) otherwise it's a criminal” (line 6430). I was expecting to hear an account that states that they are able to check, but Daniel was talking as to be thinking out loud and said: “If they're lying about the composition [”; then Brian went back to the quality control idea that is again beyond the lab’s control: “in quality control you have also the: they check” (line 6433).

It seems that my idea pushed them to think in a different direction. Stephan, mentioned that if the providers are cheating, then may be one day one lab would notice a strange thing (that he did not mention) and would come back to them to ask for clarification. I reminded Stephan that I did not mean that the provider would change the medium from time to time, but to be constantly selling an altered medium. Gaelle again interfered to say: “but they cannot”, I asked her “why”, she said “because they are approved for example before (.) by u:h if it's pharmaceutical company (.) they're proved by FDA” (line 6448). She again added: “= they're they have agreement and u::h”. Stephan was thinking about the severity of the issue and said: “it depends if it's a problem in (.) if they put glucose yeah your cells are kind of addicted to glucose (.) may be indeed yeah (.) but it's” (line 6453). Brian made a general account stating: “and if you check the composition in the medium (.) it will be different from the one they state (.) you will say to them it's not right” (line 6455); but I told him again that he can check if he knows what they added and not check what he does not know. Daniel admitted: “you can't check everything” (line 6459). They discussed a little bit about what they can do to detect anything that is added to the medium, but ultimately decided that the provider can cheat and that they would not be able to know. Brian said: “but of course it's possible to put something”. Stephan added “everything is possible”. Brian stated again “it’s possible to put something that is not detectable like doping” and Daniel agreed: “exactly”.

After all those discussions, Gaelle interfered to say that it is not possible and Stephan started to step back by mentioning “but you're testing also your raw materials before you put them in your” (line 6493). I was surprised to

hear Stephan talk about testing their raw materials, because I know that they do not; so I asked him: “They (.) you don't test”. He admitted that they do not test: “No but it it it hhh” and did not complete his sentence. Brian took the floor to say: “but we’re talking about cheating” (line 6500); but Stephan went back to his initial idea, which is that if the medium varies you would discover the variation during the experiment: “You get variation and it could come up” (line 6499). I had already answered this when I said that I did not mean variation, but a constant and consistent alteration of the medium. After they failed to provide a scientific answer on why they trust that the medium is correct and is not skewing their experiments, Brian diverted the discussion to elsewhere, by saying: “but this is could be in a company could be a politician could be your colleague” (line 6502). Now that they made sure that I was sure that they have no scientific answer, they will make defensive accounts, which will serve them to say that the problem is not theirs, that it is global and applicable to others as well. Daniel started by mentioning that “there is no way to control cheating”; so the problem is not his, because the “way to control cheating” is simply not existent. Gaelle mentioned an example of cheating in another domain: “it's like ((she mentioned an automobile maker)) and the:::”. Brian again made the issue a universal problem not only theirs: “it's a global truth yes there is not something”. Now after they were sure that I heard the idea that this issue is global and everywhere else, they reminded me that they need to trust and that they have no more options. Daniel started: “you need to trust them”; and Brian supported him: “exactly otherwise you won't trust any” (line 6510). This time too, they both preferred to use “you” instead of “we”, because “you” puts me in their shoes as well, and it helps them to say in another way the following idea: “don’t tell us that you too wouldn’t do the same thing in our place, because we will not believe you”.

The last point that I wanted to compare with the individual interviews is to what extent it can be serious if the medium would turn out to be really altered by the providers. I restated the question 21 of the individual interviews (page 137). In this regard, the answers came very short, but almost identical to what they basically said in the previous interviews: Stephan said: “anything” can happen. Daniel supported him by saying: “yeah exact”. Brian in his turn said: “we don't know but yeah (.) probably”; and Gaelle admitted what was said by simply saying: “uhm”. These accounts are also consistent with Paul’s response (during the case 3 individual interview) when he said: “Yeah

there there there you are (.) you are blinded (.) you are acting as uh as uh being just prone to believe that uh what you are using is is uh doing what you what it is described to do" (line 7090).

I will now close this section with the following 5 conclusions that I drew from discussions with the participants: 1) the current medium usage can skew results, because it does not mimic the human body (i.e. the in vitro cells can change of behavior compared to in vivo, due to the medium usage); 2) the medium can cause auto-contamination, due to its stagnancy; 3) the lab members do not ensure that the medium is identical to how it is described by the providers and do not know the exact composition of the medium they purchase, although they insisted that every little change can have consequences on the research results; 4) if the medium is systematically altered by the providers (i.e. cheating to make cells addicted to a specific medium) the lab would not be able to know; and finally 5) I detailed through the discussions that the participants developed defensive positions regarding their own practices. They additionally avoided contradicting each other, even at the expense of contradicting one's self. To do so, they trivialized and detrivialized the same idea, depending on how I asked questions.

3.3.7 Exploring the (de)trivialization dynamic (case 2)

In the same way as I have proceeded in chapter 1, in this section, I will zoom in on the (de)trivialization dynamic in the case of the medium usage, based on the 3 previous subsections. Following are the questions, which I will address in the subsequent paragraphs: (a) what is (de)trivialized regarding the medium usage? (b) Who is (de)trivializing it? (c) Why is this (de)trivialization occurring? (d) How serious are the consequences of this trivialization? When and where does it occur? How is it practiced or accounted for? As a matter of fact, the (a) through (d) questions happen to be inextricably related and can hardly be dissociated, but we will nonetheless discuss each question in a separate subsection, for the convenience of dismantling the dynamic into distinguishable elements. These elements are summarized in table 2, on page 196.

(a) What is (de)trivialized regarding the medium usage?

With regard to the most basic question: "*what is (de)trivialized regarding the medium usage?"*, the shortest answer is the 3 conditions that can potentially skew the research results. They are namely: 1) the fact that the medium is constantly on the cells and not flowing: a condition that is not similar to what is happening in the human body; 2) the risk that the constant medium can cause auto-contamination; and 3) the risk that the medium may contain unknown molecules, which can skew in vitro experiments. In this section, I will aim at providing evidences on 2 opposite elements: the first element, which will be detailed through a number of accounts, is that the 3 mentioned conditions related to the usage of the medium have been trivialized, prior to my raising of this question, but also during the interviews. The second element, which will also be detailed through a number of accounts, is that the 3 mentioned conditions can have negative implications on cancer research and that they are therefore important to reconsider.

Medium stagnation vs. flowing

To start with the skewing condition number 1 (the fact that the medium is constantly on the cells and not flowing: a condition that is not similar to what is happening in the human body), the lab members have directly answered my question number 11 "do you think that this issue has been trivialized?" with varying degrees of

affirmation. In the following excerpts, I will illustrate how the lab members accounted for the fact that the medium constancy is trivialized. When he was referring to others, or using the personal pronoun “you”, Stephan said: “u::h may be it's trivialized in the sense that it's very easily to be ( ) too much and we have tricks and buffers that can u::h (.) you know compensate for the medium problems and if you once in a while replace it (.) it's a big enough substitution (.) so I guess it's somewhat triviali::zed” (line 2941). But Stephan continued to trivialize the issue during the interview, although he had said that the issue is important “but also to the other extent I think in most cases in practice (.) I think that the:: effect might be marginal that we would gain by using such a system” (referring to the flow system as an alternative). Stephan explained the trivialization saying: “because n (.) there is (.) it's easier to: do it in this similar way (.) it's more (.) it's cheaper it's faster it's (.) yeah it's u:h (0.3) it's the falsely cheaper ((laughter))” (line 2949).

Brian said: “Yes I would say some labs that (.) here is the lab is specialized in metabolism (.) so it's quite involved in the: how the nutrients are used by the cells (.) so: we take care of this u::h issue (.) so we change the medium every two three days to keep always the same levels of nutrients (.) but I would say labs working more in: (.) more distant from metabolism (.) it could be an issue (.) you don't take care of this u:h problem” (line 2954). Just like Stephan, Brian admitted the trivialization, but to be occurring elsewhere not in their lab. Brian explained this trivialization in terms of how important it is to rethink the current system, because the fact that the same medium is constantly there and that it gets consumed but the cells, it will sooner become unable to provide the cells with constant concentrations of the nutrients they need. Comparing to what happens in the human body, Brian said that it “would change the activity of proteins (.) activity and expression and whatever (.) it would change things for sure in the cells (.) so it's important to work always in the (.) more constant [concentrations]” (line 2965). Daniel said that it is clearly trivialized by many scientists and when he did his doctoral research, he too used to trivialize this issue, unlike now in the lab, because they specialize in cancer metabolism and therefore this issue is of great importance to them: “u:h je dirais un peu négligée (.) uh par une grande partie de: des scientifiques j dirais un petit peu moins dans (.) les gens qui travaillent sur le métabolisme puisque ça reste à uh fin le les nutriments les choses comme c'est

clairement notre uh notre domaine donc on y pense un petit peu plus (.) mais clairement fin par exemple si je compare à ce que je faisais pendant ma thèse (.) je réfléchissais pas trop à ce qu'il y a avait dans mon milieu la concentration en glucose ou en glutamine (.) pour moi c'était des conditions standards qu'on m'avait donné (.) mais j'avais pas en tête uh:: ben l'impact que ça avait sur uh sur les conditions des cellules (.) donc je pense que inconsciemment il y a quand même un: oui une partie: de trivialité: sur ces conditions de: culture et des milieux" (line 2972). Once again, Daniel said that this issue is trivialized by others but not by this lab. Dilara said exactly the opposite as Daniel. While Daniel said that this lab is in particular paying much attention to the medium, because it is part of their specialization (cancer metabolism), Dilara said that this issue is important for other labs, but because this lab is focusing on other things so, it is fine to trivialize this issue: "Je pense que tout dépend du laboratoire je crois qu'ici la question est n'est abordée parce qu'on on se focalise vraiment sur des aspects très très précis euh de la cellule tu vois avec on va vraiment très très très loin dans les processus de biologie de biochimie (.) donc on va étudier l'enzyme alors que si dans des études tu vois on étudie le passage nutrIment ou alors l'exposition d'un polluant ce genre de chose là je pense que c'est plus qu'important de se poser la question (.) mais ici finalement on étudie pas au niveau de la cellule on est bien plus profond" (line 2986). Perhaps, the total contradiction between Daniel's and Dilara's accounts, is the best illustration to how the issue is said to be very important by the former, but understood to be marginal by the latter.

Gaelle did not say that this issue has been trivialized, but that she "never heard this question before". She later said that this issue is important "yes (.) important not important but it's uh yes it's important as well". While her former account indirectly illustrates how the issue was trivialized in her own (as she never heard people discussing it), the latter illustrates (through the initial irresolution) how she was trying to figure out an answer and to make an account, simultaneously. Gaelle later said "it's important to understand what the cells are uhm have as nutrients (.) to be sure it's the same it's the right condition (.) to not study somethin:: with wrong basis" (line 2875). Gaelle was more precise on what is particularly important with regard to this issue, saying that "no for me it's more the::: the concentration of each stock [of nutrients] not really the fact that it's exposed all the time or not" (line 2880). But, as

we have discussed in the first section of this chapter, Paul, Brian, Daniel and Stephan, have explained that the fact that the medium stagnation (and not changing more frequently and in a more dynamic way) is the condition that causes the nutrients concentrations' variation. Consequently, I consider that Gaelle indirectly accounted for the trivialization of this issue on an equal footing with Paul, Brian, Daniel and Stephan. Cédric was clear about the fact that this issue is trivialized, mentioning specific cases where you need a flow system instead of a constant one: "yeah yeah yes because you have some specificity when you need flow like to (.) to test for u:h (.) a lymphocyte and endothelial cell work condition (.) if you don't have the flow they will just grow on the cells and that's all (.) but if you put some flow they will start rolling on the cells may be stopping so you need the flow sometime" (line 3028). In a more firm way, Paul said: "yes (.) clearly (.) people do not (.) they underestimate this point" (line 3035). Paul's account was similar to Brian's, Stephan's and Daniel's and certainly opposite to Dilara's in his attribution of the trivialization practice to "people". Contrary to Paul, Dalila admitted that this issue is trivial and that she is trivializing it, because it is not interesting "non pas intérêt". Finally, Joanne accounted for the fact that this issue was trivialized, by saying "yes" and relating it to the fact that they are so advanced in their field and so focused on other issues: "uhm well because you're always learning more complicated things (.) you will always try to understand the complicated things (.) and we always try to learn (.) and to get new things and you will you will not think about the: the basic (.) ↓ things wha: what you are doing here in the lab" (line 3053). Once again, Joanne preferred to use the "you" pronoun while she was justifying the trivialization.

Auto-contamination

With regard to the skewing condition number 2 (the risk that the constant medium can cause auto-contamination), which I introduced in the second paragraph of this subsection, I will now classify the participants into 3 categories. In category 1, Stephan stands alone, because he straightaway admitted that auto-contamination is quite possible, which means that the current usage of the medium is trivializing such consequences (that of the auto-contamination). In category 2, I grouped all the other participants, except Paul and Daniel. These participants admitted after discussion that this auto-contamination is possible. In category 3, Paul and Daniel, have rejected the idea of auto-contamination, considering it to be unconceivable. For Paul, the reason is that cancer is a cell disorder and not a matter of a molecule that is constantly there. More precisely, for Paul, when a molecule causes cancer, it does not stay there; it goes away. For Daniel (alone), cancer cannot even be caused by a molecule. Therefore, based on the contradiction between Paul's and Daniel's accounts on one hand and also on the contradiction between Paul and Daniel's position as opposed to all the other lab members (participants from category 1 and 2 who said that contamination is possible), I can conclude that Paul and Daniel continued to trivialize this issue even during the interview session, perhaps unknowingly or due to a misunderstanding. Further investigation of this issue was possible through the collective and diachronic interview, where I was able to confront the opposite accounts. Ultimately, it turned out that Stephan was correct, from their own perspective; as he convinced his colleagues, who stepped back and tried to reframe their initial mistake (see diachronic discussion related to this idea on page 169). Consequently, using the diachronic discussion, I can now argue that in the synchronic interview this auto-contaminaton idea was clearly trivialized by everyone, except Stephan (especially Paul and Daniel who never stepped back).

Unknown molecules?

With regard to the skewing condition number 3 (the risk that the purchased medium may contain unknown molecules, which can skew the in vitro experiments), none of the participants, except Dalila, has first accounted on this risk on their own. Then all of them happened to account for the negative consequences of such a condition, but only when I put the question straightforwardly. I will establish this trivialization case, based on two facts: fact (1) that

initially the participants (except Dalila) said that they trust that the provider is giving them the medium they want and that this trust is not scientifically based, but rather an opinion (question 20). Fact (2) that they all said that if the medium would contain an additional or unknown molecule, then the medium can skew their research (question 21). Through these two facts I will argue that the participants are trivializing a matter that they said to be of considerable importance. In other words, the condition that they say it can potentially skew their research- the risk of the medium inconformity- is not only out of their control, but they also do almost nothing to check it; i.e. it is exposed to a potential risk.

Stephan answered question 20 (do you trust that the company from which you buy the medium is giving you the medium that you want?) as follows: “u:::h ye:s I find some are quite well (.) they are (.) at least there is still test reports that you can request and (.) a::nd also the company at least I always have been using and I've never had any problems so it's at least in my own experiments I I found them to be very reliable and if there will be strange effects may that we would notice and it will be very bad when ( ) ((laughter))” (line 5202). Already, Stephan started to hint on the fact that if the medium would have “strange effects”, then it will be “very bad”. But Stephan’s way of checking is relying on the the company (the provider itself), which is both a judge and defendant. Stephan immediately reinforced the idea that the lab does not check after the provider. Rather, he is assured, because the provider’s best interest would be to sell an adequate medium, since its reputation is at stake: “there will be other business very very fast so I don't think they can afford themselves (.)” (line 5208). Stephan continued the sentence saying: “but some companies yes (.) I also have noticed that the: quality is not always consistent so people have to be we have to aware of the quality of the medium uh and match the bad variances”. In this very last sentence, Stephan started to make one step back, by admitting that mistakes happen and that not all providers are reliable, and that they need to be cautious. So far, Stephan mentioned that they have to be cautious about the “variances”, i.e. that the medium concentrations have to be consistent. He so far, did not mention anything with regard to the possibility of having a consistently wrong medium. Additionally, Stephan said that he does not remember all the ingredients of his medium by heart, but that he is serious about it. He is using the first personal pronoun, which conveys some realism

on what he is describing: “u::h I just I don't know it by heart but when I start an experiment I::: do a little study of what medium to use a:nd so when I go (.) at the moment like ok this good or what should be into the medium and I check this and if that is fine with me then I would proceed with my further experiments but I make a well thought choice of a medium before I start my experiment (.) I don't just take one medium a (.) I really choose like what medium to choose if there is many different ones” (line 5214). Now, that the importance argument is established, I will ask him question 21 (are there risks that if the medium components are not exactly as they are described they make the cells to develop resistance to drugs or in protein expression?). Stephan responded to my question using the “you” pronoun: “well I guess like indeed like (.) yeah may be in you have some interaction you could have some uh ((noise)) yeah in principle you can have inter: interaction between the 2 compounds so if it's different it could have in principle an effect it's possible (.)” (line 5679). After he acknowledged the skew that can affect the research, I expressed my surprise that he does not check on something that he said to be important. He will now reply, partly using the “I” pronoun when he will mention an action that is positive and partly the “you” when he refers to a weakness: “well it's it's possible when I start I investigate what is in it so I don't know by heart but at that moment I know what is in it and I know it's good or not and afterwards yeah you just have to assume that the manufacture is doing a good job and producing the right product (.) but it can happen indeed that u:h after a while you discover oh there must be a problem in the medium and then you'd have to back (.) it happens but then you'll have to repeat a big part of res of your research it's possible” (line 5690). Basically, Stephan, confirmed the idea that all he is doing is assume that the “manufacturer is doing a good job”. Consequently, at least according to Stephan, the lab does not know the exact composition of the purchased medium, which they say to be very important for the research results.

Brian too said that he trusts the provider: “yes (.) I trust”, and that he does not know the composition of the medium by heart, justifying it using the “you” pronoun: “no (.) it's a long list ((laughter)) you know the the: the critical like the glucose pyruvate lactate (.) the: glutamine normal acids and amino acids (.) you know more or less (.) but you don't know the serum you know that you need to grow factors (.) the grow factors to make the cells to be ( ) but I don't know it by heart” (line 5236). Just like Stephan, Brian will not account for the risk of that the medium may

contain some molecules that would skew the research results. I wanted to confirm my doubt, by checking on what is important for him. I said “you don't know by heart so what is important for you is that as long as the cells are alive that means that the medium is fine?”. He replied: “no (.) well this is important but the: (.) it's good it's important that the composition doesn't change (0.3) it's important that the composition doesn't of the medium is always the same” (line 5242). Again, Brian’s concern is consistency. Only in his reply to my very precise question (question 21) that he accounted for the risk of the unknown molecules in the medium, by saying: “yes if the components are not the same (.) it can change a lot (.) a lot” (line 5709). He then went back to justify this trivialization by the fact that the companies are reputable: “because you buy from companies that they have the quality control in the company is not like (.) yeah we prepare also our own home made medium (.) and it's much more variable I will say than the commercial one (.) so it's better the commercial one because the quality control is much higher (.) yeah we can depend on the ( ) is working well (.) is well calibrated (.) in company (.) all these parameters are controlled very tightly and they have better machines (.) everything is u:h more constant” (line 5716). In his justification, Brian was again referring to the idea that these companies are able to produce more consistent and more precise media then if they prepare them in the lab, but the question was not about consistency. It was rather about if this company adds a molecule that they would not know of. Brian repeated the same idea three times: that these companies are professional, that they are reputable; that their reputation is at stake and that their product is consistent. He avoided accounting for how to check if for instance the provider adds a molecule to make their research fail and keep using their products. With some degree of skepticism and cynicism, I can imagine a possible scenario where these providers would intentionally be causing the cancer research results to fail worldwide; otherwise they will stop selling and close down their business. I double checked this idea, by asking “but before trying another one don't you try to put your own medium and test it? At least for once in your life or in your career?”. Brian said: “I never do that”. Consequently, Brian too is confirming that the lab does not check after the provider, although he accounted for its importance with regard to research results.

Paul's accounts were also very similar to Stephan's and Brian's. Paul said "I trust them yes (.)", but when I told him that "and if suppose there is a molecule that should not be there is this u:h how how how dangerous can this be? Or how [", he interrupted me saying that "well it can lead to misinterpretation for sure" (line 5459). I then, expressed my surprise with regard to how they rely on trust; he replied as follows: "there is no other way to do (.) sometimes (.) I would say in my career may be I encountered may be 2 episodes of bizarre results that we tried to understand u::h and we identify some some aspects u:h being related to u:h the medium or the serum we used as being the the likely even for explaining the difference we observed (.) so I would say in a 25 years I've b b I have been for 20 years I've been facing a problem of such a problem because something not being as it was before (.) that's the only way I could identify if there is something occurring" (line 5461). Exactly like Stephan and Brian, Paul did not address my question which is about an unwanted molecule that can be added to the medium (maybe on purpose). Paul only referred to the inconsistency of results ("something not being as before"). In other words, if the skewing molecules can be constantly added to the medium, Paul will not be able to notice what he called "something not being as it was before". But Paul added that that is "the only way [he] could identify if there is something occurring". I reminded Paul that my doubt is about a consistently wrong medium (line 5468), but he replied with "yes yes" at the beginning of his explanation then he did not at all address my point (line 5474). He went completely off topic, mentioning general ideas with regard to how researchers need to check and reproduce results of other labs and how they would contribute to research. At this level, I tried to get the conversation back to our point, by first asking if the researchers need to know the medium components by heart. He admitted it stressing on why it is important to know: "again because of the core business of the lab (.) we we my (.) students my post docs myself we are we are more aware of what is in the medium(.) but again this understanding is even for us limited (.) we know several things and we know that I think clearly in a more elaborated way then many other labs who do not work on metabolism and the role of nutrients u::h so it's important for us to consider that medium and the content is not exactly the same and can influence the phenotype of your experiment (.) butsss no body will know (.) could tell you about the exact concentration of each salt amino acids vitamin in the medium (.) but whether they are glucose pure ( ) u:h glutamine u:h serin these kind of things is now more and more known by the by the people because (.) we know that this not

u:h anecdotical" (line 5488). Now that we are back to the point, I put my question 21 (if there is an unknown molecule, what would happen; line 5921). At this level, Paul put much stress on the extreme risk of skewing the research results: "(0.6) if the exact composition of the medium is not known u:h cells can simply proliferate u:h quicker or slower if there are minor change there is one thing lacking u:h the cells specifically the tumor cells they can adapt to such a situation and u:h what is more and more known is the plasticity of cells and tumor plasticity is such that u:h they can accommodate u:h many different stress conditions what an healthy cells cannot necessarily do a:h so (.) the cells can adapt and you may work on something which is due to u:h uh an adaptation not wished but an adaptation which to some problem in the medium you use so if the medium was not the is not the expected one (.) you could think that what you observe is due to the kind of tumor cells you use or the treatment you have added on the cells but it could be really due to something else which could be the lack of one amino acids because the company u:h would which offer which did produce the medium forget one step in the process but we cannot control everything clearly" (line 5925). Finally, after he admitted all that risk, I put the question again and telling him that if all the labs in the world are using the same medium (or the same raw materials), the skew would be occurring constantly and everywhere, so there would not be a possibility for other labs to check his own results, or for his lab to check other labs' results. Paul then replied using the "you" pronoun: "if the control of your experiments which is that other labs can reproduce your experiments and your results (.) if everybody is making the same mistake then you may certainly misinterpret the data and if you work on the drug development you can find a drug which is active artificially but which not which will not relieve patients because your model is too far from the reality ah if you push that at the extreme you could think about that" (line 5943). The most important expression, in this latter verbatim account is "if you push that at the extreme you could think about that". Consequently, Paul, like Stephan and Brian, prefers to trust that the provider is doing a good work; and that it is better not to worry about the possibility of the providers skewing the results (intentionally or unintentionally) of the whole world's labs. From this stand point Paul is trivializing a matter that he himself accounted for to be of extreme importance.

I will finish with Dalila's position, because it started differently. All the other participants' positions are similar to Paul's, Stephan's and Brian's. Dalila's position was different in that she had accounted that she does not trust the provider, right from the beginning, unlike the others who only expressed their mistrust based on the scenario that I suggested. But, what is even more interesting in Dalila's positions is that although she is in complete contradiction with all the others (who made clear trivializations as detailed in the previous paragraphs), her accounts still illustrate a well established trivialization practice. Dalila initially said that she does not trust the provider. She added that because its medium is industrial, it is necessarily not as reliable as the one made in the lab: "puisque c'est de la production donc d'office ils produisent jamais pareil (.) après u:h j'espère que ça soit eu:h assez reproductible mais euh je pense pas que ce soit 100% identique" (line 5504). But the most pertinent account to the idea of trivialization that Dalila made, is her reply to my question whether she knows the composition of the medium. She did not only say "no", but also that it is kept secret by the providers, otherwise other people or rivals can reproduce the same thing and compete with them: "ou ce qu'on rajoute dedans? ah non non tout fait je connais pas généralement c'est secret en plus (.) pour mais de mémoire on devait chercher les compositions des milieux et les 3/4 du temps tu sais pas puisque oui ok tu utilises ça ça ça et ça mais ils te disent pas tout parce que n'importe qui peut faire la même recette que et le vend moins cher" (line 5525). More interestingly, when I asked her to explainn more, she said in French something like "I don't know, it's like Coca Cola": "chai pas coca cola". She then added that the doctoral students only know what they were allowed to know and then she clarified "what the firm wants them to know": "les doc u:h les doctorants savent (.) ce que l: on veut bien leur dire […] ce que la firme veut bien dire" (line 5532). When I expressed my surprise, with regard to why the researchers do not think about their medium, she said "why should they spend time to think about it?": "pourquoi passer du temps à réfléchir?" (line 5547). She then added that if "you" want to study your medium, then you would do your research, but researchers can only select their medium from an online library named ATCC[49]. Again, when I specified more saying that if the researchers want to test a drug on cells, would they not wonder about the medium in which they live? She said one more time that they just look at what kind of medium they were told to use to keep in mind, but "you" cannot do anything but believe in what they

[49]ATCC is a private company, as mentioned in its website: http://www.lgcstandards-atcc.org/?geo_country=be

say: “comme ça tu l'as en tête mais u:h tu sais rien faire de plus (.) que croire ce qu'on te dit” (line 5563). Clearly, Dalila is accounting on the fact that the usage of the medium is taken for granted, that “they” anyway cannot do otherwise. She even said that the medium prepared in the lab uses a basic substance that they purchase readymade, and which we can only trust to be fine. She then compared it to what we buy from the supermarket, hoping that it would be beef and not something else: “il y a de toutes façons on a de toutes façons une base mais c'est comme ce qu'on achète dans le super marché on peut espérer que c'est bien du bœuf et pas autres choses (.) c'est la même chose” (line 5608). It is perhaps legitimate to wonder if Dalila’s accounts are to this extent contradictory with Paul’s, Brian’s and Stephan’s, because she is a technician while they are researchers, but I found that even Dilara made exactly the same comparison, when she said, that she would hope that such a cheating would not happen, but unfortunately it is possible, because we have seen stories such as “lasagne” made with horse meat, when they are described to be made with beef: “ben j'aurai envie de dire non (.) mais la réponse est certainement oui parce qu'on a bien vu euh avec les lasagnes les gens mettent du cheval au lieu du porc et du bœuf donc euf oui on peut tomber sur un dirigeant un mal veillant qu'il le fera (.) moi j'ai envie de me dire que non ça n'arrivera pas (.) mais eh ça peut arriver” (line 5782).

I have so far discussed 3 trivialized aspects with regard to the medium usage. I provided illustrations of how the lab members account for the extreme importance of these aspects on the one hand and how they are not taken into consideration on the other hand. We will now tackle the actants of the identified trivialization, through the “who” question.

(b) Who is (de)trivializing?

In the same way as we have done in the first chapter, in this subsection, I will address the actants of (de)trivialization; i.e. who *is trivializing the 3 skews of the medium usage?* I will detail the idea that actants of trivialization are multiple, interrelated, human and non human, and that they are both intra and inter organizational. Figure 3 (page 304) depicts the actants of trivialization. It aims at portraying the idea that these trivialization actants act as barricades that prevent us from understanding this issue. Also, in order to come to a (de)trivializing discussion,

I needed to formulate and reformulate my questions over and over again. Because people (our participants in this case) are perhaps more worried about the role they play during the conversation (the interview) than with knowledge building; varying the ways I put the question, triggered different accounts, and each account fulfils a different function that is context-bound. I detailed this latter idea on page 74 (section 3.3), based on Potter & Wetherell (1987)'s major tenets of Discourse Analysis, as a social psychological theory and method. It is perhaps counterintuitive to imagine that the informants would forget about who they are and how they think they are supposed to act during my interviews, or during any scientific discussion. If, for instance, they realize that they have just made scientifically wrong statements, would they be more interested to save their face and protect their image at the expense of accuracy, or would they sacrifice their image for the sake of rigorous knowledge building? This is why the current study considers knowledge building to be a dynamic that is not purely truth-oriented. The knowledge-building dynamic is dependent upon the contextual conversational interactions, which may even go against scientific accuracy; and this is why communication has to be at the heart of the knowledge building process. Our interview discussions around what has been otherwise trivialized can be a unique breaching experiment strategy (Garfinkel, 1967) to build a real meeting dialogue (in Eisenberg et al. 2010's terms), where knowledge can be more genuinely discussed. Finally, this is where discourse analysis emerges as a unique method to study the trivialization dynamic, as it is practiced during the interviews (by the participants), but also prior to interviews (by other factors, human and non human).

The outermost actant of trivialization, which is independent from our research and was in force, long prior to my observation (but was discovered through observation), represents a condition not a particular person or group of persons. The lab members happen to trivialize the need to think about this matter, because they take it for granted, as we detailed in subsection (a) above. Hence, trivialization is mostly an uncontrolled condition than a deliberate practice, because it is visibly inadvertent. But, trivialization can also be a practice, because at least according to Paul, Stephan, Brian and Daniel researchers do not only have the possibility to scrutinize their medium, but also have to do it, in order to avoid skewing their experiments. Using the participants' own accounts, trivialization in this sense is the

practice of omitting to take into consideration a crucial aspect of research reliability. From this perspective, and based on our analysis (particularly subsection (a) above), all lab members without exception have been trivializing the reliability of the medium usage and its skewing potential, regardless of how much they accounted for being serious about it. In section 3.2.3 (page 68) I distinguished between who has the legitimacy to say what is trivial (the participants) and who has the legitimacy to figure out who is trivializing (myself, through the analysis of the participants' accounts, and using their own words). The combination of a "general condition" which is inter organizational, perhaps governed by medium manufacturers (and is therefore profit-driven more than scientifically oriented), coupled with the practice of "omitting" (by the participants) make the outermost actant of trivialization, which functions as a barricade that is unfavorable to knowledge building. The unintentional "omitting" as a group practice may coincide with the "groupthink" phenomenon, introduced by Whyte 1952, Janis 1982 etc. to illustrate the idea that groups tend to converge and do things in the same way, for the sake of avoiding disagreements or misunderstandings. Disassociating between "the general condition" and the practice of "omitting" is perhaps absurd, because each of them can be both a cause and an effect for the other; and they can well be neither cause nor effect, but rather two sides of the same coin.

To move to the second actant of trivialization (level 2 on figure 3, page 304), I now wish to distinguish between the trivialization that preceded my interviews (as a practice made by participants in their own research activities) and the trivialization as a discourse practiced during my interviews discussions. More precisely, I wish to draw a line between the lab members that only trivialized "by default" or prior to this research and those that continued to trivialize the skew potential of the medium use, explicitly during the interviews (beyond the threshold of my breaching experiment). As a matter of fact, all the lab members have with varying degrees, trivialized this issue prior to our research, because (regardless of the feasibility) none of them had tried any of the following, which they reported to be considerably important: 1) to expose the medium to the cells, in a different way than the current system; 2) to try to avoid the possible auto-contamination, caused by the medium usage itself; and 3) to check if the provider's medium is constantly skewing their experiments (example due to an unwanted molecule). Then, during the

interview, all the lab members, except Paul, Stephan and Daniel have trivialized the necessity of: 1) exposing the medium to the cells, in a different way than the current system; all the lab members than none of them except Paul and Daniel have trivialized: 2) the idea that the medium can itself be a source of auto-contamination; and finally, all the lab members (except Dalila) then none of them have trivialized: 3) the need to check if the provider's medium is consistently skewing research. Finally, during the diachronic and collective interview (level 3 of figure 3, page 304), the participants initially proceeded in the same way as in the individual interviews, as if no previous discussons were made. They first trivialized the 3 matters related to the medium usage (1, 2 and 3), but along with discussions they detrivialized them. The recurrent oscilliation between the two opposite positions is detailed in section 3.3.5.6.

(c) Why is this trivialization occuring?

The participants' justifications of why they trivialized the items (1) to (3) mentioned in the previous paragraph, exposed a number of causes. The element (1) was justified by all post docs and the lab director by the unavailability of other techniques; which means that Oncology as a whole discipline has so far failed to notice this matter (again although the participants said it is of extreme importance). The element (2) was justified by the fact that cancer cannot be a matter of molecule contamination and that once cells become cancerous, they would not retain the molecule that caused cancer, anymore. I have detailed in the previous subsection how the lab members said that cancer can be caused by molecules (except Daniel only) and that auto-contamination is possible (except Paul and Daniel). Therefore, the trivialization of element (2) is due to the difficulty to sketch molecular cancer to be a matter of contamination or to imagine that the molecule never leaves the cells and the medium. Only through the interview discussions that all the lab members were able to change their positions and account that auto-contamination is possible (except Stephan who straight away admitted the idea). From this perspective, cancer seems to be not yet well understood. Although cancer research is advancing at many levels, it may not be advancing at one level.

I wish to argue one more time, that *understanding* or *knowledge building* is not a linear activity. Knowledge building can advance very much on one side, but gaps of knowledge would persist, even at the basic level. This is

true for both elements (1) and (2) above. While element (1) clearly concerns the whole oncology domain (because it fails up to now to come up with a better medium usage system, no matter how important it is for the totality of experiments, worldwide), element (b) is at least true for our lab members, who are still unable to account for a very basic information regarding their domain. This is perhaps true for other domains, including Communication, which can hardly define very basic concepts; although they are used abundantly in the literature and researchers keep building on them. Therefore, both elements (1) and (2) unveil an underlying cause (or condition) behind the trivialization of the potential skew risk of the current medium usage: it is again a rather complicated condition or situation- the condition of the non linearity of the knowledge-building process. I will call it: the "bricolage" nature of science, which was reported by Lambotte & Meunier (2013) to be also featuring research in social sciences.

The third justification of why element (3) is being trivialized, as accounted by our participants is the following: that they trust the provider. All our participants except Dalila said that they trust the providers, and all of them said that if the providers would intentionally or unintentionally add an unwanted molecule they would not only be unable to know, but that also the totality of their research will be skewed. And if this is true for all the experiments that they are doing, then all the experiments worldwide would be dependent on two conditions: that the provider has to be doing a perfect job; and that the provider deserves such trustworthiness. Clearly, these two conditions are out of the lab's control, which means that the participants' assumptions have no scientific basis. Therefore, element (3) altogether is unveiling another underlying condition- the condition of the "naivety" embedded in the practice of science. The term naivety here is not pejorative, but is rather used in the sense that Latour & Woolgar (1979) gave to how scientific facts are not always scientific. More precisely, Latour & Woolgar (1979)'s observation of the laboratory life provided a number of examples, which "point to scientific reasoning as not being dissimilar to common sense reasoning. Whether evidence is accepted as such, depend on factors that are not necessarily scientific" (p. 178). In our case, the "trust" factor is clearly not scientific, but is rather a common sense type of reasoning.

With regard to the trivialization that persisted up to the interviews, it is noteworthy that the three trivialized elements (1) to (3) were only trivialized before the interview discussions advanced, and that the lab members have on

the whole stepped back significantly towards the end of the interviews. The participants that trivialized the element (1) were all the lab members, except Paul, Stephan and Brian. I have detailed how these lab members have stepped back from their first accounts, in the first two sections of this chapter. Consequently, I consider these participants to have trivialized, because they simply had not thought about the medium, in the way my question was formulated, before my interview. This was also made obvious, by their responses to my question number 11 (have you thought about this question before?) where they said "no". So, perhaps the participants that trivialized the element (1) did so, because it came to them as a breaching-experiment type of question (Garfinkel 1967). Element (2) was only trivialized by Paul and Daniel, but for contradictory reasons, with each other on one hand; and with all the other participants, on the other hand. In anyway, Paul's and Daniel's accounts cannot be both true; and Paul and Daniel's position can only be true if all the other participants are wrong. In other words, cancer can or cannot be caused by a molecule (according to Paul and all the other lab members, it can; but according to Daniel it cannot). And, medium can cause auto-contamination or it cannot (for Paul and Daniel it cannot, but for all the others it can). Whereas Daniel refuted the whole idea that cancer can be caused by a molecule altogether, Paul, admitted the molecular cause, but refuted the contamination, because for him the molecule that causes cancer does not stay in the cells, but goes away. In the diachronic interview, after discussions with Stephan, Daniel changed his mind and admitted that auto-contamination is possible and that cancer can be caused by a molecule (see discussion on page 168).

Finally, all the lab members (except Dalila) have trivialized element (3), simply because all of them did not consider the possibility of the medium to be constantly wrong. Again, this trivialization was accounted for to be due to the fact that they trust that the providers are doing their job accurately, because they are professional and highly reputed. But when I mentioned this scenario, they admitted that this would then be serious, and not at all trivial. Therefore, the lab members who trivialized this issue are perhaps trapped in a system and plagued with the dogma of their routine research practices, which prevents them from noticing important factors (again according to them). This leads us to capture another hidden cause behind this trivialization, which I name: "the dogma". This is perhaps an instance of how the practice of science, which was initially conceived to be a liberating act, has become dogmatic

and sectarian (Feyerabend, 1978). In the way this research lab is functioning today, why would a PhD student or a postdoctoral researcher worry about the reliability of the purchased medium? It is counterintuitive to sketch them being able to find the time or interest for such a concern, with the number of issues and constraints they are facing, within the system (time pressure, funding limitation, stress, career invisibility etc.). Additionally, perhaps the providers of the medium, being profit-driven, have succeeded to dominate research and make it dependent on them. The domination of industry over research will be thoroughly discussed in the next chapter. But, I wish to mention here that power which is in Foucault (1969)'s terms, 'microphysical' and 'omnipresent' emerges significantly. Perhaps another discourse analysis, based on the Foucauldian method (Kendal, 1999), which I introduced in section 3.2, would make a very interesting contribution, because it will systematically dig down for how power is practiced on science and what role it (invisibly) plays to make this or that scientific issue important or trivial.

(d) How serious is this trivialization regarding the culture medium usage?

Based on the previous analyses (a) through (c) of the present section, we will now aim at understanding the seriousness of the fact that the medium has been trivialized or never questioned. Again, it is beyond the scope of our research to argue that this issue is or not serious for cancer research as such or that it has implications from that perspective. That the issue is important or not from a cancer research point of view was already discussed according to the lab members and using their own accounts in a verbatim way (particularly, subsection (a) above). Our aim at this level is rather to understand the magnitude of the issue, from our own stand point. In order to figure out the magnitude of the issue, we also need to sketch when, where and how the trivialization regarding the medium usage is occurring. The shortest answer to these 3 questions is correspondingly: 1) all the time, 2) in every cancer research lab and 3) first unnoticeably then defensively.

Addressing the "when" part of this concern, all lab members have said that the way the medium is being used (constantly on the cells) is the only technique available in their field, except the flow system, which is newly invented and still not well established. Historically speaking, this technique has been being used since the beginning of the cell culture. This being said, this medium technique is being used all the time, within this lab and outside it. In terms of

"where" this technique is used, according to the lab members, the answer is: in every cancer research lab in the world, except for the few ones that started to use the flow system, for a limited numbers of experiments (because it is still not practical). Based on the fact that the medium is used, all the time and everywhere in the cancer research labs around world, in the same way, it is clear that this issue is of huge magnitude. More particularly, if the providers turn out to be (intentionally or unintentionally) systematically adding unwanted molecules in the medium, the negative consequences on cancer research described by our participants, would be valid for all cancer research in the world and not only for this lab. It would potentially skew results systematically and everywhere, around the world. As we have detailed in subsection (a) the issue seems to be trivialized prior to our research, although it was reported to be important.

I can now directly address the "how" part of the trivialization dynamic. Prior to my raising of the question, trivialization seems to have happened unnoticeably, because the 3 objects of trivialization (the adequacy of the constant exposition, the non possibility of auto-contamination and the provider's reliability) were taken for granted. In the subsection (b) above, we have already argued that there are hidden non human actants of trivialization, which surpass the individuals or the groups. I have mentioned that trivialization is rather a condition rather than a practice; a default situation rather than a choice. I have also argued that prior to our research, the trivialization has occurred unintentionally. In the previous subsection (c) I concluded that our participants' practices of trivialization prior and during the interview (no matter how different they are) were rather unintentional for the various possible reasons that I have detailed.

With regard to intentionality, it is very hard to determine whether or not the lab members seem to have been knowingly trivializing the need to rethink the medium use, especially as related to its skewing potential. The issue is multi-layered, both at the individual and the group levels, and both during the interview and prior to it. To put it anticipatively, the answer depends on whether we refer to trivialization during or prior to the interview, and on the exact object of trivialization; from the 3 elements detailed in subsection (a) on page 175. I also have mentioned at the

beginning of the previous subsection (b) that each of the 3 objects of trivialization is partly dictated by a condition rather than by an individual or a group decision. The first condition is the unavailability of other technical alternatives; the second is the fact that the participants (except Stephan) initially said that the medium cannot be a source of contamination (because the molecule that may have caused cancer is allegedly no longer there), but then changed their position during the interview (except Paul and Daniel). And the third condition is because they all trust that the provider's medium is fine. From this perspective, I consider that the trivialization over the first object of trivialization (the fact that the medium is constantly on the cells) is *almost totally* unintentional, because it is almost completely out of any one's control. Since there is technically no other better alternatives, the current medium system is used without necessarily being thought of by any of the lab members. Perhaps the best evidence to this is their answers to the second question in the interview (have you heard this question before?), where they all said "no", except for Paul, Stephan and Daniel. From the same perspective, trivialization over the second object of trivialization (that the medium can cause auto-contamination) is *partly* unintentional, because the participants always have the possibility to doubt that the medium can be retaining the molecule that may have caused cancer, but reportedly they have never considered this possibility prior to our interview. By the same token, they also have the possibility to use some techniques such as the one described by Stephan to prevent auto-contamination (page 154). I said partly, because they only figured out the possibility of such auto-contamination, during the interview discussions; therefore, the neglect of such a possibility is perhaps conditioned by their routine practices. The third object of trivialization (element 3) is perhaps more *intentional* than the other two, because all the participants on the hand accounted for the extreme importance for the medium to be exactly as it is described, but they on the other hand leave such an important thing in the hands of the provider. From this perspective, neglecting the need to check after the provider is perhaps a choice that they intentionally made, because they all have this possibility. But from another perspective, checking after the provider has been completely neglected, perhaps due to the intellectual power that the providers exert on the participants decisions. As we have explained in the previous subsection, trusting the provider is not scientifically based, but rather a common-sense type of an opinion that is perhaps built based on the marketing and communication power exerted by the providers.

During the interview, and as discussed in section 3.3.6.2, all the participants who initially trivialized the medium skew potential emanating from the fact that it is constantly on the cells, have stepped back from this position after I asked them whether the issue is or not important and whether or not it has been trivialized. Similarly, they stepped back from the trivializing position with regard to the auto-contamination risk as caused by the medium use, during discussions around question 16. Again, all the participants who initially trivialized the risk that the provider would not give them the right medium have admitted during the last question (21) that the risk is there. From this perspective, their initial trivializing account was rather unintentional, probably because they have never had the occasion to consider the 3 risks that I mentioned (questions 1, 16, and 21). At this level trivializing seems to be inextricably related to taking things for granted. This leads us to the discussion of "why" the participants are trivializing these 3 risks. Table 2 (page 196) summarizes the (de)trivialization dynamic related to the current case (case 2).

Table. 2 The (de)trivialization dynamic (case 2)

What is (de)trivialized? (Object)	**Skew potential due to the current medium usage:** • **Object 1**: the consequences of the medium stagnancy (not flowing) regarding the nutrients' concentrations and the cells consumption. • **Object 2**: the auto-contamination risk emanating from the stagnant medium, in case of molecular cancer. • **Object 3**: The risk that the purchased medium may contain unknown molecules.
Who is (de)trivializing? (actants)	**Object 1**: • Prior to interviewing: A default condition beyond the lab: trivialization, no detrivialization. • Synchronic interview: all participants initially trivialized then detrivialized; except Paul, Daniel and Stephan, who only detrivialized. • Diachronic interview: recurrent oscillation between trivialization and detrivialization, by all. **Object 2**: • Prior to interviewing: A default condition beyond individuals and groups. • Synchronic interview: all lab members trivialized it, except Stephan; then all detrivialized it except Daniel and Paul. • Diachronic interview: all initially trivialized, then all detrivialized after Stephan's intervention. **Object 3**: • Prior to interviewing: Taken-for-granted: total trivialization, by all. • Synchronic interview: first all except Dalila trivialized then all detrivialized. • Diachronic interview: recurrent trivialization and detrivialization, by all.
Why does this trivialization occur? (causality)	**Accounted for:** **Object 1:** unavailability of alternative technological solutions. **Object 2:** molecules do not stay in cells once they affect them; cancer is not molecular. **Object 3:** providers are professional, competent and trustworthy. **From analysis** **Object 1** • Prior to interviewing: not identified. • Synchronic interview: no predisposition to rethink the current system, as long as better alternatives are not yet available. • Diachronic interview: (same as synchronic). **Object 2** • Prior to interviewing: not identified. • Synchronic interview: trivialized because cancer was accounted for not to be a matter of contamination emanating from molecules; detrivialized when the contamination was accounted for to be possible. • Diachronic interview: (same as synchronic). **Object 3** • Prior to interviewing: not identified. • Synchronic interviews: trivialized because they said that they trust the providers; detrivialized when they realized that this gap is possible. • Diachronic interview: (same as synchronic).

When is this trivialization occurring? (temporality)	• Frequency: All the time, almost in every experiment. • Historically: Since the foundation of the lab.
Where is this (de)trivialization occurring? (spatiality)	• In this lab (from finding). • A worldwide condition (accounted for).
How does this (de)trivialization occur? (manner)	• Prior to interviewing: Not identified. • Synchronic interview: **Object 1**: trivialization: unnoticeably; detrivialization: willingly. **Object 2**: trivialization: unnoticeably, except Paul and Daniel who trivialized willingly; detrivialization: willingly. **Object 3**: both trivialization and detrivialization: willingly. • Diachronic interview: **Object 1, 2 and 3**: recurrent oscillation between trivialization and detrivialization: willingly, defensively, with signs dissonance avoidance.
How serious is this trivialization? (magnitude)	• It can potentially skew in vitro research results.

3.3.8 Primary Findings (case 2)

In this section, I will outline the primary findings of case 2. In section 4.1, where I will outline the findings of the whole research, these primary findings will be squeezed up with those of cases 1 and 3. While the primary findings are specific for each case, the whole research findings will be reshaped to fit the 3 cases altogether, before moving to the discussions (section 4.2).

Discoveries

When I started to manifest my interest in the medium usage, the participants (unlike in case 1) told me that they are very careful about it and that it is one of their major priorities, which did not discourage me from studying its trivialization. I based my continued interest on the participants' trivializing accounts, which they made at the very same time they were emphasizing that the medium was important to them (prior and during the interviews). Unlike in case 1, the medium usage is not itself *a seen but unnoticed* practice (Garfinkel, 1967). Rather, within the usage, I found that there are 3 conditions that are seen but unnoticed. Hence the medium usage itself is not trivialized, because participants consistently said that they take it very seriously, but I found that the following 3 conditions in particular were *seen but unnoticed* (i.e. trivialized): 1) the medium stagnancy (not flowing) and the risk of shifting from in vivo models due to the fluctuations of the nutrients' concentration and the consequences on the cells consumption; 2) the auto-contamination risk emanating from the stagnant medium, in case of molecular cancer; and 3) the risk that the purchased medium may contain unknown molecules that may skew the research results. While the participants' first described that the medium usage is appropriate and non problematic, unnoticeably trivializing the serious negative consequences of the 3 mentioned conditions, they ended up considering that they are of major importance. Therefore, the participants have on the one hand detrivialized the skewing risks of these 3 conditions, but on the hand trivialized them, justifying their position as follows:

1) For condition 1, the participants said that there are no better technical solutions to get the medium to flow and make cells consume in a rather more similar way to how cells consume in vivo. Furthermore, when I mentioned another system that allows the medium to flow (and which was initially mentioned by one participant during the observation), they described it to be closer to in vivo and hence better; but when I asked why they did not consider using it, they minimized its usefulness. One participant said that it is not that important to be very close to in vivo, although in other contexts they always emphasized that there is no priority over being as close as possible to in vivo (i.e. to avoid false negative or false positive findings). Another said other labs might be interested in the flow system, but not this lab in particular, because this lab has other priorities; while all her colleagues emphasized that this lab is in particular focusing on cancer metabolism and that the medium composition role is a core issue, in this regard. At other occasions, the participants said that maybe it is better to use the flow technique and be closer to in vivo.

2) For the second condition, only one participant immediately recognized the risk of auto-contamination due to the current medium usage. Others initially refuted the idea that auto-contamination is possible, justifying their position by saying that cancer is not or cannot be caused by molecules (i.e. it is rather a cell proliferation disorder). Further discussions with some participants made them say that while cancer is a disorder in the cell proliferation, it can be caused by cancerous molecules and that these molecules can possibly stay in the medium and cause auto-contamination. One participant never changed his mind during the individual interview, but dramatically shifted of position during the collective interview. When I pointed out that only one participant immediately acknowledged the auto-contamination risk and that others did not, the controversy emerged again and this participant managed to convince all his colleagues that the auto-contamination is quite possible and that cancer can be a matter of morbid molecules that never leave the cells and not only a state of proliferation disorder. After the “dispute” was settled, the participants ended up to acknowledge this auto-contamination risk.

3) The participants do not know all the medium components and justified this condition differently (that the list is long; that they do think about them when they design research, but they do not know them by heart; and that

the medium components are kept secret by the providers). More importantly, although they mentioned that every detail may skew research results; the participants said that they trust the medium's providers and that they do not check the components after them, because they are: a) professional, b) competent and c) trustworthy. When I stressed on the question "*how do you know that they are trustworthy*?", they stressed on their competence rather than on trustworthiness. When I insisted on the bases of trustworthiness per se, they insisted on that the lab is not able to do what the providers can do. Finally, when I asked them to account on the possible outcomes, if the providers happen to be (for any reason) systematically altering the medium's components, they said that it would then be very serious (i.e. "there you are blinded"; "results would be wrong"; "that would change a lot a lot"; "you can have terrible consequences" etc.). However, as soon as they describe these negative consequences, they restate again that this scenario would not happen. When I ask why, they say again that because providers have no reason to cheat.

(de)trivialization

I found that the participants used the same discursive strategies as in case 1 to switch between trivializing and detrivializing accounts, and to minimize the amplitude of the contradictions that I pointed out or to reframe the weaknesses of the discussed practices. In the individual and collective interviews, I found the same strategy of switching into the use of the inclusive "you" instead of "we" or "I", when they switched from describing real practices that foster their competence to describing weaknesses or hypothetical practices that they do not do. The use of "you" played a role in implying that "you too would do what we do and not only us". Furthermore, they used "sometimes", "it depends" and "also" to join two incongruent ideas together. In the collective interview, the participants initially responded to my questions with very simple answers and avoided unpacking the complexity and the controversies that we have already discussed during the individual interviews. They also avoided contradicting each other, except once when I clearly pointed out that the one participant said one thing and the other said the opposite. Unless I asked them to provide precise accounts on the negative consequences of the 3 potentially skewing conditions, the participants defended the usage of the medium, without backing up their position. They rather accounted again that they have no better technical alternatives (although they mentioned the advantages of the other

flow system) and that the providers are trustable (again because they have no reason to cheat). Thus, the participants restlessly oscillated between the two positions.

Case 3: Do you own your research?

This chapter is closely related to Appendices D1 and D2. Appendix D1 consists of the series of the individual interviews' transcription; and appendix D2 consists of a collective interview transcription. The transcription lines are numbered, for convenience of reference. Every verbatim account that I will use in this chapter will be referenced using their respective line numbers.

3.3.9 Background (case 3)

This case perhaps started from a feeling - an intuitive feeling that was ignited by various accounts from the lab members. One day, I invited a director of one of the other labs that share the space with this lab, for an informal interview in the cafeteria. During the interview, she said in French "malheuresusement dans notre domaine, on est confronté à l'échec d'une manière régulière", which I translate as "unfortunately, in our domain, we are regularly confronted to or faced by failure". She transferred to me a feeling of frustration and bitterness that perhaps played a role in my focalizing on this idea. When I asked her why, she said because most of the experiments do not lead to promising results, in spite of the fact that cancer and heart diseases research are the most sponsored, among all the others.

Another day, the lab director told me that he heard that I was asking 'hard' questions, then he expressed his sorrow that in their field they are surprised that the reasoning capacities of their PhD students is still under par. Another day, he told me that he is impressed by how his counterparts (other professors and lab directors) from the social sciences fields deal with the same administrative issues that they face, and how they are able to negotiate issues. He mentioned a couple of examples, to illustrate how they were smart and adaptive, during negotiations. Additionally, on different occasions, I was told by different lab members expressions of this kind: "we are weird, right?". Bottom line, I had a feeling that people are not sure that what they are doing is right, probably because the majority of experiments do not usually lead to promising results. When I discussed with the post doctoral researchers, they kept telling me that they are actually trying things out, and that they have to make guesses and take choices.

Every day, I opened new discussions with the lab members regarding the rigor of their research, but the overall impression that I had is that the biological experiments are extremely complex in terms of parameters and variables and researchers need to build cases, based on assumptions and educated opinions. At some level, I asked a number of them, whether biology is that complex or is it rather that the way they are approaching it is more complicated than it perhaps should. The question was like a surprise to many of them, because it was like an occasion to think on whether or not all that complication can be approached in a simpler way. At a later stage, I decided to every time zoom-in on a specific research practice, at a time. The question that I had in mind, during this mission was: how can they be sure that this or that research practice enables them to know what they want to know? I ended up with four main inquiries:

The first inquiry was built around the use of machines. There are plenty of machines in the lab; some are simple and some are very sophisticated. Sometimes, I followed one researcher in his or her day journey from one machine to another; and sometimes, I sat by a particular machine and asked different researchers the same question about it. I always opened the discussion with this question: “what does this machine do?”; and I had always received very simple and similar answers such as: “it gives you this or that measure”. I asked other questions, such as: “but what does it particularly do to be able to find this or that measure?” At this level, answers start to become different, usually depending on the participant’s profile. Then, usually my third question is: “do you trust this machine?”. This question almost always triggered some surprise. Usually, the lab members answered it straightforwardly as: “yes” followed by “hopefully”; but Stephan’s answers in particular were more sophisticated. Then the fourth question was “how do you know that this machine measures what you want to measure?”. It is at this level, that the participants’ answers become considerably different. Some say that they don’t know how, although they had said that they were sure that the machine is well designed to do its work properly. Some, depending on the machine, were able to explain how it works. Some said that they should not go into those details, and that as long as the machine is working, they should not waste their time to think about such “philosophical” issues, because they do not have time for that. On different occasions, I have seen maintenance technicians visiting the lab to fix machines. Usually, the technician is

received by one of the post docs, who tells him what sort of glitch the machine is having and then leaves him fixing the machine without necessarily trying to learn from him more about how the machine enables them to know what they want to know. The technician spends some hours and sometimes a couple of days fixing the machine and then calls the designated post doc to test that everything is fine to finally go away. Bottomline, the lab members seemed not to doubt or think about machines, unless they make errors. At this level, I decided to include this inquiry in the next interview series. The concern is precisely the trivialization of the risk emanating from the possibility that machines are not measuring what they are supposed to measure. Machines may be consistent, but they may be consistently measuring other things; not specifically what the participants want them to measure (Latour, 1987).

After machines, I decided to zoom-in on consumables, which resulted in the building of my second inquiry. The lab members consume products that range from substances such as medium, trypsin, powders, gels and the like, to small tools such as pipettes, plates, tips, filters and many more (technical words are defined in the glossary on page 330). These consumables have to be certified and they have to be purchased from a reliable provider, because their chemical composition may influence the cancer cells and therefore skew the research results. At least from my perspective, I see that these products consumption is ongoing every second all around the clock. The lab members are constantly consuming without stop. In the way research is currently designed, without these products, research operations would not be possible. I asked the post doctoral researchers who confirmed that almost everything is purchased from providers and that their research heavily depends on them (they all confirmed this during the interviews too). I wondered if the lab controls these products to ensure that they do not skew their experiments. Surprisingly, all the lab members said that these products come from highly reputable providers that perform checks and controls in their firms, which means that they trust them and do not need to check after them. I wanted to make sure that if mistakes are made in these products, the consequences are or not serious. Again, all of them told me that yes these products have to follow strict rules and specifications; otherwise they will skew the experiments. Discussions with the post docs left me with the impression that the lab never doubts the quality of these consumables, unless something wrong happens. The wrong “thing” that they all referred to was if experiments are not consistent,

but what I was thinking about is if some products are consistently not as described, they would not be able to know. I decided to include this inquiry in the next interview series. The concern is precisely, the trivialization of the risk emanating from the possibility of the purchased products to be not as described and hence consistently skewing their experiments.

To go back to the statement made by the principal investigator in French which I translated as: “unfortunately, in our domain, we are regularly confronted to or faced by failure” (first paragraph of this subsection), I wondered if there is any procedural habit that is taken for granted and that may be contributing to this failure. The lab members told me many times that successful in vitro research often fails at in vivo stage. They said that this is due to difference between working on naked cells and working on mice, for example. If they test a drug on a cell, the drug may work, but when they test it on mice, it may not work; because it may not reach the cells or because cancer behaves differently within organs and in the body. Although they all accounted on this problem (also all of them spoke about this in most of the interviews), they still do not involve clinicians in their in vitro experiments. Because clinicians (i.e. oncology physicians) know the body’s anatomy and physiology more than biologists do; they are well positioned to predict, if this or that drug would be unsuccessful if it would be administered to mice and humans. I opened this discussion with the post doctoral researchers, who stressed on the importance of involving clinicians in their earlier research stage. When I put the question “but why you still work without clinicians here?” Their answers varied from “yes, we should do better in this regard” or “we already have some here”, although they do not. The two physicians, who were there, were two cardiologists from the other labs that share the space with them. They were constantly busy with their own research, and as far as I could understand, they were never involved in any of the research protocols of this lab. So again there is a discrepancy between what the lab members say it is important (involving clinicians right from the in vitro stage, not to be confronted with surprises at the in vivo level) and what they actually do (not involving them). I decided to include this inquiry in the next interview series. Along with this concern, I needed to ask the lab members, why they consider that cancer is something to be understood at the cell level first and then move on to bigger and bigger (tissue, organ, and whole body). Therefore, this concern will make

my third inquiry during my next interviews, because it will be useful to understand the different lab members' positions with regard to their reductionist [50] approach.

Finally, the discussions about the need to study cancer from different perspectives caused the idea of multidisciplinarity to come up to the surface. Discussions about multidisciplinarity entailed the citing of the different specialists that are related to cancer, such as: biologists, oncologists, medical doctors or clinicians, nutritionists, psychologists etc. Along with the citing of these disciplines, issues of coordination, cooperation, communication and interaction were always part of the discussions. The lab members said that they understand the need to study cancer in a more diverse context, but at the same time they cannot imagine how they can design the work in such a way that ensures an effective way of orchestrating all those specialists. Whenever, the discussion gets to this level, I always ask if they know that this is already an area of interest in the framework of Organizational Communication. Overall, the participants have very little knowledge of the fact that Communication and Sociology are interested in studying how interactions and group dynamic are strongly related to knowledge building. Yet, during the discussions, the lab director and the post docs, in particular, were able to figure out that interpersonal interactions enable us to build new knowledge and not only share it (i.e. not just to exchange information). Almost all of them have cited at least an example, where important knowledge emerged due to what they call a 'positive' interaction environment. Bottom line, I had the impression that this latter idea only became clear during our discussions and was not thought of before. Therefore, I decided to include this inquiry in the next interview series. The aim is to understand what makes research labs designed in such a way that they are not as diverse as they ought to be (at least as how the participants said they should be). Again, the discrepancy here is between *saying that diversity is crucial* and *designing research in more or less compartmentalized way*. Additionally, there is a need to understand why although our participants say that 'communication' and interpersonal interactions are very important in knowledge building, they never considered consulting communication specialists in the design of their labs.

[50] Understanding a system by dismenting it into the smallest entities, assuming that understanding the totality of the entities will enable the understanding of the whole.

When I combined the four inquiries that I have just outlined, I found that the common ground between them is encapsulated in the question: “do you own your research?”. The next subsection will try to answer this question, in view of the four inquiries: 1) if the participants happen to blindly trust the black boxed machines and cannot ensure that they are really measuring what they want them to measure, how can they still be confident that their research is under their control? 2) If they happen to be unable to check whether or not the products they get from external providers are not as described, how can they ensure that their research is not skewed? 3) If their successful in vitro experiments often face failure in vivo (because the body is different from the naked cells); how can they still keep going in vitro experiments without the other specialties? 4) If they keep saying that ‘interaction’ is crucial in their own knowledge building, to what extent they can still ignore to seek communication specialists’ advice in designing the whole labs’ modus operandi at the interpersonal, interdepartmental and inter-organizational levels? While inquiries 1 and 2 will aim at exploring the risk of over entrusting a significant part of their research to the providers of machines and consumables, inquiries 3 and 4 will aim at exploring the risk of not entrusting some aspects of their research to specific specialists, when the need is said to be real (examples: the clinicians and drug designers at the in vitro level; and the communication specialists in designing a suitable organizational environment for fruitful interaction and optimized knowledge building). Hence, the interviews will be centered on to what extend our researchers own their research, by doing or by omitting (i.e. hyper entrusting and hypo entrusting).

The semi structured interview, which I will introduce in the next subsection, will be dedicated to the issue of our researchers’ controllability of their own research, i.e. *to what extend is their own research under their control?* I suspected this to be an instance of trivialization- a trivialization about something that is otherwise described by the researchers to be extremely important. I was intrigued by the paradox between the almost total trust they give to machines and products on the one hand and the statement that the tiniest error in these may have severe implications on their experiments, on the other hand. Conversely, I was intrigued by the paradox between saying that they crucially need other specialists on the one hand and not soliciting them on the other hand.

3.3.10 Interviews analyses (case 3)

In order to be able to address the questions (1) through (4) that I mentioned at the end of section 3.3.9, I initially designed a semi structured interview, which I conducted with each participant individually, between June and September 2015 (Appendix D1). Then I conducted a collective interview in February 2016 (Appendix D2). While the individual interviews will be analyzed synchronically in the subsections 3.3.10.1 and 3.3.10.2, the collective interview will be analyzed diachronically in subsection 3.3.10.3. The individual interviews, included in appendix D1, have 9 questions (numbered from 22 to 30). As I did in the other two cases, depending on the participant's response to each question, further questions were asked to deeply explore each participant's position (or positions). With the interview questions altogether, I meant to target the consequences of the excessive trust our participants give to machines and products and the insufficient trust they give to other specialists. The collective interview is rather less organized, because the participants anticipated many questions, but I nonetheless made sure that I went through all of them. The following is the list of the 9 questions:

Question 22: What are the processes that you usually undertake to check and double check your results?

Question 23: Do you trust machines?

Question 24: Do you trust the products that you purchase from other companies?

Question 25: In your experiments, what is the ratio of the operations that are made ready to you by other parties?

Question 26: Can you draw me a diagram showing the different stages or operations of your current research?

Question 27: How do you account for the condition of depending on other parties?

Question 28: Do you think that cancer research labs need to open up and involve other specialties?

Question 29: Why do you need to study cancer at the cell level first?

Question 30: Is it enough to read about other specialties' contributions or do you think that everyday interactions are irreplaceable?

3.3.10.1 Accounts variation (case 3)

For the convenience of reference, the participants' accounts were reordered by question, not by participant. Thus, we can spot and compare all the participants' accounts for each question. If we want to reorganize the interview per participant, we can do so, by first selecting the participant initials (example RM) and second trace his or her numbered answers from 22 to 30 (example. RM's answers are from RM 22 to RM 30).

In this section, I will illustrate the participants' variation of accounts, with regard to the following: that they excessively entrust a significant part of their research to the providers of the machines and consumables that they are using.

Machines

After I made an indirect and open question (question 22: what are the processes that you usually undertake to check and double check your results?) to see if the participants would hint on any idea suggesting that they sometimes doubt the machines, question 23 was very direct and open: "do you trust machines?". Whether they said "yes", "no" or "sometimes", no one among the participants accounted for what I personally meant by the question. With every participant, I had to make another step forward by saying "how can you be sure that they are measuring what you want them to measure?". Even with this question, some participants did not seem to have got my point. I will now divide the participants into 3 groups: group one (Cédric, Paul and Joanne) who accounted that they trust the machines and do not have to worry about their inherent validity; group two (Stephan and Daniel) who accounted that they do not trust machines and have other ways to further investigate; and group three (Dilara and Gaelle) who said that they sometimes further investigate. I will now illustrate how all the participants no matter if they belong to group one, two, or three, trust machines in the same way, when it comes to practice. No matter how the accounts are different, they all converge to the same position, when it comes to practice.

“I trust machines”

Cédric responded to question 23, determinedly by saying “yeah”. When I told him “is it like you have to trust them or you need to trust them?” he confirmed his position, by saying: “no, they are designed to do what they are doing”. When I told him “and how do you know they they are functionning u:h for example measuring what you really want to measure??” (line 6849), he started to figure out that the question is meant to go into deeper discussions. At this level, he acknowledged that he does not know and that his relationship with the machine is rather down-to-earth: “((sighing)) I don't know it's (.) just they are designed like that (.) it's like if you're using Word but you type a double b you see the double b on the screen you know that the machine is doing the right stuff (.) so ((sighing))” (line 6851). I tried to make this position problematic, by saying machines may be like black boxes and we are not sure if they are doing what we really want them to do (line 6856), but he confirmed again that this is not a concern for him: “Yeah that's why you pay a lot for these machines (.) so if if they start not to work (.) sometime you can have trouble and then you call the man from the company (.) and he can he can he can come to see what's going wrong (.) sometimes of course it will take you one or two months to see that there is something going wrong with the machine” (line 6887). For Cédric, machines are not to be doubted. As long as we pay much, we can obtain machines that can assure validity and reliability. He added that the only issue is troubleshooting, but not to question their validity as long as they are up and running. Therefore, even after I explained my point very well to Cédric, he continued to consider it not an issue to be concerned about, which is quite understandable from a PhD student who is still in his first month in the lab. Cédric is perhaps focused on his research proposal and is not yet well positioned to rethink everything. Yet, his use of the “you” pronoun suggest that his position is not only his, but rather a ‘reasonable’ one and that doing otherwise is perhaps a waste of time or unworthy.

I went even further and explained to Cédric that people who designed the machines may be not as knowledgeable as himself in terms of biology, and may also be profit driven, so the machine may be not doing what he is assuming it is doing, but he said “[Yeah but that's that's YOU (.) that I have to work with the machine in the right way (.) so the man designed the machine to uh sense fluorescence (.) so then you use the primer data

fluorescence and the machine is going to sense the fluorescence (.) just going to give you the amount of fluorescence in your in your wells that's all" (line 6897). Again, for Cédric the problem is in "you" not in the machine. Although we were speaking in general and not even about a particular machine, he basically said that "the man" (which he does not know) who designed the machine knows what to do. At this level, I would understand Cédric's position to be rather defensive. He perhaps felt uneasy to step back and acknowledge the sense that I meant when I said machines can be problematic. Perhaps he did not want to contradict himself, so he preferred to keep the same position.

Paul said right from the beginning that "he had to trust them", hence acknowledging that it is a matter of obligation rather than a choice. I explained to Paul that what I meant by the question is if he is sure that the machine is measuring what he (as a biologist) wants it to measure, not just being sure that the machine is running well or is well calibrated. He said "yes". When I explained my idea, by saying that machines can be consistent and well calibrated, but they may not in fact be measuring what we want to measure, he said "I don't ask myself these kind of questions (.) I'm I I consider that is being measured is uh reflects the the reality (.) I I don't doubt the except if if again we have obtained ten times the same thing and the eleventh time we do the experiments we have u:h we have uh bizarre behavior than we could start to check whether is the machine is is working properly but we we do the maintenance of the machine in order to: to to get them: uh as uh as efficient as possible (.) this is uh usually checked for the machine why you have them the highest risk of uh of problems this is checked by companies uh who knows the machine better than than we do and who come to validate them using specific QC protocols or quality control protocols (.) so we we are prone to to believe (.) the machine" (line 6821). Paul's account, on the one hand admits that he never doubted that the machine in the way that I explained before, but he added that the providers should be doing their job properly, because they have specific controls. After I explained again to Paul, he almost repeated the same idea, which is basically that they buy from highly reputable providers and that the machines "have been validated by by others uh (.) so uh we we have reference on on what we should get with ourselves". Paul, who had already said that he does not ask himself such questions, now told me that he does not purchase black boxes, a term which I did not use myself: "we discuss with colleagues already using the machines so we make our minds in order

to (.) finally buy the machine which (.) for which we are convinced that it does measure what we (.) u::h want to s to be measured and then it's a real it does truly reflect the parameter we we uh which is the one to investigate so we uh uh it's not like we we're dealing with black box we're dealing with machine which which are uh which have been validated by by others uh (.)" (line 6834). Paul's account serves him to say that they know what they do, and that before they purchase machines they make good decisions based on a reasonable strategy, which is asking "others", again without accounting on what makes him say that they do not buy black boxes or how they know that they are measuring what they are supposed to measure.

Joanne did almost as Paul and Cédric, but acknowledging that she does not master these areas. She said that she trusted machines, because others also do: "because I don't I don't really know u::h machines and stuff like that but u::h if it is used by everybody like I have to trust them ((laughter))" (line 6915). When I tried to explain more, she interrupted me by saying: "[it's not stupid people who're making those machines" (line 6918). Joanne here joined Cédric, by assuming that people who make machines know what to do, so we do not need to doubt their work. I told her that because these people are profit-driven, so for example they are supposed to work more on convincing you that their machines are able to do what you need, which is not necessarily correct. For me it is the biologists who are supposed to ensure that the machines that they are using are in effect doing what they want them to do. But again, Joanne insisted that "Yes they must produce something it's like it's not like u:h for example if you use a ma machine to:: to check the concentration and stuff like that it's not a stupid thing (.) it's correct" (line 6924). Then, we focused on a specific apparatus as an example, but again Joanne kept on describing how to use the machine and the technique, while I tried to reformulate my point again and again – that I am not speaking about the use or the calibration of the machine, but about how we can make sure that they really reflect what we think they are measuring (lines 6936-6980). Ultimately, Joanne said that she does not know and that she will ask about that: "I don't know [ ] maybe if I ask I will know" (line 6984); I said "ok", she said "I will get the answer". Joanne's latter account is suggesting that answers to such complicated questions are out there somewhere in a document or in someone's mind.

Perhaps, these latter accounts reflect an over simplistic view of knowledge and knowing as opposed to information and data.

"I don't trust machines"

On the opposite side, Stephan said that he does not always trust machines: "u:::hmm: not always (.) depending (.) I'm I always remain critical to see in the (.) it's also why I do my own controls and calibrations and all checks to see also whether the machine itself might have (.) error" (line 6653). Then, when I asked if he sometimes has to trust them, he went straightaway to what I exactly meant by my question: "[you may so much so much treat it like a black box and uh you put something in and you get something out and it's (.) you just have to re rely on what happens in between this (.) it's correct" (line 6661). I did not want to use the expression "black box" on purpose, because I would not know how it can be interpreted, but Stephan used it himself to refer to the idea that we can blindly trust machines assuming that they are doing what we want them to do. Interestingly, Stephan used the "I" pronoun in the first account, but switched to the "you" pronoun, although he is talking about his own activities. When he used the "I", his account suggests that he is doing rigorous and well thought of research activities: "I always remain critical", "I do my own controls and calibrations" and "all checks". However, he used the "you" to refer to doubtful research practices: "you so much treat it like a black box", "you put something in and you get something out", "you have to re rely on what happens in between this". This switching between "I" or "we" and "you" is now a very recurrent practice. It is almost used systematically by most of the participants, which is why I would consider it to be a pattern that mirrors the swinging between two opposite accounts: the "we/I master" account and the "you grope" account ("tâtonner" in French). In this particular case, Stephan's opposite accounts attributed the use of machines as "black box" to "you" and the "critical", "checks", "controls", "calibrations" to the "I".

On the same side as Stephan, Daniel responded to my question tentatively, but at least, his account shows that he understood my point right from the beginning and that he is engaged in a critical thinking about the issue: "((laughter)) these 3 points no: I would say that (.) in most of the cases we (.) if we know how the machine is u:h working (.) we can know (.) if we can know trust the machine or or not (.) so normally what (.) I don't like so much

u::h when the machine is uh giving directly the results so (.) I prefer to analyze the raw data (.) and to:: to interpret by myself uh (.) the data ((noise someone interrupting)) but u:hm (.) sometimes we:: (.) it's not often the case we (.) or we say that for (.) yeah (.) normally we we:: (.) we have the the possibility to:: to check (.) how the the results are: obtained" (line 6678). Basically, Daniel's account enables him to show that he is 'aware' of this issue and that he can know what machines are exactly doing if he wants to know. However, the confusion comes from the fact that he said 'normally' and 'we have the possibility', which means that this is not necessarily what happens in practice, but rather a possibility. This latter idea was reinforced when Daniel added: "[But it alway it always depend if you:: uhm in your experiments you add (.) some controls (.) to be sure that what is measures it's true or not (.) if you have a positive control or negative control and you where is the differences (.) ok you can trust" (line 6691). Again, Daniel is using the "you" pronoun although he was talking about his own experiments, which reinforces the idea that understanding how machines measure things is not necessarily practiced, but rather a 'possibility', otherwise he would have used the first personal pronoun "I". More importantly, Daniel accounted on "positive control or negative control", which is not a way to understand how machines inherently measure things, but rather to compare the machines' measurements with other measuring ways or using different machines for the same aim. This idea of comparing rather than understanding is reinforced by his saying "and you where is the differences". In this very last verbatim account, Daniel also avoided completing the sentence with a verb (after the pronoun "you"), such as "understand" "figure out" "find" etc. Interestingly, Daniel started to answer my question (first verbatim account in this paragraph) with "we know", then reduced the likelihood to "we can know" and then to "we normally have the possibility", which suggests that he was figuring out the answer at the same time of speaking. During the same account, Daniel mentioned what he "prefers" to do using the "I" then he figured out that what he refered to is not what usually happens; so he said "but sometimes we" then omitted to continue the sentence with "we" and used "it" instead: "it's not often the case". He also replaced the "sometimes" with often, which means that even the checks that he referred to are not often done. I conclude: 1) that Daniel has never thought about this issue before and therefore his account failed to provide an answer to how they check the machines' validity 2) that he formulated his account to show that they are aware of it and that they are methodically able to address it, appropriately.

“I sometimes don’t trust machines”

Dilara started with laughter, then an out loud sigh, and then she said in French “yes but not all”. She then added “not all” again, “because some are calibrated by the Man, and that Man’s work has to be checked”: “((sighs out loud)) oui mais pas toutes (0.3) pas toutes parce que:: il y en a qui sont::: calibrées par l'Homme (.) et euh: je crois que ça il faut vérifier aussi” (line 6710). Dilara’s explanation attributed her *occasional distrust* to issues of calibration, not to what I meant by the question- whether or not the machines really measure what they want them to measure. Hence, I had to make another step forward and say it explicitly that did not mean calibration, but rather whether the machines are doing what she wants them to do: “ok maintenant au delà du calibrage supposant que le calibrage est parfait (.) comment est ce qu'on peut mettre sûr que la machine mesure ce que t'as envie de mesurer?” (line 6717). She took 3 seconds and then replied with a question “how to be sure?”: “comment être sûr?” . She then immediately started to describe ways to check, but she again was not answering the question; she rather was mentioning ways to ensure that the machines are well calibrated and or consistent. More importantly, she used the second singular pronoun “you” in French “tu” rather than saying for example this is how “we” check; which I would interpret as she was not describing what they actually do, but what can be done. She reinforced this idea by saying again “do you want to check that the machine is measuring what you want, if this is important to you”, which literally means that this is important to me but not to her. All the rest of Dilara’s account was about how to compare results, but never about the machines’ inherent validity: “(0.3) comment être sûr? Tu veux vérifier (.) si la machine mesure ce que tu veux (.) si pour toi c'est vraiment important (.) tu peux le faire avec un autre échantillon dont tu connais les résultats (.) donc si tu veux mesurer par exemple le pH d'une solution (.) de ta solution de ton milieu (.) tu veux mesurer le pH de ton milieu (.) pour être sûr (.) tu prends (.) d'autres solutions avec un pH connu (.) et donc si tu prends une solution à 5 (.) et tu prends une solution à 8 et que le machine te donne le bon résultat (.) et toi ton milieu est entre les deux (.) mais il y a des chances que ça soit bon (.) ou alors tu peux aussi (.) vérifier (.) confronter le résultat de la machine ((snicker)) avec une autre machine (.) par exemple une autre marque (.) ou alors une autre façon de doser l'élément que tu veux doser (.)” (line 6718).

In a very similar way as Dilara's, Gaelle expressed that she moderately trusts machines. She said that she does trust machines as they are, but that she does not always trusts the results: "u:::h (.) fff u:::h the machine yes but some the::: you can see that the results seems to be u:h (.) con not front but euh bizarre? hhh and" (line 6731). When she completed her account, Gaelle explained what it means to trust machines and not always trust results. Her explanation confirmed to me that she did not understand my point just like group one (Paul, Cédric and Joanne) and unlike group two (Stephan and Daniel). She basically said what it means that our use of the machines is the problem, but not how the machine is designed and whether or not it actually measures what we want it to measure. She confirmed this interpretation, when she said that if there is a problem, we can resolve it, which basically means repairing or recalibrating the machine: "and so::: u::h you can check for example if there is not a problem with the machine and also you cannot always trust (.) if there is a problem we can see: it and u:h try to resolve" (line 6734). I did not want to interrupt Gaelle, until she finished all her explanation, with regard to the cases where she does not trust machines; but she never hinted on that she sometimes doubts that some machines are not trustable, because she does not know how they measure what she wants then to measure. She finished her account with a third confirmation that she only doubts troubleshooting no more: "you can think first maybe I did a mistake in my u::h in my experiment (.) may the machine has some problem so you can look to the machine and u:h try to solve it (.) and if the machine has a problem you can do again the experiment and see if you did a mistake in your experiment" (line 6747). Once I was sure that there is a discrepancy between my understanding of "trust' and hers, I explained more by saying: "ok actually I meant something else (.) uh I meant uh do you (.) are you sure that the machines measure what you want to measure? Are they really measuring what you want to measure?" (line 6751). Gaelle did not get my point again and said: "I don't under so if (.) if the machine?". She did not finish the word "understand" and restarted the sentence by making a question that we can only understand if we hear it with the used intonation. Through her intonation, I understood her question as "if the machine is what?" And "what" refers to the expression that I used in my question: that they are "really measuring what you want to measure". At this point I explained much more the difference between calibration and the validity of the machine altogether; she then said that she hopes that they are trustable, but again explained as previously: "I hope (.) the m the machine are calibrated (.) so:: (.) normally I hope they give

((laughter)) what you is normally uh (0.3)". Even after my third explanation, Gaelle only talked about calibration and never about the machine's inherent validity.

At the fourth time, Gaelle provided an answer slightly similar to Dilara's, which in fact refers to how to check the validity of one specific machine: "uh for example (.) for the:: the CMA (.) you can make uh like u::h it's meas it's measuring the: the glucose consumption (.) so you can make by yourself a solution (.) with u::h for example 10 milli molar concentration you use the machine and you look if it's giving 10 milli molar concentration or not (.)" (line 6764). From this example, it is possible to check the validity of the machine, because the glucose concentration can be known in a different way, before using the machine. However, this method is only applicable if we are sure that the other way to check the glucose level is also correct, knowing that the raw materials are purchased readymade, which means that their glucose levels are assumed to be true based on the providers' claim (and providers may have used the same machine). Additionally, Gaelle only picked an example of machines, which as simple as the one used by ordinary people with diabetes who want to measure their blood sugar level. When I referred to another machine (a spectrophotometer[51], which is slightly more complicated), she recognized it referring to it using the brand name. I confirmed to her that I meant it and asked if it is a black box type of machine. She said "yes", but then she said that it is possible to know how the machine works using the manuals, again using the "you" pronoun: "Yes (.) because you have the:: you have the reference manuals so you can you know how it works (.) the light will come through uh filter and then uh it will uh read the absorbance (.) so you know basically how it works" (line 6800). After the fifth attempts, I was not able anymore to keep repeating that I did not mean to say how the machine works but rather how it really measures what it is supposed to measure, so I decided to move to the next question. For this case in particular, the question was rather oriented into how the spectrophotometer calculates the "fluorescence", "absorbance" or "salability" that Gaelle said that the machine is able to measure.

[51] An apparatus for measuring the intensity of light in a part of the spectrum, especially as transmitted or emitted by particular substances (downloaded from oxford dictionary).

I have now argued that no matter what each participant responded to my question number 22, and even though I initially categorized the participants into three groups (the yes group, the no group, and the sometimes group) none of them has accounted for a method that enables them to make sure that the machine is actually doing what they assume it to be doing. Stephan and Daniel in particular (the only ones who understood my point right from the beginning) were not an exception. Paul was the most straightforward participant, because he acknowledged that he does not ask himself these questions, but he too said that he relied on the providers' own controls. Now, I will move on to question 24, where I will enquire about whether the participants trust the products that they purchase readymade.

Readymade products

In the subsequent paragraphs, I will detail the following: 1) that almost all products and consumables are purchased readymade, according to participants; 2) that all of our participants trust the products that they purchase from providers and that they do not check after them unless something 'unusual' happens; and that 3) any of these products can skew their experiments. The participants initially took 2 positions: this time Stephan will stand alone, because he said that he does his own checks and verifications. All the other participants have right from the beginning stressed on that providers are trustable and that there is no need to check after them. Yet, no matter how our participants' accounts were varied at the beginning (including Stephan's), the discussions revealed that they all share the same position, in practice. Additionally, I wish to illustrate how accounts switch recursively from fact judgments to value judgments and from scientific to common sense reasoning, which was paralleled by a switch from the use of "I" or "we" to the use of "you". Stephan's position was not an exception, even though his initial account was clearly different from all the others'.

"I trust providers"

Paul first responded to a specific aspect of my question (do you trust the products that you purchase from providers?). More precisely, he said that he trusts providers because it is easy for him to check if the needed protein

is in the product that they purchase: “uh uff (.) again we (.) they are products for which it's quite easy to see whether it works or not (.) if you take an antibody (.) u:h it's easy for us to see whether it works or not (.) u:h you do the: the experiments (.) you try to detect a protein (.) you know (.) that protein is there or is not there (.) and you know at which size and some characteristic of the protein so (.) if the antibody does not recognize the protein then you know that the antibody does not work (.) u::h having done these kind of excerce:::: exercise several times we we know that there are company which are not trustable (.) we know that everybody knows that in in science that if you buy an antibody from a company called ((company name removed)) u:h you have uh much less chance than with any other type of companies that the antibody will work (.) so they are less uh reliable” (line 7075). I then told him that he only mentioned a particular case in which he can check, and asked him if they are able to check if the product (a substance) would contain a molecule. At this level, he said that they are not and that they would be completely blinded: “Yeah there there there you are (.) you are blinded (.) you are acting as uh as uh being just prone to believe that uh what you are using is is uh doing what you what it is described to do” (line 7090). Clearly, Paul now realized that “trust” can be about other scenarios, such as the one I mentioned, and is not limited to the case he mentioned. Paul’s last account illustrates the idea that their trust is not scientifically based and therefore represents a big threat to their experiments’ validity. I wanted to make sure that this condition is also applicable to other labs in the world - at least from his point of view- so I said: “and the issue is that that's (.) consumables are used everywhere in the world it's not only in this lab so may be if there is a mistake or a problem it would be everywhere so you wouldn't know (.) nobody would figure it out” (line 7093). Paul confirmed my statement, by saying: “clearly (.)”. Now at least from Paul’s point of view, if mistakes are made in any of the purchased products (intentionally or unintentionally) the labs would not be able to figure them out, especially if they are consistently made everywhere else in the world, because providers distribute their products worldwide. The only mistake that labs can discover is when they are looking for a particular protein, that they know of or that is described in the product specification. In other words, labs cannot search for what they do not know, simply because molecules are unlimited.

Now because labs cannot check the validity of the products they purchase (according to Paul's accounts in the previous paragraph), I wondered to what extent they are dependent on providers based on how much they need to buy from them, in order to be able to do research. Paul responded with: "well we we we uh we do not u:h we do not generate uh (.) many things (.) I think that we we are dependent of 95% on on uh external stuff" (line 7400). So, according to Paul's account being "95%" dependent on "external stuff", which we cannot ensure that they are correct, is not problematic. I asked if this situation of depending on products that we do not know exactly is recent or not; using the following sentence: "and this used to be the case before or (.) it's has always been like that or that's in the recent years it started to be more and more dependent on other firms?" (line 7402). Paul basically said that the dependence on providers has increased, but it is acceptable and that it is even better to trust external products than internal ones, because external providers are professional and more precise. Again, Paul is basing his value judgment on a common sense idea not on scientific evidence or a factual judgment: "well it it it is certainly more and more increasing u::h more and more increasing because with all the limitation you have you have mentioned u:h you may think that a company for which it is the the job to to prepare to prepare medium u::h according to some quality control u:h level (.) you may at the end trust them more than you could trust your own lab in doing this u:h in a very punctual way (.) so yes they are more and more u::h (.) use of external u:h uh product uh clearly uh I think that from the from the cell to the mice you we we we are dependent on the on providers but we can also appreciate the professionalism of these providers so I do not see that as as a real uh weakness in the system" (line 7404). Although, I have not yet made or solicited any value judgment and was only asking about how much they purchase from providers, Paul's account was already oriented into judging this condition. He used two expressions "with all limitation you have mentioned" and then "weakness in the system" to achieve one aim: that this situation (of depending on providers) is acceptable. The expression "with all limitation" serves him to say that he is aware of the situation, but it is only a limitation like in any other research areas, and that even with this limitation, they still know how to cope with it; to avoid skews. Then, the expression weakness in the system is negated: "I do not see it as as a real uh weakness in the system". Although Paul made value judgments without my solicitation, he did not give any reason on why they should trust the providers except that because "according to some quality control u:h level".

Obviously, the control levels are again only made by the providers- which act as judge and judged- and are only about what the labs can know of. And one more time, Paul switched to the "you" pronoun when he referred to this trust: "you may at the end trust them more than you could trust your own lab".

When I asked Cédric about how much he uses in terms of purchased products (question 25), he said "everything (.) almost". In question 26, I asked him to draw me a diagram where he would mention all his current research steps, stating which ones are provided and which ones are prepared in the lab. He did so and at the end he confirmed his previous statement again: "everything is coming from different stuff […] Yeah everything" (line 7762). In question 27, I asked him to account for the situation of depending on external providers, he said that it is a good situation, because in that way, if labs from all over the world which purchase from the same provider, then they can be sure that they are using the same products: "For me it's (.) can be a good thing like the medium (.) it's a really good thing (.) because that means that everyone in u::h the world (.) can use the same media that you are using (.) because if they just buy from the same provider then you buy it from (.) then they are sure they get the same (.) the same medium that you are getting" (line 8035). Basically, Cédric's account took for granted that the provider's product is perfect, and from that standpoint it would be a good idea to use it worldwide. However, if there is something wrong in the product, the whole world's experiments altogether will be skewed and no one would discover the skew. When I told him this idea, he stepped back by saying that some people do not use those products and make their own: "Yeah but some people use different media and some not not not agree why he's using media from ((company name)) some people are making their own" (line 8046). I expected a discussion on whether or not it is a good research practice to standardize the use of a product worldwide when we cannot ensure that these products are in fact as they are described by the provider. Instead of responding to my concern, Cédric mentioned what "some people" would do. Then, Cédric went back to his previous statement (about standardizing) mentioning that it is a good practice that other labs reproduce experiment in the same way and finding the same results, and that this is only possible if labs use the same products: "It's also good (.) but the thing is that is good if you do the experiment you know it's working (.) and someone else wants to redo it someone else then can just use your protocols so just buy the

same stuff and we do it on" (line 8049). What I found to be awkward and counterintuitive is this logic of the need to reproduce the same experiment, even at the expense of accuracy. I explained my point of view, by saying that if different labs replicate a same experiment but using their own products (for example the lab's own medium or trypsin etc.) and find different results then they would discover that the product can skew results; and in this way labs will be able to know for sure if they can or cannot trust these products. I also said that if one lab wants to replicate another lab's experiment, it can just follow the research protocol with whatever details it mentions, and not necessarily replicate the use of standard products. Yet, Cédric insisted that it is a good idea to have "the same starting point". For Cédric, these products that constitute "everything" in their own research are only a starting point. He mentioned the example that if labs around the world use the same medium that they purchase from the same provider, then we can be sure that they started from the same starting point: "No but if you want to especially find it different because you changed the medium (.) it's just that if you use the same medium (.) you pretty sure that'll have the same u:h starting point in every labs" (line 8060). Clearly, the medium for example is not a starting point, because it can change results (as we have discussed in the previous chapter-case 2- where all the participants including Cédric ultimately accounted for its skew potential). Even from Cédric's point of view, his account can only be correct, if we are sure that providers do provide exactly the same product all the time and to all labs, which is not always true; as I will illustrate using Stephan's, Daniel's, Dilara's accounts below. But more importantly, Cédric himself did not provide any factual account to justify why he trusts the providers' products. He rather justified the trust by making a value judgment, although he said that if for example the medium is altered then it would result in terrible consequences, depending on what was added: "Yeah (.) yeah of course if you like uh if you put u::h a form in your medium you will add terrible consequence if you put like I don't know (.) fructose instead of uh 1% of the glucose (.) may be won't have any consequence" (line 7171). Additionally, Cédric made a statement that is similar to Paul when this latter said that they are completely blinded, because they cannot know if the providers would provide substances that are consistently containing molecules that are not described in the product specification document: "It's impossible to know (.) depend of what you're doing (.) depend if the molecule interacts with the pathway studies (.) it really depend on that" (line 7168).

Now, once I was sure that Cédric's position- with regard to how much he trusts the products- was only based on a value judgment, I wanted to confirm my understanding by suggesting that providers may willingly for some reason or unwillingly alter the composition of the products they provide, because they know that labs anyway have no means to discover the alteration. He interrupted me saying: "[why would they do that?" which basically means that he considers that providers have no reason to do; I said "so again again it's a matter of u:h trust?". He said "yeah". I repeated: "Because you trust that they would not add?"; he confirmed again "Yeah (.) but there is no point for them to add a molecule that is not listed in the product" (line 7133). At this moment, I followed his logic and said: "How do you know that there is no point?"; he replied: "For them there is no point (.) it's not like tobacco you can put stuff to get people addicted (.) for the medium there is no point to do that (.) if they put like (.) something inside it and it can be toxic for some cells (.) your cells die with this medium" (line 7136). Clearly, Cédric accounted that there is no reason that would urge providers to alter the products that they sell. Cédric's latter account assumes that tobacco producers can willingly "put stuff" in cigarettes to make people addicted to them, but he did not consider it reasonable to think that their providers can also "put stuff" (to use his words) to make their cells addicted to their products. In this scenario, the result would be that when labs try to buy from other providers, the cells would die and labs would then conclude that these other providers are not as serious as the bigger ones. Obviously, this was only an example that parallels Cédric's idea of tobacco instigated addiction. When I urged him to suppose that there would be an interest by saying: "ok let's suppose there is interest in adding (.) u::h any type of interest (.) it's hypothetical ok? " (line 7162), he then admitted that in this case "it's impossible to know" (line 7168) the consequences. Consequently, Cédric's position clearly is based on a value judgment type of accounts.

Joanne's logic was almost identical to Cédric, she said "uhm yes (.) I have to trust them otherwise ((laughter))" (line 7177). When I told her if the purchased products would be altered by the providers, would she be able to know, she replied: "I won't I won't know about that (.) but you have to use it" (line 7184), hence confirming that she is rather obliged to trust, because she has no more options. In the same sentence, Joanne started with "I" and switched to "you" to justify this value judgment of trust. I wanted to make sure that she means what I understood, so

I told her if the products are not as they are described, what the consequence would be; she said “Yes (.) so usually it should be like (.) the company should have some kind of u:h checking maintenance stuff like that (.) so they should not sell something and they are not telling the truth (.) it's not uh again (0.6)” (line 7195). Joanne confirmed that her trust is based on the idea that “usually” the company should have some kind of checking, but not on checks done by the lab itself. At this level, I wanted to know her position with regard what would happen if it turns out that the medium for example would be not as described; she said “It will be very serious but I don't think I don't think they will do something like that like (.) the government should uh ((laughing)) If there is only big company to sell stuff like that so it should be u:h all all the time check and stuff like that ” (line 7205). So, Joanne acknowledged that in such a case, consequences would be very serious, but she immediately switched again into value judgments by saying that she does not think that such a scenario would happen. Joanne made a trivializing account: She ruled out the core issue, which she described herself to be very serious, preferrig to emphasize what the “government should” do; without completing the sentence. Then after laughing, she went back to the idea that the company should do all the checks and “stuff like that” (line 7208). Joanne was so surprised by my hypothetical scenario that she did not know what to say. She was desperately trying to concoct an answer, perhaps to avoid the discomfort of the dissonance between the two accounts: that she trusts providers and that if providers would defraud them, the consequences “can be very serious”. I asked Joanne what she means by serious, she said “yes of course (.) some results would be false” (line 7212). Because she said “some” perhaps to attenuate the seriousness, I reminded her that providers actually distributed all over the world and some are monopolizing the market and that in this sense, the impact would even be at the global level. She said “yes”. Yet, Joanne preferred to avoid talking about the consequences, and to stress the idea that such a scenario would not happen, by saying again: “I don't think it's possible” (line 7219), without justifying her position. I asked her again why she said that such a scenario would not happen, she made a long account, where she basically added nothing other than that providers “should” check: “may be like when you will have one or two bottles like when they do the check they they slip there is one slip may be you know but but I don't know for ALL the things that they say there is something wrong in it? No I don't think so (.) there's they have to check before they sell (.) so I don't I don't know the law exactly of u:h when you buy those companies what they do

inside their companies and stuff like but logic (.) ((laughter)) they have to check what they are (.) they're selling before selling to other people" (line 7221). At this level, I am left with no doubt that Joanne's position is based on a value judgment that is not strictly scientific, and that her account camouflages the discomfort of making discrepant accounts. Whether the camouflage account is intentional or unintentional, it constitutes a threat to knowledge building. If I expect that such a practice can happen anytime in any context during interactions between scientists of any levels (classrooms, labs, conferences workshops etc.), I would expect that it would constitute a barrier for knowledge to build, conversationally.

Daniel first said that only 30% of the products they use is purchased readymade (question 25). While he was drawing the diagram he realized by and by that it is actually much more than he said, so he kept saying: "A lot of providers ((laughter)) even in this simple u::h experiment" then "ok (.) ok so they are quite a lot ((laughter))" (line 7585). When I asked him if he only discovered this situation now, he said: "Yeah (.) u:hm never thought about that but" (line 7594). Unlike his colleagues, only after drawing the diagram that Daniel realized how much they purchase readymade. When I asked him for the first time to account for the situation of depending on providers, he said: "Of course but u:h I hope we can trust otherwise (.) this would change change a lot" (line 7648). Daniel then confirmed that they do not check after providers and that if the products are not as they are described, the results would be skewed. Then when I asked him again (question 27), he said: "No for me it's (.) it's fine because u:h (.) they gi:ve they give us information about the product we:: we want to buy so (.) we know (.) we know what we're buying so" (line 7897). Daniel latter account justified the situation by saying that providers give them information about what they buy, putting the stress on that they know what they buy. At this level, I asked him: "you know based on what they say?". He said: "((laughter)) yeah but we cannot for everything", which literally means that they do not know. Daniel then explained more why they should trust these products: "in these companies there are: a lot of steps also to check purity quality of the products so" (line 7905). He confirmed that the trust on which he built his position is completely out of his own control, by mentioning what the companies do rather than what he does to ensure that he receives what he exactly needs. One more time, I wanted to make sure that the lab does not do anything to ensure that

the parameters of its own experiments are under its control, so I said: “but it's a problem because if uh you're doing research and things really are not under your control (.) you're not 100% sure that these things are correct”. Daniel again replied based on that the professionals know what to do, and said nothing to show that they can check anything: “yeah yeah yeah (.) uh:: I understand your point but u::h (0.3) yeah (.) I know we are: asking to professionals so:: normally they should provide uh” (line 7921). I then told him: “ok normally ((laughter)) may be they're not”. Finally, Daniel said: “Maybe they're not yeah” (line 7923). Daniel is therefore assuming that these companies are always serious, professional and that they never try to cheat for any reason. Therefore, Daniel’s position is clearly based on a cantilever (porte-à-faux) kind of trust. In other words, trusting other companies, because they are highly reputable or they are known to undertake a series of controls may be acceptable elsewhere, but not in scientific research. To build all one’s research on that kind of trust is too risky from my point of view; particularly that Daniel said that if anything goes wrong in these companies’ products, results “would change a lot”. Thus, it was not easy for me to get to this idea, because Daniel’s accounts were again ambiguous, and I had to ask precise questions and keep digging down, to understand to what extent the lab masters its own research elements and on what basis Daniel’s position is built. Clearly, Daniel’s position is built based on value judgments that are not necessarily scientific.

Dilara said that she trusts these products, but she admitted from the beginning that her trust is rather a must than a need. She started her sentence in French and completed it in English: “Je crois que We have to ((laughter))” (line 7023) with a remarkably higher pitch at the level of “we have”. Then in question 25, Dilara said that 50% of her research is purchased readymade. I told her that all her colleagues said 80% or more, she said that she knows that her case is different from the beginning: “Je sais ah depuis le début ((laughter))” (line 7315). Then I understood that she was actually referring to her previous research (her PhD at another lab). So I told her to account for this lab’s situation, then she said that we are in a rich lab “on est dans un labo riche” (line 7322), which means that the lab is purchasing more than supposedly “poorer” labs, and that labs can function without necessarily depending that much on providers. Then Dilara mentioned an example, where in her previous lab, she used to do operations that are now purchased readymade, but she did not mean to say which is better. All she mentioned was the cost of purchasing, and

even the example that she mentioned is not related to where I wanted the discussion to go- the skew risk of buying what we really do not know exactly. Clearly, Dilara's account shows that she did not get (or did not want to get) my point, and she mentioned the example of the tips[52] that can hardly be a case of skew risks, and which she explicitely described as expensive: "Tu vois les boites de tips? ou tu as les petites pointes là pour les pipettes ((snicker)) et ben nous on remplissait ça nous mêmes ((elle parle de son laboratoire précédent))" ; (me: "ah tu parles de ton PhD là?"); "Oui De mon doctorat (.) on les remplissait (.) on avait euh on achetait des grands sachets (.) de tips parce que ça coûtait moins cher (.) et donc on prenait les tips après les mettait dans la boite (.) tu vois ça aussi je fais pas pareil ((laughter))" (line 7326). I waited until I was sure that all what she wanted to account for was the cost, and put again the question in a general way, asking her to account for the condition that everything is purchased. She then said in French what I translate as: "but you master less, if you buy readymade, you master less": "Mais tu maîtrises moins de choses (.) tu (.) c'est du prêt à l'emploi (.) et donc tu maîtrises moins (.)" (line 7927). Now that I am sure that Dilara is exactly about to account for what I wanted her to account for, I said: "what is the risk?". At this level, Dilara switched completely to the opposite position, by saying what I translate as: "when I say you master less it depends (.) because sometimes it's more precise when you buy": "((sigh)) tu fin quand je dis tu maîtrises moins (.) ça dépend en fait parce que parfois c'est mieux maîtrisé quand tu achètes toi même (.) quand tu achètes tout fait (.) parce que eux ils vont vérifier les vérifications qui sont faites alors que (0.3)" (line 7930). So clearly Dilara stepped back completely from what she had just said a minute before. Now, the word "parfois" in French means "sometimes", and in this case it was used by Dilara to attenuate the complete contradiction between "buying readymade is mastering less" and "sometimes if you do it homemade you master less". The first account was a general condition, the second was a "sometimes" condition that popped up when I wanted her to develop more on why you master less when you buy readymade.

[52] Small, plastic, disposable devices attached to the pipettes. In order to avoid contamination and not to sterilize the pipette after each use, tips are attached to them so that fluids do not get in contact with them. Only the tips touch the transferred or measured fluids. What Dilara said is that in her previous lab, they used to reutilize these tips (obviously after sterilizing them) and not throw them away after each single use.

I explained to Dilara that I wanted her to give me her opinion about the fact that they purchase products that they do not control locally. The discussion became very long, because I was unable to persuade her to tell me her opinion about this situation- for example whether or not it is a risky situation, and whether or not it would change results if providers alter the products. Dilara wanted to speak again about the idea that the providers are more professional and precise than the lab. After a long discussion, I understood from Dilara that this lab buys everything ready. I said in French “so everything is purchased?”, she replied in French: “yes everything is purchased; well almost everything”: “oui (.) en fin presque tout” (line 7999). Then, I wanted to try another time to hear from her about the condition of not being able to check and control what they buy from providers. She here said that it is not a problem to depend on others and that everyone depends on others, because they are not farmers anyway and they are not able to grow things: “oui mais bon on est pas::: on est pas agriculteur (.) on est pas capable de produire des choses fin c'est le (.) tout le monde dépend d'autres personnes (.) tout le monde (.) c'est pas grave” (line 8003). I failed to convince Dilara to tell me about what kind of risks they run if these providers are cheating for any profitability reason, for example. At this level, Dilara seemed to me to have understood my point very well, but again did not want to account for whether or not there is a risk. She made a long account to state that it is possible to visit the providers anytime and ask questions, and she emphasized that providers are kind and open. In the middle of her description, Dilara said that she had a lot of problems with the purchased products in the past, and that the providers were able to solve her problems, by suggesting other solutions, and hopefully she was able to finish her PhD research. This account is clearly contradictory with her previous account when she said without any reservation that providers make neat and precise products. She ended her account with the idea that she prefers to think that people are nice and therefore not expected to do not cheat: “Si uh on peut demander pour aller pour aller visiter et cetera fin (.) dire c'est pas il faut pas voir les firmes comme quelque chose de fermé fin c'est pas quelque chose de:: (.) c'est des gens qui sont accessibles (.) ils veulent bien te donner (.) si tu leur demandes de faire un test supplémentaire il faut un test supplémentaire (.) fin c'est euh il y a moyen de discuter avec ces gens (.) tu vois si t'as des résultats un peu un peu bizarre (.) puisque j'en ai eu (.) j'ai eu plein de truc bizarre (.) eh ben j'étais en contact avec eux ils m'ont conseillé tu peux faire ça comme test renvoyez nous on va essayer avec un autre mode on a eu ça comme résultat et vous? Et

donc finalement (.) et je suis toujours sur mon idée de base que: les gens en général sont de bonnes compositions ((laughter))" (line 8008). I replied in French "and did that solve your problem?": "((laughter)) et ça a résolu ton problème quand" ; she said "yes oh yes yes yes thank God and I was able to finish my dissertation": " oui (.) mais oui oui oui oui dieu merci j'ai pu terminer ma thèse ((laughter))" (line 8018). Consequently, I failed to urge Dilara to give me her position with regard to the condition of trusting the providers without checking, but I was able to make sure that her position is based on a value judgment: that providers are professionals, that they do not cheat and that they are kind and welcoming. During the discussion around question 27, I said in French so you base your opinion on the idea that providers do not cheat; she replied "yes": me "tout ça en se basant (.) en disant que (.) fin les autres ne trichent pas (.) que il n' y a pas d'intérêt pourquoi il s doivent tricher (.); Dilara: "oui" (line 7933).

Gaelle's position was very similar to Dilara's, but Gaelle was very direct. She said that they trust providers because they have quality controls: "Yes (.) otherwise we will not ((laughter)) use them (;) and they have to::: take uh check so they have quality control in industry so:::" (line 7028). She added: "but they have quality u::h quality test and quality u:::h they have good manufacturing procedures ((laughter))" (line 8030). She also said that they depend on providers because they cannot do everything by their own: "Yes (.) we do the::: without the: company we can't do a lot of things" (line 8025). When I asked Gaelle whether they are able to check if providers would alter the products, she said: "No we don't check u::h". She then said: "uhm (.) but since we cannot check (.) we will not (.) we are not able to check ((laughter))" (line 7044). I asked if providers are able to totally know their own products, and if they do would they tell everything to them; she said: "Yes but if they know what they put in (.) they will (.) it's u::h a nonsense to not tell uh (.) the people (.) who buy it so (.) there is no reason to tell you that it's the wrong uh" (line 7047). Because her account was somehow ambiguous, I gave her a specific example about a provider who is willingly adding a molecule to the product for other reasons, and asked if he would tell them; she basically said no: "But what they will not uh tell that this molecule is (.) there is no::" (line 7052). Consequently, Gaelle's initial position was also based on one value judgment idea: that providers would not makes mistakes and would not cheat.

But unlike Dilara, when I mentioned a specific example, Gaelle was direct and short. She admitted that this is possible, without necessarily changing her position, because she did not say that this possibility is a reality either.

"I don't trust products"

Stephan, who was initially more skeptical about the purchased products than all the rest of the participants, said that although he generally trusts providers, he still checks the correctness of the products, in person. Regardless of its veracity, Stephan's account enables him to appear as a careful and rigorous scientist: "u:h y:: (.) in general I trust them (.) but I also perform some of my own checks to (.) evaluate their them being correct (.) and at times I also find that (.) may be they were not as they should" (line 6992). Stephan's account "but I also perform" literally means that his own checks are done on a regular basis, but once I told him "you sometimes check?" he switched into "sometimes" himself then he immediately focused on a case: "At (.) sometimes I double check things (.) functionality like now (.) I also have something ordered (.) I didn't like what I see and I contacted the company like I think may be something wrong (.) can you explain me why I see what I see?" (line 6996). I first thought he was talking about a general situation, so I asked him "what do they say?". He said "I'm now waiting for thehhh ((laughter))"; now I realized that he was talking about a particular case, which I do not know why he picked it if it is not yet understood as a case. So, I first understood that Stephan does his own checks regularly. Then, he said sometimes, and finally he picked one case that is not complete to explain how providers make mistakes. More importantly, Stephan said that he asked the provider to give him explanations on why he sees what he sees and that he is waiting for an answer. If Stephan is able to find errors in the purchased products, he would not ask for explanations, but rather reports them and asks for reimbursement or makes new orders etc. At this level, I doubted that Stephan does not understand "checking" as I understand it. He probably was more interested to have products that are similar to the previous ones, in order to keep the cells in the same conditions, not to risk skewing results due to a change in a parameter halfway through the experiment. So, Stephan was rather referring to a very specific thing that he can check; it is whether or not the product ordered this time is identical to the same product ordered the previous time. To ensure that that was all that Stephan was referring to, I asked him again in general about the risks that they may have if mistakes happen.

He said: “yeah the r:: the risk of that of course is like if u::h you're assuming that something is like one batch to the other batch should be the same but you're just assuming that this indeed correct but (.) there is a possibility that this is not (.) and sometimes you might have studies have like (.) you get variations because of the inconsistencies in the product” (line 7241). Now, although Stephan’s account contradicts group one’s (who trust providers and say that they do not make mistakes), he still only referred to a particular risk, which is the product inconsistency. Yet, what if the previous and the current ordered products are identical (and so consistent), but are wrong? Stephan, so far has not accounted for this, and I assumed that this does not make part of his concerns. At this point, I had to make sure that that risk exists and that his checks do not include to test if the product is correct or not, regardless of consistency with the previous order. Finally, Stephan confirmed my understanding: “Yeah exactly (.) u::h so for sure there is indeed u:h that is risk for that and (.) to some extend we cannot really avoid it but u:h (0.3) there is (.) the only way that we have to check is by (.) I think maybe other people doing similar kind of experiment but may be using them from different manufacturers and then see (.) but yeah it's it's an inherent risk” (line 7251). Thus, Stephan who’s first account reflected his rigor and carefulness, now admitted that his own checks do not enable him to know that the products that they use are correct and that they run risks anyway.

With regard to how heavily the lab depends on providers, Stephan said: “it's indeed when you think about that it's u::h (.) probably like 80% or something of the stuff is actually ready but we mostly do is (.) combining the different things in different settings to have a different conditions and just (.) see what happens (.) what basic sources originally might happen yeah (.) already pre poc pre made by the providers” (line 7235). This means that the risk that Stephan referred to (in the previous paragraph) is applicable to the 80% of their research, which is not under their own control (because it is provided readymade). At this level, there is no more difference between Stephan and group one, because the checks he referred to at the beginning turned out to be only limited to one condition: the consistency of the products from one batch to another. It is only through further questions and discussions that Stephan accounted on the risks that they run because of the situation of not checking other things. Stephan called this situation “an inherent risk”, although his very first account (question 24) reflected a completely different position- where things

seemed to be under complete control. This is obviously a pertinent example on why fully structured questionnaires do not suffice to understand issues like these, no matter how significant the number of participants is. It is only through detailed discussions and discourse analysis that I was able to find out that Stephan was initially playing down this "inherent risk" and playing up the idea that things are under control. Furthermore, only after asking Stephan to draw me a diagram to cite all the products that he needs in his current research, did he realize how much they depend on external providers. He said: "oh it's a lot ((laughter))", which suggests that he discovered this situation for the first time. Then, when I asked him to account for this situation (question 27), he said: "No it's again crazy it's so many parameters for things that can go wrong" and immediately justified this situation by saying "but it's (.) I think it's one of the only ways because you cannot control (.) you have no time effort options to get all these materials you cannot create them yourself" (line 7841). By doing this, Stephan is again playing down the risks of what he himself called "so many parameters for things that can go wrong" by justifying this situation with the impossibility to produce these product by themselves. Literally, Stephan's account means that working with "so many parameters for things that can go wrong" is acceptable, because we cannot manufacture these products at home. Additionally, I did not suggest or ask whether they should try to manufacture these products locally. It was rather his own alternative that he deemed to be impossible by himself. Stephan's account harnesses my thinking to only see two options; one of them is not possible: Either to accept the risk of getting wrong products that is used in "80%" of the research, or to think about manufacturing everything, which is not possible. Hence, with this way of putting things, I was not able to imagine that there are other alternatives. Based on the risk that Stephan has just described, I am enquiring why cancer research labs would not consider finding other feasible ways to make sure that all they get from providers is definitely as they want. Stephan is therefore trivializing what he later called the "inherent risk" of not knowing the real composition of "80%" of what they use in their own research.

3.3.10.2 Synchronic inconsistencies and contradictions (case 3)

In the previous subsection, which was focused on the variation of accounts across persons, I discussed how the lab excessively entrusts to providers. In this section, I will discuss how the lab omits to entrust to other parties, although these parties' contribution is crucial, according to the participants. While the former concern aimed at discussing the lab's trivialization regarding the potential risk of leaving a significant part of its own research out of its own control, the latter concern will aim at discussing the trivialization of the potential risk of ignoring other specialists' contribution. Therefore, in this subsection, I will build on the participant's views with regard to the vital need of doing cancer research along with other specialists (clinicians in particular). More particularly, I will discuss how cancer research practiced solely from a biological point of view (like in this lab) is insufficient to understand cancer and cure it (again, from the participants' points of view). As I did in the previous two chapters, the discussion in this section is more oriented to individual inconsistencies, although I will from time to time compare between the participants, for the sake of better understanding of the issue.

Questions 28 through 30 aimed at understanding the following 3 concerns that emerged from the observation, as I explained in the background section: 1) how can a cancer researcher say that involving other specialists right from the beginning is rather a must not an option, to be able to understand cancer, and at the same time keep designing and doing his or experiment without them? 2) how can a cancer researcher keep studying cancer at the cell level first and strictly from a biological point of view, and at the same time keep saying that when we move to in vivo (mice or humans) we are often surprised that "things do not work in the same way as we expected in vitro?"; and 3) how can a cancer researcher say that interactions are irreplaceable and unique in knowledge construction, whereas these interactions are very limited, in practice?

In the subsequent paragraphs, I will analyze these 3 concerns, illustrating with examples each participant's oscillation between the two paradoxical positions, in each of them. More particularly, I will analyze how each participant trivializes the importance of the same issue and how he or she detrivializes it, depending on the context.

The trivialization in this case is about the pitfalls of the compartmentalization of cancer research. Because the participants have significantly accounted on the major drawbacks of their current research model and on the tremendous need for a more multidisciplinary environment, I found it quite inconsistent to keep doing things in the same way that we criticize. While I understand that individuals do not necessarily have the power or the means and ability to change what they criticize, my analysis will nonetheless try to understand to what extent the participants support and reinforce what they themselves criticize, at least at the level of discourse.

Contradictory but consistent positions

This time Stephan and Cédric will take two totally contradictory positions, but they will remain consistent (i.e. faithful to their respective positions). All the other participants initially started from a position with regard to the 3 concerns, but later explained things in inconsistent and contradictory ways. Stephan, who also agrees with much of what has initially been said by most of his colleagues, was consistent with regard to these 3 issues, and from this standpoint, he was the least controversial. Stephan said: 1) that cancer research labs (including his) are not open to different specialists and that this condition is literally handicapping much of their research. He admitted that this condition is significantly problematic and unlike his colleagues did not defend this situation by any means; 2) that cancer needs to be observed at the patients' level, before designing experiments on cells and all along the in vitro stage. He admitted that this is not the case in practice, and that this is why experiments on cell lines level fail later; and finally 3) that daily interactions and close collaborations with other specialists are a must and not an option, to be able to construct knowledge. He admitted that this is not the case in this lab and that this is a problem in terms of knowledge building.

In answering question 28 (whether he thinks that cancer research labs would need to open up and involve other specialists), Stephan said: "I think they should do more yeah" and "Oh there is a BIG need of that" (line 8108). I told Stephan that one of his colleagues said that there is no need to involve more specialties at the in vitro level. I referred to a case (during observation) where Sophie told me one day that other specialists (clinicians) would

interfere later at in vivo levels (mice experiments, toxicity and the different drug administration steps). She told me that biologists anyway read about others' contributions and would not need to work along with them all the time. Stephan Responded to this idea by saying that later stages would be too late and that close interactions with other specialists is a need right from the early stages: "No that's for I think for (.) to some degree ok (.) but it's (.) one of the things for example if you have no like ok (.) I've done a compound screen (.) and you think like ok I want to study this (.) before you (.) maybe if I did much experiments you thought ok it works for this model (.) I want to do it further (.) you might have the problem that this compound is really toxic and things but you don't maybe have the expertise (.) so maybe it will also already good to: (.) have some content with uh I don't know ( ) doctor or somebody in medic (.) or who can see about this toxicity (.) give you feedback like (.) this one is probably not going to be a problem (.) or they say no that really is u::h likely going to kill you because then you can say ok (.) may it works in here but (.) I can save myself the trouble because it's not worth studying anyway because we cannot put it into practice (.) because it will be stopped half the process later" (line 8116). During the same discussion, Stephan mentioned another concrete example where he incidentally discovered that many of his colleagues design their research in a wrong way, because they lack the background that Stephan has: "we should a:sk and get advised from outside people may be that we don't have expertise on (.) sometimes you can u::h you can't see things (.) I've also seen many people in here working on things but just by chance I saw them working on something I was like I think your approach is wrong you seem to be doing this and this and this but you're also doing it wrong (.) you you you're wasting your time (.) that's it's because my bio (.) if I've a biotechnology background and very different than (.) while people here are like from pharmacy or they don't have some of my uh (.) experience" (line 8136). Stephan's account basically details the idea that many pieces of research can be designed in a wrong way or simply be a waste of time, due to the condition of not working with other specialists right from the beginning.

I asked Stephan to account for the condition of studying cancer at the cell level first and then move on to bigger levels (question 29). He said that it should not be enough to study it in that order: "Uhm:: (.) I don't think I don't I don't think it's should be nice surely to just study it at ((coughing)) the cell level" (line 8289). I told him: "but

that's the case", because I know that at least in this lab, that is the only way how it works. At this level, Stephan provided an account that no one else did. He basically advocates for the idea of starting cancer research from real clinical cases (example a real patient), understanding how cancer affects the patient's body and then get back to labs to design experiments based on a relatively holistic understanding- unlike the research which starts from experiments in vitro and later on try them in vivo: "I think we've (.) like (0.4) it depends which stage you are in because you can also I can also imagine you are in the clinic and there is patient coming with cancer and they thought oh it's always mes mes me::h you get a metastasis and it's going to the liver and why is it going there? let's start looking at the body first and you find that what were cancerum (.) in the human and then isolate it and then go to cells but maybe sometimes it's better to start we start with the human because if you know if the human body ok these cells are located there and there they cause the problem (.) if you know about the whole system you can already some ideas about the more ideas about the context (.) the topic you have to first studying (0.2) may be you had a more focused approach" (line 8292). Clearly, Stephan's account does not reflect what is being in force in this lab, where experiments only start from in vitro (on immortalized cell lines[53]). I told him in a clear way that his account does not reflect how it actually works in this lab; he admitted this and added that the way they are doing research right now is causing them to go too far, due to not being able to understand how cancer works in the body (i.e. not just in cell lines): "[Yeah to to to some extent it's correct indeed (.) it's like uh because they already know it's cancer so they've made models of this cancer but it's sometimes maybe uh sometimes may be good sometimes it might not (.) maybe the models might not good because it's the cell model and (.) we also experience that here we u::h (.) sometimes we go too far from the natural environment" (line 8304). I wanted Stephan to account specifically on the approach of studying cancer from the smallest level (the cell) to the biggest level (the whole body); he said: "Yeah it's also sometimes I experience also there is (.) we're not doing the correct thing" (line 8320). Stephan further detailed the idea that the current research design is perhaps leading to a dead end, because they assume things at the in vitro level, based on how cancer behaves at the cell level; and only think about how it would behave in the body at later stages, when it is too late: "I also found like for example like we're doing culture of colon cells where the way we're

[53] Cell line: A cell culture developed from a single cell and therefore consisting of cells with a uniform genetic make-up.

culturing them is not representative for the body (.) so then yeah of course (.) you're finding something which is just opposite of what is happening in the body (.) and I think it would be::: sometimes better to have more interaction between biological::: like bio people and medical people" (line 8322). And one more time, Stephan critiqued the way their research is modeled, in particular how they culture cells and skew results by assuming things that do not reflect the reality in the real body. Stephan's following account will further illustrate that this compartmentalized way of doing cancer research is misleading: "for example here we're culturing cells in a medium at pH 7.4 or something like that (.) it's not (.) but the problem is like we're studying also colon cancer (.) and the colon is lower pH it's more acidic and yes and here they say well if it's more acidic it's more like in cancer because in a cancer it becomes lower pH (.) but in colon it's just (.) it's normal for the colon cells to be in this way so if you see something that is changed by lower pH it's not because of cancer (.) it's because (.) being less cancer (.) because it's more normal for this cell (.) so it's (.) sometimes like the model: what people use if they don't think about the whole context of course they are doing exact the wrong way" (line 8332).

Finally, Stephan was very sensitive to the idea of the need for interactions more than anyone else (question 30). He not only said that multidisciplinarity and diversity are important, but that frequent interactions in particular are irreplaceable. He stressed that everyday interactions with other specialists in particular, cannot be replaced simply by establishing collaborations or meetings from to time to time. Stephan basically said that their research should be designed collectively right from the beginning and not wait until later stages to get other people's feedback, because it would simply be too late: "I think you have to be more:: involved already in earlier stages (.) u::h like for example I was in the USA there was (.) when I was working in the cardiac sciences there was on a weekly basis every week there were people presenting (.) one day it was a clin one week it was a clinician the other day it was a clinician the other day it was more fundamental basic science u:h biologists (.) and they were:: presenting data (.) every week there was someone presenting it was talking about studies (.) and then like clinicians in biology they give (.) they share their ideas and that time together ↑ and it's much more regularly and you can get much more faster feedback (.) if you have much more people contact (.) there's much more:: they were forced to work together (.) for example I

think here it would be also a good idea than it has to (.) have more joint meetings for example the research group here and may be some doctors from the hospital" (line 8585). I pushed the discussion with Stephan further to better understand his position regarding whether interpersonal interactions simply get people to exchange knowledge or build knowledge. Giving an example, Stephan was an advocate of the idea that knowledge is rather created during discussions: "[It is created because yeah that's also whenever that does meetings sometimes they give them comments they say no but it is like this and then this is the way we::: see this mechanism they say oh but maybe it's because of that and then at that spark they came up with an idea then maybe we should do like this (.) yeah if you use this and this method you can may be see this and then they have new ideas for (.) things and uh" (line 8599). When I told him that his colleagues actually said that it is no problem not to be working closely with other specialists, because they will later on share their knowledge, he said: "no but it'll be too late" (line 8611). Stephan added that the lack of collaborations and interactions is causing them to be doing the same mistakes by different people over and over again: "so I'm more for collaborations and more interactions I've seen it many times before we're reinventing the same thing over and over or going in the wrong way" (line 8617).

Unlike Stephan, Cédric was at the other end of the continuum. Although he agrees to the idea of the need for multidisciplinarity, Cédric (whom I interviewed on his third day since he joined the lab for a PhD research) said: "yeah it's already:: what we are doing here (.) are different type of people biochemists working with chemists working with pharmacists and there is doctor and (.)" (line 8256). Cédric's position is dramatically different from Stephan's and also with all of his colleagues' initial accounts. I asked if he is satisfied with how it works in terms of diversity and real collaboration; he said: "No no yeah we have collaboration like (.) for example in one of the projects of:: Brian and Paul they are working on one compound the compound have u::h ( ) by chemists and there is a lab here (.) and they are doing some ( ) in vivo experiments other people are doing the physical experiment on the molecule and so on" (line 8261). Clearly, Cédric's example has nothing to do with the type of diversity and the multidisciplinarity that Stephan referred to above; and also with all the rest of the lab members, as we will detail in the subsequent paragraphs. Cédric's example does not reflect any diversity in terms of the researchers' backgrounds.

He only mentioned how the lab does two different types of research. I wanted to ensure that Cédric is really satisfied with how it works, so I put the question very directly by saying: “so it's u:h it's well done now it's already?”. He replied: “It's already done in some part of the lab (.) also in project you cannot always do that for everything but” (line 8266). I put the question in a different way: “So for the time being it's good it's good it's working well like that? Right?”; he said: “yes”. I then decided to move to the next question (question 29) to understand his position with regard to doing cancer research on cells first and then move on to animals and humans. Cédric’s accounts will again be dramatically different from all his colleagues. He said that they are already doing in vivo and in vitro at the same time, whereas all his colleagues insisted many times that in order not to kill too many mice, they ethically cannot do experiments on them unless they succeed at in vitro. This has been said many times all along the 3 three interviews: “Yeah of course but you always do: you don't always do like (.) like that I mean sometime you start both in parallel and you start in vivo project and in vitro so you work on cells and on mice or what's ( ) and then you can from the (.) from the cell you can get some result that are more basique (.) for the mice you get more complicated res result but the the one on the cell can appear to understand uh more complicated results” (line 8491). Additionally, Cédric said that they are working on biopsies too, whereas all his colleagues said that this is not the case (as I detailed with Stephan’s accounts above, and as I will detail more with the other participants’ in the subsequent paragraphs): “You can also work from biopsy and stuff like that yeah (.) so directly from patient” (line 8518). Interestingly, Cédric switched to the use of “you” again whenever his accounts are rather groping or hypothetical and not referring to a real case. However, in his initial account where he referred to a real example, he used the “we” pronoun. Finally, I asked Cédric question 30 with regard to the need for everyday interactions especially with other specialists. Again, contrary to all his colleagues, he said: “May be it's hard to have daily but monthly or a couple of months reunion or every few time” (line 8721). I said “a couple of months?” to make sure that he meant what I understood, he said: “Yeah it can be (.) depend on the project but sometimes it's not (.) like the clinical stages totally different from pre clinical stage (.) so you're not trying the same stuff and not (.) observing the same stuff” (line 8726). Cédric’s position entails that there is no need to think collectively around research issues and build knowledge together by and by. Knowledge building is here accounted for to be an individual activity, and individuals can meet from time to time

to exchange some information and share data. I asked him about the need to be in contact with physicians, because they know the human body better then biologists and can help research designs at in vitro be closer to in vivo. He said: “Yeah certainly (.) I mean like I have a committee and inside the committee you have physician (.) so if like during one of my committee I say ok I will try may be to use that in my next experiment (.) may be going to tell me that there is no point because if you do that (.) you'll never go to clinic with that it's:: already a toxic compound” (line 8733). Again, Cédric made it clear that it is not necessary to work closely and daily with physicians, because it is just enough to have a physician as a committee member, who will correct things at the right moments. Cédric confirmed that knowledge is not something that we construct together, and reduced it to the level of information. I told him that in his case maybe it works and wondered about the cases of the postdoctoral researchers who do not even have committees. He basically conformed for the third time, that it is enough to call a doctor and take his or her opinion on this or that matter: “then you will look up if the com if this compound is already: u::h known toxic compound or some stuff like that (.) or you can just call a physician and ask him (.) may be he's going to tell you yeah I know I don't know uh look it up in the bibliography” (line 8739). Thus, it is now very clear that Cédric’s position is totally contradictory with Stephan’s, with regard to the following: 1) cancer research labs are diverse and multidisciplinary enough and do not need more openness to other specialists; 2) biologists working on cancer research do not only start from cells and go up to the human body. They start in vivo at the same time as in vitro and can also work from biopsy; 3) there is no need for daily interactions with other specialists. A meeting every two months would be enough and if one needs a piece of information; he or she can call the right person and have the answer.

Inconsistency on a continuum

Now between Cédric and Stephan, the other participants will considerably vary on a continuum of inconsistency and contradictions. The contradictions, which are in this sense quite explicable from the cognitive dissonance point of view (Festinger 1957 and 1962), will be minimized using different linguistic tools. Paul (lab director and founder) disagrees with Cédric (a PhD student on his third day in the lab) that the lab is diverse enough. He said that multidisciplinarity would help them achieve better results. He accounted for the need for more openness

to be able to understand cancer from different perspectives. However, Paul is also in contradiction with Stephan. While Stephan accounted for the need of regular and face-to-face interactions with other specialists, giving past successful examples; Paul said that collaboration and meetings from time to time would be acceptable. After mentioning 3 examples of how cancer research involves different specialists and should not be fragmented, he started to account for the need for interactions, but said that there is no need to work together all the time: “Yes at least this is the: is the (.) the multidisciplinary uh uh approach which is certainly uh the one to go if work with chemists if we work with engineers is because we cannot handle the job ourselves and that we uh u:h we learn from them and they learn from us […] so yes definitely we need to have more more larger mixture of of person in the lab (.) either physically but also uh may be uh through collaborations I mean you you if the collaborations are facilitated (.) I've no problem of uh of uh the people being uh contacted but not being here physically present in the lab I think it's sometimes but people think about (.) which is too much to to imagine having uh all people together in the same building (.) ok it helps if you have only to go once one floor below but but uh to work on the on the way to connect people even though they are not physically nearby I think that this is something which could be more easily reached and easily reached” (line 8224). Paul’s position shows that he is for diversity and multidisciplinarity, but not necessarily to work physically in the same place. Although Stephan had said that it was possible in his previous lab in the USA, Paul said it was too hard to imagine this way of working together. Perhaps, from Paul’s point of view it is hard to imagine labs as focused as his own one to be able to orchestrate many specialists in the same projects. If cancer research would be completely restructured, and especially not fragmented into pharmacological, biological, clinical and more specialties; it would be possible to imagine more comprehensive, organized and synergistic projects to be conducted collectively by all parties in single step and not in sequential order.

In responding to question 29, Paul detailed the limitation of their approach (studying cancer at the cell level), but justified it with the simplicity (because the cell is the simplest learnable entity). For Paul studying cancer from cells and up is simpler, but during his account Paul said that this simplification is not necessarily the right way or not necessarily similar to how cancer behaves in the body, which is why they are using it in a “smart” way: “you need to

uh simply the system but uh using the the (.) the most uh (.) most simplified model which is the cell the cancer cell (.) but seriously now we try to to uh to use this simplified model in a smart way (.) the first consideration is to know that we are working on a restricted model (.) on a simplified model and that there is (.) still a lot of things which need to be validated first in animals before the clinics (.) and the second thing is to try to make these model less artificial as possible so to consider uh the pH to consider the access to oxygen to consider the the amount of nutrients (.) so we need models but we need also at the same time we need to know the limitation of the model we we used (.) but we could not work directly on the on the on the tumor biopsies from the patient to understand the disease" (line 8433). In his explanation, Paul basically reinforced the idea that cancer cannot be studied unless it is validated by many other specialists. Although, Paul started his account to justify this model, he realized halfway through his explanation that their model cannot stand alone, but is rather dependent on others; which is why I insisted on asking about this fragmentation. I did not understand the incongruity between studying cancer in a fragmented way (starting at the cell only and then move up to higher levels), not involving other specialists (clinicians in particular); and keep saying that this way does not enable them to understand the whole metabolism. Stephan admitted that this is "perhaps not the right way". Paul used another strategy, which is putting the emphasis on that research is going into more holistic approach, that they are improving and learning how to correct models by and by. When he mentioned that all cancer research is like a chain that starts from basic research as theirs and ends at clinical research (the patient level), I asked if their own research builds on the clinical one to ensure that they are closer to human bodies, right from the beginning. He said that that what should ideally be done, but did not say if they do it or not: "you know that's that's the the the optimal way (.) it should be like that uh knowing that for the last three years there is an increasing uh models of uh of uh biopsies of patient tumors so taking a little bit of the patient tumors which to directly insert the small biopsies of tumors in the mice in a mouse in order to (.) to have a mouse developing exactly the same cancer as as the patient (.) and if you do that on several patients including some responding and not responding (.) you may develop the cancer in the mouse and then you can extract the cells from these mice and have again the the the model to understand why this one did respond in a patient and this one did not respond and to know again quite the kind of uh the very basic research to to uh to identify the pathway which make a difference to be back to the clinics and say

ok the (.) we can now consider that these biomarker is a good one to determine whether the patient will response or not (.) and for the patients who are not responder it doesn't make sense to treat with this compound but to choose another one (.) so uh the multidisciplinary approach is necessary and it has to go in in in the different direction to to to find to work with uh specialists in other fields which allow to better understand your basic research and clearly the clinicians in order to uh to have your feedback about how the the drug which is derived from your experiments it work or how it we could improve its use in the future (.) so I think it's kind of uh uh feeding itself" (line 8464). Paul account mixed the "you" and "we" during the explanation of the same idea. Whenever he referred to something that they are not doing, he used what I have already called the tentative or groping "you", but whenever he referred to something that he knows for sure, he used the "we" of proud. Bottomline, Paul did not answer my question (i.e. why they do not do what he said to be the best practice), but diverted it into what should be done. The diversion was correlated with the switch to the "you" pronoun.

Daniel put the two contradictory ideas in the same account. He stressed on the need to understand cancer collectively, because biologists alone would not be able to understand it without taking other knowledge about the disease into consideration, but immediately said that they do not need to necessarily work together in the same place: "Yeah yeah (.) yeah it will be nice (.) because u:h as (.) as we said at the beginning cancer is very complex (.) so there are different approach to:: (.) to fight cancer (.) yeah if we can get uh ideas from clinicians (.) epidemiologists and all these people working together it will be uh (.) it will be better (.) not necessarily in the same lab but (.) make kind of collaboration and I don't know every:: two three months having a discussion all together (.) I'm not sure that u::h every day we need uh necessarily to (.) to discuss always it u::h" (line 8150). Like Cédric, and unlike Stephan, for Daniel knowledge is a matter of information exchange that we can do through discussion every two or three months. Daniel again reinforced the idea that he is not sure that he would need to be acquainted with other specialists, although he said he needs their knowledge. Perhaps, Daniel sees projects in their current designs and did not try to image bigger projects. I mentioned the idea that being acquainted with other specialists would create an environment that is more diverse and that everyday interactions would cause novice ideas to emerge and knowledge

to be built faster. He said “yeah” and continued: “But I would say that already in the lab we have some doctors so we can u:h we can already discuss about theses points (.) so it's already u:h kind of av advantage (.) so for us” (line 8169). I told him that there is only one doctor who is doing her PhD and who is in fact not even in their own lab but in the other lab that is sharing the space with them. Additionally this doctor is constantly focused on her own dissertation and is almost never involved in other people’s projects. I have never seen her attend any of the lab meetings, and clearly her presence there is accidental and anyway not decided by this lab, simply because she was recruited by the cardiology lab. He said: “Yeah yeah so in our team there was Nairi who now left but who was also doctor” (line 8174). Nairi was also a cardiologist who was in the other lab and who already left. I told him “so this is by chance?”, he said “yeah”. Bottomline, Daniel’s account deviated the discussion from the need to study cancer in a more holistic approach into saying that they “already have” that diversity, which turns out to be a single example that is incidental and not even in their own lab.

I moved to question 29, where I asked Daniel to account for the logic of studying cancer at the cell level and up. In responding to this question, Daniel was initially categorical by saying that cancer is a matter of cell proliferation, and that the cell is the entity to study. For him, studying the smallest entity would enable him to understand the bigger picture: “because (.) these are the cells which are responsible for the cancer (.) o we need to understand (.) what it's happening in the cells (.) this these cells are the source are the source of cancer (.) so::: yeah we need to study to study them to understand the cancer” (line 8342). I told him that maybe this is his own perspective as a biologist which is not necessarily that of the clinician who would want to look at the bigger picture. He restated his previous idea, which is basically that cells are anyway the entity to study: “Yeah but at the end we know that (.) these are the cells (.) u::h which forms the tumor which are responsible for the progression and (.) of the cancer so (.) this is the entity of the of the cancer” (line 8349). At this level, I told him that maybe cancer as a disease is a bigger issue, and mentioned the logic of Gestalt psychologist Koffka (1935), which is that “the whole is something else [he did not use the word “more” and not even the word “other”] than the sum of all the parts” (p 176). I added that some physicians are also doing research on cancer, but are looking at the whole body not at cells. At this

level and after he had already argued that cancer has to be studied at the smallest level, Daniel now admitted that this is another way of doing cancer research, and that cancer is not only a matter of cells: “Yeah yeah (.) some people are working directly on the (.) patients or:: (.) uh uhmm (.) I'm not saying that (.) one option is better than the other one I think it's quite u:h complementary (.) approaches so uhm for sure we need to understand (.) what is directly (.) in for patients (.) but we need also to understand at the cellular (.) so (.) ” (line 8361). I told him that maybe biologists would need to see the bigger picture from time to time; he said “Yeah but the opposite is u:h is also true” (line 8369). He added that in order to understand the bigger picture we would need to focus on the smallest part too: “to understand it at the::whole level (.) it’s also need to understand what is occurring at the cell level (.) so” (line 8371). This discussion brought us again to the need to work tightly (the bigger picture researchers along with the smallest picture researchers). I asked: “so there is a need to work tightly?” (line 8373); he said “yes” after he had already said that they would only need to meet once every three months to exchange ideas.

It was the best time to ask Daniel the question 30, where I solicited his opinion with regard the role of interaction in knowledge building. Surprisingly, Daniel who had initially and spontaneously said that they would not need to meet with other specialists all the time and that a meeting every three months would be enough, will now change of position: “u::h (.) I would (.) I would say that ( ) interaction is u:h it's better (.) or mo::re (.) useful most appropriate way that uh (.) yeah we cannot replace interaction between uh between people to (.) to discuss” (line 8643). This major shift from his previous statement, made me curious to know how much he means of what he said. So, I said that some people might think that it is enough to share ideas through publications and readings. He responded to this idea with “no this is not my point”, as if I said that I meant him by my statement, which does not insinuate that it applies to him by any means. I only said “people” and mentioned ideas that he did not say before. Yet, when he continued his explanation, he did not refer to daily interactions as an important factor to collective knowledge building as I have expected, but rather to “present” “data” and collect “feedback” to be sure that they are “in the right direction”: “no (.) this is not my point and that's why also we: try to (.) to present the data in (.) meetings so in congress and seminars (.) to have the feedback also of (.) other people to: to be sure that we are in the right way

or may be also to (.) yeah to" (line 8649). Thus, although Daniel used the expression "this is not me", he made an account that reinforces his initial position with regard to the role of interaction in the knowledge building process. Finally, I decided to ask Daniel if he can recall an example, where he personally learned a specific matter from interpersonal interactions in the lab or elsewhere, and which would not be possible to learn otherwise. Daniel responded with an example, where he did not mention what he learned but what he thought he taught another person (the lady) "based on his results" and how he was useful to others (probably he was referring to patients): "Yeah when there was u:hm at the beginning when I when I came I discussed with a lady in u:h (.) she was in the the: the radiotherapy u::h and (.) and we discussed quite a lot about u:h (.) how we would be able to detect u:h tumors in the: in the patients (.) based on my results and u:h and this is this kind of discussions which is uhm important because clearly can makes the link between (.) experimental researchers and what they need to: yeah what they need u:h for radiotherapy cell or clinician or (.) yeah it was something I really liked because u:h at this time I really consider that ok we are making research and this is really useful fo::r (.) for somebody else (.)"(line 8656).

3.3.10.3 Diachronic inconsistencies and validation (case 3)

In the same way as I have proceeded in cases 1 and 2, I conducted a collective interview for case 3 with the following objectives: a) to validate a number of conclusions that I have so far made in my previous analysis regarding the individual positions taken during the individual interviews; b) to explore the evolution or change of positions over time, regarding the different issues related to the researchers' controllability of their own research ; and finally c) to compare the (de)trivializing accounts over time. The transcription of the collective interview is in appendix D2. The collective interview went through all the questions that I asked in the individual interviews (listed on page 208), but in a less structured way.

Hyper entrustment

Machines

In this section, I will revisit the participants' positions with regard to the fact that their experiments depend heavily on machines. The first question was too general (do you trust machines), to which Brian responded with a joke: "made in china or what?" (line 8770). After everyone laughed at it, Stephan said: "well that's why you do controls" (line 8773), which suggests that he is somehow skeptical about them, specifying that it is a matter of performance that he is able to check: "[= to check the performance of machines" (line 8775). However, Stephan was first interrupted by Daniel, who only said "yes" (that he trusts machines). Brian did not go into the direction of Stephan and preferred to say: "yes we trust them" without any attempt to nuance his view. Because Daniel said "yes" and did not go into the complexity of the issue (although Stephan had already hinted on it), I addressed my question again to him in particular, because he had said in the individual interview that he doubts machines and prefers to do some operations manually and that he does not prefer sophisticated machines (page 2013). However, Daniel again said "yeah", which suggests that he does not want to go into the problematization of machines usage. Gaelle interfered and nuanced Daniel's position, by saying that "there are some machine that u:h for example (.) you test and you say ok u:h (.) I can trust because u::h I have the same kind of result from time to time (.) then u:h and sometimes when the machine give can gives wrong results (.) it can be either the machine or you experimental condition so you

can check u::h the machine. Gaelle preferred to use the “you” pronoun, when she was supposedly referring to herself and her colleagues. Her account suggests that she is referring to the machine’s reliability or to issues of calibration and not to whether or not the machine is for sure measuring what it is supposed to measure. The expressions that specifically supports this latter idea are: “you check” and “wrong results”. Daniel interfered again and added that the trust is there “= because we’re using control”. After he was supported by Stephan who added “yeah”, Daniel reinforced the idea that what he was referring to is the machine’s calibration and not the inherent validity: “”=so we can:: know (.) if the machine is u:::h giving (.) the right u::h values [”. Brian too supported Daniel, starting his account by the “we” and ending by the “you”: “always we use u:h yeah always (.) yeah normally you need to test the machine ↓ and (0.2)”. Brian preferred to associate the pronoun “we” to the word “always”, which suggests systematicity and hence rigor, but did not specify what they use. However, he switched to the use of “you” when his account became less sure and may be hypothetical: “normally you need” (from line 8770 to 8790). Again, Brian’s use of the word “test” suggests that he refers to the calibration or the reliability of the machine. Bottomline, none of the participants wanted to go into the discussions that we have had previously, at least during the individual interviews.

I then aimed at putting the question in a more provocative way: “Ok (.) but you know you know the difference between trusting the machines because they are consistent (.) you trust because they are consistent so there is no troubleshooting and trusting it because you're sure that it measures what you want to measure?” (line 8791). Brian was the first to respond immediately and clearly showed (or wanted to show) that he understood my idea and that they are already aware of “both” concerns: “Yes both you need both” (line 8795). The words “yes” and “both” together help Brian to say that he already is aware about the machine’s validity along with reliability, although he did not mention anything about the validity prior to my last question. Then Daniel mentioned an example on how they are in fact able to check that the machines measure what they are supposed to measure, by giving me a very simple example of machines that test glucose, which they can test in a different way: “Like u::h you know you're measuring glucose in the medium (.) we have also:: some ( ) standard with u:h glucose u::h I don't know (.) glucose buffer or:::

and we we know (.) which concentration of glucose you should measure and we will see if the machine is able to:::" (line 8801). I responded to Daniel, by saying that he is giving me a simplistic example and that I meant to talk about sophisticated machines, which take measurements that they cannot test in different way (line 8805). Stephan understood my concern, and interfered to explain his position with regard to how they know that these sophisticated machines are doing their job properly: "Normally you have some kind of a standard and you should get a regular correlated response (.) and if you don't get that standard there is a problem somehow in the machine (.) because you know what you put in (.) so you know what normally the machine (.) you have some preparation what should come out (.) if it does not come out then you know it's possible malfunction (.) or you compare one machine with the other (.) and then see (0.3)" (line 8808). Stephan's response creates another problem with machines; it is again the issue of standards. From Stephan's account, I understand that researchers know beforehand what kind of measures they should get at the end of measurements, and if they do not, they would know that they are not consistent with the standards. Therefore, they will do anything that enables them to have measures consistent with the standard. Consequently, the researchers would not be able to research and think outside the standard. Stephan stepped back from the idea of standard and added another idea, which is basically the same that has just been mentioned by Daniel; i.e. that they are able to know in a different way the measurement or the value that the machine is measuring: "**[**because if you (.) like if you have a measurement machine you (.) you make your own concentration your way you pipette your solutions (.) you measure it in the machine and you get a signal (.) and then you:: plot and you can see if you have (.) the::: the radiation shift shoo shoot" (line 8815). Basically, Stephan suggested that from the quantity of the solution that he pipettes and gives to the machine to measure, he can know in advance what the machine's measurement would be, and if it is not as expected then he would become skeptical. This account is more problematic, because the machine is not measuring what he already knows (the dosage itself), otherwise why is he measuring it? The machine is supposed to measure another thing. I responded to Stephan, by saying: "If you determine the dose (.) then the machine is not going to measure the dose (.) why you need the machine because you already determined it (.) the machine will measure something else" (line 8832). At this level Stephan admitted that he was wrong and said: "Yeah but exactly (.) to::: because (.) that response I know how it should respond to the dose I

put in". He added: "[It's measuring something else (.) a product or something else but it's related to my dose" (line 8838). Now, we are back to his previous idea of standards or the preset values; i.e. that he knows the machine's measurement in advance, based on the dosage that he puts in it.

I restated the concern that we left during the discussions over standards; I said: "This is what I'm saying (.) the machine may be consistent (.) every time you give it this dose it will give this (.) and then you give a higher dose it will give you that (.) and in terms of graphs and colors and shapes it's consistent no problem (.) but how do you know that it is measuring what you want to measure?" (line 8840). Now, Gaelle will really respond to the concern. She referred to a particular machine (the QPCR) and said: "because if you put water for example it will not u:h measure" (line 8844). Everyone seemed to enjoy this idea and they all supported it the one after the other (from line 1844 to line 1854). However, I responded to this idea, by saying that while the machine may be designed to work with cells and not with water, it may not really measure the specific aspect of the cell that they want, but rather a different thing. Here, Stephan's volume dropped significantly and admitted that the machine may measure other things: "↓may be like contamination or something" (line 8859). But Gaelle took the floor immediately and switched the discussion to another direction, which exactly addresses my question: that they know how machines work. She said: "Yes but you know the principle of the machine" (line 8862) suggesting that she knows how machines function inside, but again using the "you" pronoun with the "know" verb. Stephan liked this idea by saying "that's why". Gaelle added that: "Yeah (.) even if it's a black box". I asked again: "you know the principle?" (line 8865). Although this time we were not talking about a specific machine, Daniel said "yeah of course". Gaelle reconfirmed "yeah"; but Stephan said: "you have to know". The use of "you" and "have to" suggests that they are supposed to know, but may be they do not necessarily do, in reality. Then Daniel diverted the discussion to a larger principle about people in general rather than about them ad their own machines: "no one can trust a black box" (line 8869). Gaelle liked to go in this direction and put it in a humorous way, by saying: "it's not a magic stuff" (line 8871), which provoked the laughter of everyone. This joke functions very well in this context, as it communicates the idea that they are aware of what they are doing, and that they know the difference between doing magic tricks and doing science.

Because Brian left the room for a while to speak on the phone, when he went back I looked at him and restated the question that I have asked while he was outside, regarding the difference between the machine's consistency or reliability (checked through calibration or due to troubleshooting) and validity (that it is in fact measuring what they want it to measure). Surprisingly, it was Stephan who jumped into the discussion again and put the same answer that he made earlier, although it was clear that the question was addressed to Brian (line 8894). Brian remained silent until he understood Stephan's position, and then only added: "yeah". Gaelle supported Stephan, by saying "Yes usually you do several types of experiments to validate one result" (line 8891). Brian and Daniel supported her by saying "one machine" and "yeah", respectively. Basically, Gaelle's account suggests that they need to use different techniques (or machines according to Brian) and compare them, in order to ensure that the one measurement is right, which contradicts her previous idea (that they know how machines function inside). If they know how machines measure things and that they do reflect the specific thing that they want to measure, so why would they need to vary techniques? Varying techniques, although is definitely a good practice from my perspective, it reveals that the single technique in itself is not independently valid, but needs to be validated by comparing it with another one. It is hard to believe that a lab purchases 2 different types of machines (different techniques) to measure the same thing; and even if it does, it means that the one technique is not inherently valid to ensure that its measurements are correct. Perhaps Gaelle's idea is identical to Stephan's, when he admitted (in the previous paragraph) that in fact the second machine does not measure the same thing as the first (the dosage that he prepares), but that he rather knows that if the second would give him a value that he knows in advance to be matching his first measurement, then he would consider it to be correct. But this again means that this correctness is dependent on a pre-established standard that he knows beforehand, and which limits the scope of his thinking. In this sense, the standards are controlling the freedom of thinking and harnessing the research openness.

I mentioned a specific machine (the photospectometer) and asked what they know about how it works (line 8910). They said they do know and mentioned that it is a matter of different filters and different emissions of lights that interact with the fluorescent liquid that they put on cells before they insert them in the machine. I asked if the

machine responds to the compounds (the liquid) in a perfect way. They were clearly describing the machine's technique, but mentioned nothing to explain that the techniques they are describing do measure what they want to measure (in this case the density, absorbance or fluorescence of any protein that they want to detect). Obviously, I was unable to go any further regarding this example, because I do not have any background knowledge about it. Therefore, I asked if they can mention any other complicated machine that work as black boxes (line 8920). Gaelle almost shouted: "↑No (.) you know"; Stephan nuanced this position: "in principle you should know always know how it works [" (line 8925); Brian preferred to reinforce Gaelle's position or degree of confidence: "yeah you know [". They repeatedly reinforced the idea that they "know" and that they "need to know", from lines 8924 to 8933. Although we were not talking about a specific machine, the participants' clearly preferred to show that they know. Based on how their accounts are functioning altogether in this context, I can only say that they aimed at showing that things are under control. Additionally, no matter how the discussion was long, no one of them was able to take an example of a machine and describe it clearly showing that they do know how it works and that it really measures what they what it to measure.

Products

With regard to the use of products, I put a general question enquiring about whether or not they trust the products that they purchase readymade. The responses that my question solicited were clearly going in one direction. There was visibly no willingness or readiness to try to think that the products may to some extend play some role in skewing their experiments, but that does not mean that the doubt is not there. Perhaps, the participants found it fearful to imagine scenarios to which my question hinted. Perhaps, the question triggered an anxiety and threatened their tranquility. Daniel was the first to respond. He said: "we have [", but was interrupted by Gaelle who said: "Yes (.) but if you can (.) if at one moment you can see that there is something going not in the right way (.) you can say may be there is a problem with this product (.) and for example (.) you can send an e-mail for (.) to the company and ask for the control (.) the quality control see if there is other reported problems (.) and so on" (line 8941). Gaelle's account reduced the importance of the issue to the level of "sending an e-mail and ask for the control", otherwise, she

trusts the products without any reservation. I responded to Gaelle by saying: “so only when there is a problem that is apparent?”. Stephan understood my concern and said “but that’s why you perform controls to check the performance of your assay”. I tried to say that my question was not targeting the consistency of the products, but Stephan interrupted me to say “normally you trust them unless [”. He too was interrupted by Daniel and Daniel in his turn was interrupted by Brian. The three of them, said that they trust the products, as they have mentioned in the previous interview regarding the medium use: “((Daniel)) [yeah it's the same for the medium [”; “((Brian)) yes it’s the same”. I told them “it's the same you mean you have to trust (.) you don't have more options or that [”, but Brian interrupted me to say that “you” can check and that “they’re normally selling good stuff” (lines 8940 to 8961). Although the discussion is not about a specific product or company, Brian still wanted to guarantee the products are good, which excludes exceptions: “[No no you can check (.) but normally it works (.) if they're selling good stuff” (line 8954). Stephan went back to the use of “we”: “we always perform controls with also more or less validation of the used materials” (8955). While he attributed the “controls” to the pronoun “we”, he did not mention who does the validation of the “used materials”; although my question was specifically about the used materials and not about the controls that they perform as per their own research controls. At this level, I can no longer consider that the participants do not understand where I want to go with my question, because we have discussed this issue many times before: in the previous individual interviews, in the previous collective interview regarding the medium, but also outside interviews. Nonetheless, they always preferred not to go into any other meaning of the word “doubt” or “trust”. The only direction that they prefer to account for is trusting or not in terms of consistency or technical problems. Gaelle reinforced the idea that they only want to speak about malfunctions by making a long account again to say the same thing, using the power of percentages, this time: “[but may be for sure there are contamination (.) the powder that we: they give us maybe like 98% of u:h the: indicated product and have 2% of for example the previous production or some: something else (.) but: (.) they they put for example in the product there 98% u:h purity” (line 8957). Stephan supported her by restating: “[98% purity”.

Intensifying the interruptions, diverting the discussion into different arguments when I point to a specific contradiction, using the pronouns interchangeably, not specifying who does the "checks and controls", and repeating the same arguments over and over again made me decide to move to the next question and then get back to this issue of trust by and by. I asked them about how much of their products is purchased readymade (line 8962). They responded by saying that the ratio is around 80% (between 75% and 80% according to Brian). I restated the question by asking them to sketch their current respective experiments and then answer me again. This time Stephan said: "oh then there is 99% ((laughter))"; Daniel said: "yeah for some experiment it could be almost 100%". Brian selected a particular assay that they call western blot, and mentioned: "no for the western blot I would say it's 60%". Because I know how the western blot works and I know they use materials that they purchase readymade, I re-explained my question again (line 8982). Gaelle then responded by saying: "but everything (.) we start from everything that is bought [". Stephan supported her by saying: "everything we buy is u:h". Gaelle reframed the idea of "buying" into "not producing", which legitimizes the practice, because after all they are not manufacturers or farmers to be able to produce or grow the products by themselves, so buying everything becomes understandable and excusable. Perhaps Gaelle was immediately eager to implicitly give sense to this excessive dependency on profit-driven companies. That scientific research depends on business in "everything" or to the extent of "80%" or "100%", is from my standpoint an issue that we need to stop at, regardless of the possibility to do otherwise. Stephan too seemed not to be ready to stop at this concern or think about this situation, so he joked at it: "the RNA[54] is also produced by a patient so ((laughter))" (line 8994). Stephan's joke communicates the following idea: researchers are not creators or gods to be able to create things from scratch, hence pushing the reasoning into the other extreme. Stephan added: "as long as I don't study my own [RNA] it's 100%". Gaelle liked to go in Stephan's direction and said that: "yes sometimes you're using from colleagues" (line 8998). Now, after joking and laughing, it was easy to reinforce the idea that researchers of biology are not able to create things. This idea was immediately played up, whereas the need to rethink this

[54] **RNA:** Ribonucleic acid, a nucleic acid present in all living cells. Its principal role is to act as a messenger carrying instructions from DNA for controlling the synthesis of proteins, although in some viruses RNA rather than DNA carries the genetic information.

situation and account for its drawbacks (regardless of the possibility to do otherwise) was played down or bypassed. So I started to unpack this issue, little by little.

I started by asking them to compare the current situation (of depending on profit-driven providers or outsiders) with the past years (line 9000). Brian said that "they" also had to buy the same, which means that there is no difference between the past and the present. Daniel supported Brian by saying "yeah yeah". Brian was about to explain more, but was interrupted by Gaelle who said: "most stuff [was purchased in the past too]". When I expressed my surprise that they found no difference between the past and the present, where we visibly see more and more companies popping up, Brian nuanced his account by saying that: "the original compounds yes" (line 9008). Hence Brian admitted that the raw materials were purchased in the past and in the present alike, but not the readymade products. Stephan reinforced the idea that in the past, researchers used to do more on their own, but starting from the raw materials they too used to buy: "no there was more to be mixed yourself (.) now you have more ready pre mixed (.) they still had to mix (.) so in that sense" (line 9010).

Now that I noticed no readiness to rethink the situation or try to see it otherwise, I asked: "ok how how do you account for this? This u:h this situation of u:h being dependent on providers?" (line 9014). Stephan was the first to respond by simply saying: "I think it's fine". Brian endorsed him by saying: "yeah"; then they started to praise the situation, by mentioning the following arguments: 1) that it is time-consuming, 2) less expensive, and 3) practical. Brian said "money uh yeah", Stephan added "time and money", Gaelle said: "you need chemistry stuff and:: making the product and so on (.)". Then Stephan clarified his opinion by saying "it's good for consistency". Gaelle clarified Stephan's concern more: "to check the purity of the product to::". Brian mentioned that providers have: "quality control"; and finally Stephan restated: "it saves a lot of time" (lines 9014 through 9026). Clearly, the participants are only willing to praise the system, and never mentioned any concern or any drawback in the way it works.

I now asked them to account specifically with regard to the condition that they do not know what is exactly in the products that they use and that everything is built on an assumption that others have done their job properly, honestly and truly. Daniel and Gaelle were trying to get the floor by humming the first, but Stephan was the first to answer. He said that if these products are made under their control (in the lab), they would be more inconsistent: “Yeah but otherwise you have to make it yourself and it's also (.) it also there is much room for error” (line 9032). Using “otherwise” to switch to what he will describe to be worse, Stephan actually did not account for how problematic the situation of over entrusting, but preferred to go back again and describe the problems of preparing the materials in the lab, although this is not the only alternative to over entrusting. Brian interrupted to say that on top of that: “you trust them”, again without providing the basis of such trust. At this level I told them that they are again describing the problem of the alternative, but not of the current situation and that my question was now particularly about the drawbacks of the trust: “but I'm addressing a specific (.) a specific concern (.) the idea (.) let's talk in general not about your lab or you as a person (.) the idea that a lab does research (.) depending on (.) factors that are 90 % not under its control (.) so I have to trust that they're giving me (.) everything correct (.) what what do you think about this situation?” (line 9038). Even after this explanation, Brian insisted that the situation is good, and again did not address my concern. Instead, he was praising the advantage of time-saving again: “it's good (.) because you'll have more time to think the experiment” (line 9042). I was astonished by Brian’s insistence to account again and again on the same positive aspect of the current system, so I told him again that my question is about “= not knowing what's being provided”. This time, Brian’s account will show the problem, but will show his trivialization of the problem as well. He said: “but when you go to the butcher and you buy this meat because you think it's the good one (.) probably it's not the good one (.) and it was not a cow (.) with u::h a deer or something (.) it's the same (.) if you prefer to grow the cow and everything” (line 9049). The problem here is that the experiments they are conducting are blinded with regard to the components of the products that they purchase, just as any person is blinded when he or she may buy “deer meat” assuming that it was “cow meat”, based on the butcher’s allegation. The trivialization resides in the fact that Brian considers this situation to be acceptable for scientific research, although I totally

understand that this situation is not necessarily their own responsibility and I have repeatedly said “yeah we're not talking about this lab (.) but in general”.

I restated the question, by foregrounding that I fully understand the merits of the current system, in terms of practicality, feasibility and affordability, and restating that my question wants them to account in particular on the situation of “relying 90% on other people's work” (line 9052). This time, Stephan said: “it's a kind of crazy situation (.) it's more or less like a religion like I believe in the manufacturers but that's it yeah” (line 9056), but Brian interrupted him to say again “it’s good it’s good”. At this level, I tried to show to Brian that I am worried about the fact that he still says that the situation is good, even though the discussion is precisely on trusting without basis. I said: “if you can produce everything and control everything (.) which is better? ”. Gaelle admitted that this alternative is better: “yeah it's better if you do by yourself”. Brian again went back again to hint on the same positive aspect of the system, which is time: “and you still have time to to do research?”; I told him: “suppose you have time”. My purpose was to solicit an account for the trust issue in particular, but Daniel interfered by first laughing and then saying: “we know it’s not possible”. Brian again said that “you cannot be good at preparing things” and Gaelle said “[…] it’s not possible” (line 9071). Bottomline, Brian managed again to get the discussion to the positive aspects that he had mentioned over and over again. I failed to get him to account for the condition of trusting and on the idea of building one’s research on the assumptions that others’ products are totally fine. Additionally, the participants’ accounts imply that scientific research has only two alternatives: either to trust others at the expense of not knowing what we build our own experiments on, or to manufacture everything by one’s self. And since the second alternative is not possible or is at least worse, then the first would become “good” practice. I was however trying to imagine other alternatives of controlling the purchased products from within the research community and not from business. It is beyond the scope of this research to look for solutions to such problems, but I am arguing that alternatives and solutions may only exist, when we first acknowledge the problem- not to trivialize it- and especially when we distance ourselves from the position where we harness the logic to 2 extremes and communicate the need to pick the less catastrophic solution. Brian kept going in the same direction and mentioning the same positive aspects of the

current system: practicality, feasibility, cost, time. No matter how many times I told him that I totally understand these issues; that I agree with him, and that I wanted him to account for the issue of trust, he never wanted to go in that direction. Paradoxically, Brian's and Stephan's allegories alike (the butcher and the religion, respectively) reveal their position, in terms of how risky this situation is. Gaelle will now provide a third allegory, illustrating that the use of the purchased products is only built on the assumption that others have done their job properly: "it's like electricity (.) it's like for electricity (.) you're paying a lot and don't know why you're paying a lot (.) but you need electricity (.) so you::: ((laughter))" (line 9088).

Hypo entrustment

In this subsection I will recall the idea of the contradiction between two conditions: 1) that unlike clinicians, biologists lack knowledge about how the human body system works and in particular how cancer behaves at a bigger level than cells (i.e. organs), and that often successful in vitro experiments face failures in later stages, because of the inability to anticipate the translation into in vivo (animals and humans); and 2) that at least in this lab, biologists are still working almost independently from clinicians, and that they keep designing and implementing experiments unaided by other specialists, except on occasional meetings to share their research or have some feedback (when it is often too late).

I started from the preliminary idea that we also discussed in the individual interviews (question 29, page 208), which is about why they assume that studying cancer at the cell level and then go bigger and bigger (tissue, mice and human body) is a correct reasoning (line 9113). I have discussed in the theoretical background that this reasoning is reductionist, and that *reductionism* is perhaps not always a good idea to understand systems, at least from the Gestalt psychological Koffka (1935)'s point of view; i.e. "that the whole is something else [not "more" and not even the word "other"] than the sum of its parts" (p. 176). Gaelle started by mentioning that the first constraint is ethical, because they need to limit their experiments on animals, which is why they need to work on isolated cells: "I think that are different point (.) there is ethic point of view (.) you cannot use like uh mice::: [" (line 9120). Brian prompted

her with the word “everyday” that she repeated and continued: “everyday and doing what you want with the mice (.) and:: there is a second point that you need to have like more a restricted condition to see something like more characteristic” (line 9123). Her second argument is tightly related to her first one; that there is a need to make some advancement at the cell level in order to be able to move to mice. While I understand the ethical concern, Gaelle did not answer my point exactly; i.e. she did not account for the idea of why they assume that they can understand cancer at the cell level first. Especially that experiments that succeed in vitro (at the cell level) fail at in vivo, due to the difference between how the naked cancer cells behave and how organs and bodies with cancer function. So far, Gaelle mentioned the constraints that prevent them from working directly on mice. Additionally, my question did not hint by any means to the alternative of doing experiments directly on mice, without going through in vitro. Gaelle’s account supposed that my question implies that I was suggesting that they start from in vivo any time they want, whereas my question was only aiming to understand their position with regard to my inquiry (whether or not the smallest parts enable them to understand the system).

Brian went further than Gaelle (in supposing that my question implied that they should start with mice). His account is heading into convincing me that even if they start by working on mice, it would not be enough to translate into humans: “I I I think 80% of studies that you do also in mice (.) they don't (.) they're not translated (.) more than 80 87% or so (.) they are not translated into humans so:: (.) there is not like a perfect model (.) the perfect model is in human but hhh then you can't” (line 9126). Brian ended his account with a laughter within the talk right before the expression “you can’t” which helps him to make me understand that I should not dare to think or suggest that they can. Obviously, doing experiments on humans can be unethical or illegal, in many cases. However, clinicians do work with patients already and do take primary tissues samples to analyze and test (biopsy), unlike how it works in this lab (starting from immortalized cell lines[55]). We will develop this idea in the subsequent paragraphs. But Brian’s laughter excludes this idea and urges me to think that working with humans only means trying medicines on them, so I better not dare to mention such an idea. Daniel supported Brian and said: “yes you cannot start” (line 9129). Brian

[55] Cell line: A cell culture developed from a single cell and therefore consisting of cells with a uniform genetic make-up (downloaded from http://www.oxforddictionaries.com).

repeated “yeah” and laughed. I did not say anything about Brian’s idea, but got him back to my question: why assuming that understanding the smallest parts would enable us to understand the bigger system? (line 9131).

This time Stephan took the floor. He was the only one who accounted on this in the individual interviews. Stephan understood the idea very well both this time and during the individual interview. Basically, Stephan said that other studies (clinicians) are already working on the bigger system, but that they also need to study the smallest parts. Obviously, Stephan’s account contradicts Brian’s who laughed at the idea of working with humans. Stephan also added that anyway they are still not able to understand cancer very much, so they need to try different things: “well I think in research a lot of studies are still understanding the separate parts (.) it's the beginning (.) so we need to study the parts themselves (.) even before we understand the system (.) so we're still at the part level (.) so we have to study at the separate cell (.) and for other studies maybe they're already going to the whole body and they study something but (.) they might not understand yet exactly why (.) something is going on” (line 9137). Brian will now admit that there are other ways to do research on cancer. He made a long account (line 9142) to say that both methods are needed. He started his account with “you need all the: set up (.) you need the basic models and you need the more advanced in vivo” and ended it “you need all all the all the:: systems”. Surprisingly, Brian changed his position dramatically.

Now that they accounted on the idea that both methods (starting from the smallest parts and starting from bigger systems) are needed, I again asked them to account for the method of scrutinizing the smallest parts and whether it is a good decision to understand the system. Brian responded first to say: “you need everything”. One more time Brian did not respond to the logic of my question, but rather relied on a vague common sense idea (that “I” need everything). Brian did not only not respond to my question, but also interrupted Stephan who was going to develop the idea that the best way is to work on cancer from both ends (smallest and biggest) at the same time, which he also explained in the individual interview. Stephan said: “if you can work from both ends at the same time (.) also [”. Brian interrupted Stephan to again argue that studying the smallest will help the biggest, by selecting an example:

“and the protein that you have in your body (.) it will be the same as you have in the cell (.) so you can study this protein detail (.) where the compound is finding to this protein or”. Consequently, Brian inhibited Stephan’s new idea. I responded to Brian by asking: “understanding it separately (.) not in its environment (.) is this not misleading?” (lines 9151 through 9156) to get him to heart of my question. He again started by avoiding to respond to this concern (i.e. yes it is or no it is not misleading, or simply that he does not know), but because my question was very precise and adapted to his example, he ended his answer by nearly admitting that it may be misleading, probably he was figuring out an answer while explaining: “the you go to in vivo and the you see the way it's not interacting but because may be the protein there is changing the pH and there is a change in the conformation and its environment (.) so of course there is uh”. Therefore, Brian’s interruption to Stephan did not add anything new to the discussion, but only confirmed that even using his own (Brian’s) example, we can conclude that studying the smallest may be misleading.

Stephan went back to his idea (previously interrupted by Brian) and said it clearly that they do need to study cancer from both ends: “well I think that's people stu: (.) you have people studying in humans some stuff (.) you have other people studying it at the cellular level (.) and (.) by reading each other literature it's (.) you combine both (.) actually (.) indirectly” (line 9160). Interestingly, in this account Stephan confirmed that studying on humans is an actual practice, using the same expression (“in humans”) that Brian initially laughed at (page 260). Furthermore, no one tried to contradict Stephan in the idea that cancer is and has to be understood from different angles at the same time, to be able to understand the complexity.

However, Stephan’s last concern implied that different researchers can only read each others’ literature and not work together in the same place (previous paragraph). I responded to his idea by saying: “who is orchestrating these (.) I think researchers are doing you know all sorts of research (.) everyone is doing [” (line 9163). Before I finish my idea, Stephan interrupted me, showing that he understood my concern – that it is not enough to rely on reading and that everyday interactions and collaborations are irreplaceable. He then admitted that “there is no big

worldwide collaborate (.) not yeah (.) may be you have some projects in which you are combining clinicians and (.) like biologists who are working on one and the same project (.) but I think worldwide everybody is pooling on the stuff at the same time" (line 9165). Stephan's last sentence in particular accounts on that there is some redundancy and disorganization in the way research is going. Hence, Stephan's account made me anticipate the next question.

The next question is about the contradiction between saying: 1) that we need other specialists to be able to understand cancer and 2) keep not working hand in hand with them (line 9169). Gaelle responded the first, although she is the least experienced (a PhD student) and I did not expect her to know how the sector or domain of biology is organized more than her experienced colleagues (post docs who have worked in other labs and other countries before). Additionally, Gaelle has never been in any other lab before. Although, my question was specifically about their lab this time, Gaelle's account invited me to understand that "they" (without specifying who they are) are going in the direction of getting people together: "I think they are trying to have more and more doctors implicated and even some congress they ask doctors to come (.) and u::h to try to [" (line 9176). Clearly, Gaelle avoided talking about her lab, but Brian interrupted her to tell me that he admits this problem: "we don't ((laughter)) it's important". Gaelle stopped talking and Stephan reinforced the idea: "yeah you need to have mixed population in your group to avoid this (.) and that you can" (line 9181). However, Brian's and Stephan's accounts, which admitted the need to study cancer in a more diverse environment, did not account for the contradiction between saying that that this condition is important, but not considering it, in practice (which is the case of this lab).

After admitting the need of multidisciplinarity, our participants will use "it depends" as a tool to step back. I asked them if they can confirm the importance of multidisciplinarity (line 9183), but they accounted that only when there is a need such multidisciplinarity would make sense, forgetting about the previous accounts that cancer can best be understood from different sides at the same time. Stephan and Brian did not give explanations or examples, but used the words "sometimes" (lines 9185 and 9186) vaguely. Gaelle gave two types of experiments, the one needs multidisciplinarity and the other does not. Interestingly, Gaelle's account shows that she still sees multidisciplinarity

from her angle (from the biologist's) rather than from a larger perspective. Her account, presupposes that labs' structures and how they are designed would stay as they are (the biologists, the pharmacologists, the clinicians etc. would keep working separately), and that multidisciplinarity is only a matter of inviting others when we need them to give an opinion and then go back to their 'natural' workplace. However, when I refer to multidisciplinarity, I mean a drastic reconsideration of how studies can be approached from a more holistic, integrative approach; an approach that involves all the specialties.

More importantly, the idea of multidisciplinarity even if it now applied to oncology is not only an oncological concern, and does not only need to be considered from within this field. Social scientists are very well positioned to make suggestions regarding the larger restructuration of the field and the smaller redesigning of the projects too. Diversity is not only a matter of recruiting different types of specialists, but also to be able to orchestrate the diverse groups, to aid them to contribute to and accept this change, to promote a culture of openness, motivation and finally to aim at an organizational culture that optimizes the collective knowledge building. In this sense, scientific specialties appear to need to get out of its "cocoon" to be able to understand that they sometimes make assumptions (based on common sense) about other things, may be without knowing that such or such aspect of their own mode of working is in fact very well studied in other disciplines. From this perspective, any discipline does not totally own its "own" specialty. Not owning one's own research can happen at two extremes: 1) when researchers over entrust to others and build knowledge without being able to control important parts of their own experiments (previous subsection); 2) when they (unnoticeably) assume that they can do other people's jobs (be them pharmacological, clinical or even psychological, sociological and communicational etc.).

The participants, however, continued to describe multidisciplinarity and diversity, from a strictly limited angle: their own perspective, which is conditioned by how they do research right now, rather than from a larger scope. Stephan, who said in the individual interview that in the USA, where he used to work, the lab was meant to be permanently diverse and that everyday interactions between different specialists were used strategically to build

knowledge in discussions, not only to read others' findings from time to time; will now step back: "depending on your project (.) what you want to study (.) some times [". Brian reinforced this position, by saying: "sometimes you need some times you don't need (.) or very basic". Gaelle's use of the term "input" reinforces the idea that their position about multidisciplinarity is rather expressed from their own angle. The different accounts suggest that knowledge is a matter of information transfer or sharing in a send-message-receive model, and is not necessarily built in dialogues and during interactions: "((Gaelle)) I think it's like ((all speaking at once inaudible)) if you want to:: like test like u::h inhibitor of something (.) may be it's interesting to have like u:h the::: input of a clinician (.) but as Brian said if you want to just u:h see the interaction between two proteins (.) they're not needed to::" (line 9187).

Although Brian's example (studying the interaction between two proteins) supported by Gaelle, was meant to be a case where biologists do not need to work on it along with clinicians; when I focused on it, they stepped back little by little. I first asked if studying the interaction between two proteins can be a whole research or only part of research (line 9191). Brian said: "could be", which means that even in this extreme example they "could" as they could not do such a study, independently from other specialists. Gaelle will now account that maybe such a research too is meant to lead to something else under the control of other specialists- the clinicians: "((Gaelle)) because after it will lead to something another question (.) another question and maybe like: 15 years later (.) it will become more close to clinical practice and then may be the clinician would be::" (line 9106). Ultimately, Daniel will make it clear that even in this example they still need multidisciplinarity: "it depends how you design your project at the at the beginning (.) even if it's u:h basic research like studying the interaction between two proteins (.) maybe you have already in mind that (.) to have u::h therapeutic u::h implication so you (.) we already involve clinicians so (.) so it depends how you want to" (line 9202). Interestingly, Daniel used the word "it depends", which serves him not to contradict his colleagues or to correct them directly. The expression helped him to say that they can do both, but his account literally means that even if it's that basic they still need to involve clinicians. Yet, even Daniel who admits that basic biological research too needs multidisciplinary approaches, he still accounted for multidisciplinarity in terms of "we" "involve" others, to suggest that cancer research is their own and they manage multidisciplinarity by

giving or not access to others, rather than try to look at it from a larger scale. Stephan joined Daniel by saying “it depends”, which means that Brian’s example (which he initially mentioned to say that sometimes they do not need multidisciplinarity) is too not always true. Interestingly, all of them were not able to give an example or explanation of how they sometimes do not need multidisciplinarity; and even in this counter and extreme example they realized that they still need it. Hence the use of the word “it depends” has no meaning other than not to contradict Brian’s example; i.e. to avoid that the dissonance becomes apparent (such as suggested by Festinger 1957 and 1962). Because I insisted on Brian’s extreme case and whether or not they need multidisciplinarity to do research properly (line 9212), Gaelle will now use a second tool- the word “not directly” (line 9215), suggesting that the need is there, but not directly. But Daniel again reinforced the idea of the need to do this type of research together, and again used the expression “it depends”, without accounting when they do not need to do it together: “it depends if you are studying this interaction (.) because at the end you suspect a role u:h in therapeutic response or something like that (.) and you know that probably at the end of the project you: you will translate u:h”; then “you need clinics so:::” (line 9216). When I tried to ask why this is not the case in this lab “so then you would [”, he interrupted me to argue that they do through collaboration and cooperation, which is something that I cannot observe by just being there: “yeah we we would u:h cooperate and collaborate with uh clinicians and uh” (line 9222). Additionally, Daniel used the term “would” which suggests that this collaboration is perhaps more or less hypothetical than a real practice. Now, Brian will resort to the tool of “everything” vaguely, this time to imply that you can compensate for the lack of daily and direct interactions: “((Brian)) and also you have everything is (.) published”. Although he used the term “everything”, when I asked “everything is published?”, he said “not everything but u::h if they find something they”. Brian ended by saying: “but yeah I think I think it's important to have the (.) another view” (line 9233), which again suggests that his position about multidisciplinarity is a matter of “them” taking views from “others” when they want.

I restated the main contradiction between praising but not practicing multidisciplinarity (line 9234). We will now be too away from the first accounts (which started when Gaelle played up the ethical constraints and the inability to work directly in humans and which ended up to be simply not true); this time Brian’s account will play up

the difficulty to understand each other. He said: “because it’s difficult to make them to understand what we're doing and to know what they are asking sometimes it's very different mind” (line 9237). Interestingly, Brian’s account points the finger on the need to work multidisciplinarity in a rather comprehensive and inclusive way (as I have mentioned in the last paragraph of page 263), which would require involving other specialists to address this multidisciplinarity at levels (including the sociological, communication, organizational, and management perspectives). Gaelle mentioned the constraint of time: “and sometimes they have not the time ((laughter))” (line 9239); again implying that “they” would still be working in their place and have their own priorities and that multidisciplinarity is only a matter of a number or meetings that they organize to collect some “input” from others. I explained to Gaelle what I meant by multidisciplinarity again: “no no I'm talking about restructuring the whole body (.) how it works (.) how it functions (.) I'm talking about restructuration completely” (line 9241). Even, after this explanation, Gaelle still accounts on multidisciplinarity as if it means to keep working in their lab as they are doing now and invite others to meetings when they have time: “yes but you have to take into account also:: the time of (.) the time table of the: the: clinicians” (line 9243). I explained my view again, by saying: “I mean clinicians would right from the beginning all the time be recruited in labs that are constantly multidisciplinary”. But even when I said that, Brian kept thinking of labs as they are now, to be completely isolated from hospitals, and did not try to imagine other labs that can involve all aspects and levels of research. He said: “but if they are in the lab they're not in the: hospital (.) so they wouldn't have the feedback” (line 9247). I insisted by saying: “I'm talking (.) I'm thinking (.) I'm rethinking the structure of how it works”. Now, Stephan who already said in the individual interviews that such a setting was possible in his previous lab in the USA, will suggest a solution to Brian’s concern when he said that clinicians need to be in hospitals and biologists in the labs: “like somewhere it's like that (.) they're like spending like 2/3 of their life in the clinic and 1/3 in the research (.) it's being done” (line 9256). By saying “it’s being done”, Stephan hinted on the case of his USA lab again.

Now that the contradiction was well established, Gaelle will move on to say that they in fact do involve other specialists already. She started by saying: “but there are clinicians who are doing research” and then tried to give

examples of this lab: “clinicians who are doing research for example if you go in the cardio lab or in the down stair (.) downstair they are all clinicians the PI (.) for example (.) I think so” (line 9263). Clearly, Gaelle was referring to the other labs that share the same building with them, and not to diversity inside this lab. Inside this lab, they are all biologists and none of them is a physician or from any other specialty. Additionally, that there are a couple of physicians in the next door lab (downstairs) does not solve the problem, as long as these physicians do not work on the same projects, and are always busy with their own projects. Moreover, these physicians are cardiologists and not oncologists.

Stephan will now start to critique the compartmentalization of this lab: “but more and more mixed (.) like reinforcement would be a good thing like here (.) all our meetings there's never any physician from like an oncology clinic or something (.) and I think that's why we should begin by” (line 9274). Gaelle interrupted him to basically mention that they are doing well: “but for example for the committee (.) for the committee usual there are clinician (.) for example in my committee (.) there are one biochemist and two physicians” (line 9277). Clearly, Gaelle again accounts on multidisciplinary projects giving the example of her own PhD committee that only meets her once a year or so, to give her some “input”, as she said earlier. But Stephan insisted: “here it’s not enough”. Now that the contradiction became apparent, Gaelle will try to be “right at all cost”. She stated that clinicians do not need to be there all the time, as if I have not said too many times that we are trying to imagine another setting and not necessarily invite others when they have time: “it's not enough (.) but it's not also necessary to have each day (.) a contact with a physician” (line 9283). She immediately added an example of an operation (called western blot [56]) to argue that clinicians do not need to be in the lab, because they will be wasting time if they will do western blot: “you don't need a physician to:: make like western blot or something like that (.) I think it's important in the beginning of the project (.) may be to see like after some::: months (.) to see the evolution and see what” (line 9286). No matter how cancer research is complicated and how many aspects, levels, stages and operations it involves, Gaelle singled

[56] **Western blot**: An adaptation of the Southern blot procedure, used to identify specific amino-acid sequences in proteins.

out a tiny operation (i.e. usually entrusted to technicians or master's students) and used it to argue that it would be not interesting to waste the physicians' time, if they come to work with them.

A *real-meeting dialogue* (Eisenberg, 2010), as discussed in section 2.3.4 on page 49, already emerged and the role of the detrivialization is becoming more and more obvious. Stephan will respond to Gaelle to remind her in his way that physicians would not necessarily do western blots, but they have other things to do: "they could give more like scientific input on a daily or more regular base (.) like what I had in my previous job also (.) there was like in our (.) our boss he was clinician also (.) he was mostly in the clinic but every week we had a meeting and (.) you have also two clinicians in our lab (.) because work on the question that is directly related to the human so". Gaelle went back to mention that they do have physicians, although she mentioned that they are not in theirs, actually (the names that she will mention belong to the cardiology lab not to theirs and one of them already left): "but there are physicians in man (.) in many labs (.) you have Sophie (.) Sonia (.) Michel there was Myriam you can (.) ask them questions (.) if". Stephan reminded her that these physicians are in the other lab and not in theirs, adding that they actually need oncologists not cardiologists as or any type of physicians: "but I think also here it would be good to have more more relations also with oncologists because then you also get more interest of like (.) they're more directly implicated in things and I think" (line 9301). Bizarrely, Gaelle responded to such a concern by saying "but if you want you can send them email to them and ask". Stephan was however able to support his view, by arguing about the importance of diversifying people in everyday interactions; he added: "you need to have like cross fertilization" (line 9316). Consequently, Stephan's account was the strongest, because he pointed at the need for multidisciplinary approaches and admitted the need for diversity, explaining by different means that every day and face-to-face interactions are irreplaceable. Additionally, he was able to give names and use proper and precise vocabulary to his arguments, providing examples from past experience (previous lab). However, Gaelle's accounts were clearly defensive and aimed at being right at all cost. The discussion ended with everyone agreeing that the need is there, hence the consequences of the compartmentalization of research was detrivialized.

3.3.11 Exploring the (de)trivialization dynamic (case 3)

In the same way as I have proceeded in chapter 1 and 2, based on the 3 previous subsections, this section will zoom in on the (de)trivialization dynamic related to the controllability of one's own research; i.e. the need to ensure that one's own research is under one's own control. Following are the questions, which I will address in the subsequent paragraphs: (a) what is (de)trivialized regarding the hyper and hypo entrustments? (b) Who is (de)trivializing it? (c) Why is this (de)trivialization occurring? (d) How serious are the consequences of this trivialization? When and where does it occur? How is it practiced or accounted for? The (a) through (d) questions are inextricably related and can hardly be dissociated, but we will nonetheless discuss each question in a separate subsection, for the convenience of dismantling the dynamic into distinguishable elements. These elements are summarized in table 3, on page 285.

(a) what is (de)trivialized regarding the hyper and hypo entrustments?

This case of (de)trivialization has 4 facets in total: 2 Hyper-entrustments and 2 hypo-entrustments, as follows : 1) entrusting parts of the research to machines assuming that they are measuring what they are supposed to measure; 2) entrusting an enormous part of the lab's own research to external providers assuming that they are doing their job properly and trustworthily; 3) not entrusting aspects of the research to other specialized parties (particularly clinicians), despite the necessity of their contribution, as accounted for by the participants; and 4) the neglect of multi-disciplinary face-to-face regular meetings as a policy to build knowledge collectively. Sections 3.3.10.1, 3.3.10.2 and 3.3.10.3 detail these (de)trivializations as practiced individually and collectively, synchronically and diachronically.

(b) Who is trivializing this issue of hyper and hypo entrustments?

Regardless of the individual positions (or change of positions) during the interviews, the different participants accounted in different ways and with different degrees that they do trivialize these two risks, *in practice*. During *the synchronic interviews*, however, only Stephan was increasingly detrivializing the issue almost from the beginning.

The others tried in different ways to play up some aspects and not to mention others. Only when I asked very precise questions that I was able to solicit detrivializing accounts, which revealed that their previous trivialization was not scientifically based. During the diachronic interviews, the same pattern was repeated a number of times.

1) Hyper-entrustment: Machines

Referring to how he deals with machines, Cédric accounted that he trusts that those who manufactured the machines know their job properly and said that he does not doubt that they might be not measuring what he thought them to be measuring: “no, they are designed to do what they are doing”. Cédric further described how he assumes that the machine is doing the right “stuff”: “((sighing)) I don't know it's (.) just they are designed like that (.) it's like if you're using Word but you type a double b you see the double b on the screen you know that the machine is doing the right stuff (.) so ((signing))” (line 6851). Paul’s first account was even clearer than Cédric, with regard to trivialization in practice: “I don't ask myself these kind of questions (.) I'm I I consider that is being measured is uh reflects the the reality (.) I I don't doubt the except if if again we have obtained ten times the same thing and the eleventh time we do the experiments we have u:h we have uh bizarre behavior than we could start to check whether is the machine is is working properly but we we do the maintenance of the machine in order to: to to get them: uh as uh as efficient as possible (.) this is uh usually checked for the machine why you have them the highest risk of uh of problems this is checked by companies uh who knows the machine better than than we do and who come to validate them using specific QC protocols or quality control protocols (.) so we we are prone to to believe (.) the machine” (line 6821). Joanne also revealed that she has to trust machines: “because I don't I don't really know u::h machines and stuff like that but u::h if it is used by everybody like I have to trust them ((laughter))” (line 6915).

Although Stephan’s position is completely different from Paul’s, Cédric’s and Joanne’s, as he accounted on his doubts that machines may not be doing their job as they would assume, he nevertheless confirmed that in practice that is what happens, anyway: “[you may so much so much treat it like a black box and uh you put something in and you get something out and it's (.) you just have to re rely on what happens in between this (.) it's correct” (line 6661).

Stephan admitted that he has to rely on machines and that in fact he does so, but his position is clearly different from all his colleagues, in the sense that he accounted for the risk (naming it black box by himself) while others did not. Gaelle who first said that she does trust machines, when she further explained more her position, she accounted that she does have some doubts, but only about malfunctions and troubleshooting not about the whole validity of the machines: “u:::h (.) fff u:::h the machine yes but some the::: you can see that the results seems to be u:h (.) con not front but euh bizarre? hhh” (line 6731).

Cédric never changed his mind during the synchronic interview, no matter how many times I repeated that I meant to say how to ensure that machines are in fact measuring what we want them to measure. He said that as long as we pay much to buy them, then they are supposed to be valid: “Yeah that's why you pay a lot for these machines (.) so if if they start not to work (.) sometime you can have trouble and then you call the man from the company (.) and he can he can he can come to see what's going wrong (.) sometimes of course it will take you one or two months to see that there is something going wrong with the machine” (line 6887). Cédric was again talking about troubleshooting and how to address them (i.e. by calling the man from the company). When I insisted that my concern was about to what extend he can trust machines, Cédric said: “[Yeah but that's that's YOU (.) that I have to work with the machine in the right way (.) so the man designed the machine to uh sense fluorescence (.) so then you use the primer data fluorescence and the machine is going to sense the fluorescence (.) just going to give you the amount of fluorescence in your in your wells that's all” (line 6897). Thus, no matter how I insisted on what extent we should trust machines, Cédric never expressed any doubt that machines can mislead us, and the problem is in “YOU” who does not know how to use the machine, not in the machine itself.

When I further explained my concern to Paul, stressing that I mean the machine’s validity and not reliability, he said again that these machines “have been validated by by others uh (.) so uh we we have reference on on what we should get with ourselves” (lines 6833 to 6842). Hence, Paul accounted for the idea that at least for him, he does not know how machines work before buying them, but relies on other people advice; while other people can be just like

himself (they might have taken others' advice). When I expressed my own opinion, by saying that machines can be misleading us; if we do not ensure that they are measuring what we want them to, Paul started to account for how he proceeds before he decides to buy them: "we discuss with colleagues already using the machines so we make our minds in order to (.) finally buy the machine which (.) for which we are convinced that it does measure what we (.) u::h want to s to be measured and then it's a real it does truly reflect the parameter we we uh which is the one to investigate so we uh uh it's not like we we're dealing with black box we're dealing with machine which which are uh which have been validated by by others uh". Paul mentioned the expression "black box" which I did not use, hence he understood my concern very well, but still did not account for anything showing that he knows how machines work. He rather repeated that machines have been validated by others and that before purchasing, he discusses with colleagues who already used the machine.

Joanne also defended her position that it is fine to trust the machines, by saying that "[it's not stupid people who're making those machines". For Joanne, because the machines manufacturers are not stupid, so it is fine to trust them. I told to Joanne that the issue is not about them being stupid or smart but that because they are profit-driven they must produce and sell, but she said "yes they must produce something it's like it's not like u:h for example if you use a ma machine to:: to check the concentration and stuff like that it's not a stupid thing (.) it's correct" (line 6924). Joanne basically said nothing to explain how the machines are trustworthy to that extent. When I insisted on the need to doubt about how machines function, Joanne stepped back and said: "I don't know [ ] maybe if I ask I will know". She not only said that she does not know, but that may be if she would ask she would know; reducing this kind of issues with all the complexity it encompasses to an issue about which we can just ask someone else to grab an answer.

Dilara was clearly trying to figure out an answer on the go. When I told her that my question is not about calibrations and troubleshooting and asked how to be sure that the machine is doing its job properly, she said in French :"(0.3) comment être sûr? Tu veux vérifier (.) si la machine mesure ce que tu veux (.) si pour toi c'est

vraiment important (.) tu peux le faire avec un autre échantillon dont tu connais les résultats (.) donc si tu veux mesurer par exemple le pH d'une solution (.) de ta solution de ton milieu (.) tu veux mesurer le pH de ton milieu (.) pour être sûr (.) tu prends (.) d'autres solutions avec un pH connu (.) et donc si tu prends une solution à 5 (.) et tu prends une solution à 8 et que le machine te donne le bon résultat (.) et toi ton milieu est entre les deux (.) mais il y a des chances que ça soit bon (.) ou alors tu peux aussi (.) vérifier (.) confronter le résultat de la machine ((snicker)) avec une autre machine (.) par exemple une autre marque (.) ou alors une autre façon de doser l'élément que tu veux doser (.)" (line 6718). Dilara's account was clearly not describing something that she currently does, but she was figuring out a solution. She repeated my question « how to be sure? » and then started describing what "I" should do, using the "you" pronoun (" tu" in French). She then added "if this is really important for you", which suggests that this is perhaps not important for her too. Yet, Dilara's answer was again not about how to ensure that the machine is measuring what we want them to measure. She mentioned a case of how to verify the pH level by comparing the level of what a machine indicates with one that is already known. This same example was mentioned by Stephan during the diachronic interview and I responded to him, by saying "if you know the pH level so why you need a machine in the first place?". He then admitted that the machine actually is not measuring the pH, but other things and that he based his judgment on the the fact that he knows how the machine would respond to the known pH level and did not mean that it would measure it again (see discussion on page 249). Thus, Dilara did not only mention a single example, but this example is only about a very simple machine [57] that measures pH through 2 electrodes, whereas the question is clearly about sophisticated machines. Additionally, she did not explain how this machine works, but suggested to compare its measurement to a known level, which is itself measured by a similar machine (which can use the same technique that is or not fallible). More importantly, towards the end of her explanation, Dilara said in French what is equivalent to "there are chances that this would be fine" and from there she left this example and suggested another solution: to compare between 2 machines.

[57] **pH meter:** electric device used to measure hydrogen-ion activity (acidity or alkalinity) in solution (downloaded from: http://www.britannica.com/technology/pH-meter).

The only two accounts that apparently seemed to problematize the issue are the following made by Stephan and Daniel, but when I went deeper in the analysis, I noticed that again the problematization is limited to the issue of reliability (i.e. issues of malfunctions and troubleshooting) rather than about validity. Stephan said: “I'm I always remain critical to see in the (.) it's also why I do my own controls and calibrations and all checks to see also whether the machine itself might have (.) error” (line 6653). Daniel’s following account was quite elusive, because he said that he can know if machines are trustworthy or not, but did not explain how. He instead switched into explaining that he prefers not to use machines, but rather do experiments manually. “I would say that (.) in most of the cases we (.) if we know how the machine is u:h working (.) we can know (.) if we can know trust the machine or or not (.) so normally what (.) I don't like so much u::h when the machine is uh giving directly the results so (.) I prefer to analyze the raw data (.) and to:: to interpret by myself uh (.) the data ((noise someone interrupting)) but u:hm (.) sometimes we:: (.) it's not often the case we (.) or we say that for (.) yeah (.) normally we we:: (.) we have the the possibility to:: to check (.) how the the results are: obtained (line 6678). After accounting on that he prefers to analyze raw data and to interpret by himself (not using machines), Daniel finished his sentence by saying we have the possibility to check; as if his previous talk was about that.

2) Hyper-entrustment: Consumables from providers

Regarding how the participants do rely on purchased products without necessarily being sure that they are identical to how they are described by the providers, I will now restate the accounts that support 3 ideas: that all the labs’ experiments depend heavily on these products, that they do not ensure what these products contain, and that if any of these products is altered it would have considerable consequences on their research:

Paul first admitted that they depend on external providers in almost everything: “well we we we uh we do not u:h we do not generate uh (.) many things (.) I think that we we are dependent of 95% on on uh external stuff”. When I tasked him to account on this situation, he again described it to be not problematic: “well it it it is certainly more and more increasing u::h more and more increasing because with all the limitation you have you have mentioned u:h

you may think that a company for which it is the the job to to prepare to prepare medium u::h according to some quality control u:h level (.) you may at the end trust them more than you could trust your own lab in doing this u:h in a very punctual way (.) so yes they are more and more u::h (.) use of external u:h uh product uh clearly uh I think that from the from the cell to the mice you we we we are dependent on the on providers but we can also appreciate the professionalism of these providers so I do not see that as as a real uh weakness in the system" (line 7404). After all that he said, I asked Paul whether they are able to know if the providers would not do their job properly or if they would systematically cheat in anyway. He said: "Yeah there there there you are (.) you are blinded (.) you are acting as uh as uh being just prone to believe that uh what you are using is is uh doing what you what it is described to do" (line 7090). Clearly, Paul's latter account revealed that his previous account did not take into consideration this doubt that providers may act fraudulently, which is a considerable flaw in the controllability of his own research, or at least is not a scientifically based account, but is rather value judgemental.

Cédric started just like Paul: "everything is coming from different stuff […] Yeah everything". When I asked him if he would know about the consequences of any provider's fraud by altering the composition of the products, he admitted that: "It's impossible to know (.) depend of what you're doing (.) depend if the molecule interacts with the pathway studies (.) it really depend on that" (line 7168). I wanted Cédric to explain more the risk and to see if in spite of all the risk, he would still find it acceptable to rely on external providers so I asked him "it can be open to anything?". He replied: "Yeah (.) yeah of course if you like uh if you put u::h a form in your medium you will add terrible consequence if you put like I don't know (.) fructose instead of uh 1% of the glucose (.) may be won't have any consequence" (line 7171). Cédric only explained things and did not give his own opinion again with regard to whether relying on trust is or not a good situation.

Similarly, Joanne started by stating that "uhum (.) You can say 90% (.) 95%" of the products that they use are purchased. When I asked about whether she trusts them or not, she said: "uhm yes (.) I have to trust them otherwise ((laughter))" (line 7177). When I told her, if she would be able to know that something is wrong in any of the

purchased products, she said: “I won't I won't know about that (.) but you have to use it” (line 7184). Joanne insisted about that “you” have to use the product, no matter how little she knows about its composition. Then, when I boldly said that I expect scientific researchers to be willing to know, she said: “Yes (.) so usually it should be like (.) the company should have some kind of u:h checking maintenance stuff like that (.) so they should not sell something and they are not telling the truth (.) it's not uh again (0.6)” (line 7195). Joanne did not answer my question, but rather referred to the providers’ internal controls. Finally, when I told her to suppose that the providers are cheating, and to tell me her opinion, she then said: “it will be very serious but I don't think I don't think they will do something like that like (.) the government should uh ((laughing)) if there is only big company to sell stuff like that so it should be u:h all all the time check and stuff like that” (line 7205). Because the expression *“not serious”* was not clear to me, I asked her to explain in what sense; she then said: “some results would be false”.

Daniel mentioned that they use “a lot of providers ((laughter))” (line 7585); then he repeated “ok (.) ok so they are quite a lot ((laughter))”. When I told him to account for this situation of relying too much on providers, he said: “yeah (.) u:hm never thought about that” (line 7594), without mentioning anything wrong with this situation. Then, when I tried to problematize the issue, he said: “no for me it's (.) it's fine because u:h (.) they gi:ve they give us information about the product we: want to buy so (.) we know (.) we know what we're buying so” (line 7897). But I responded to Daniel, by mentioning that all he knows about the products is the providers’ own claim. At this level, he laughed and immediately made an account that serves him as a good excuse, without accounting on the idea that they are relying on what others claim: “((laughter)) yeah but we cannot [check] for everything” (line 7900). When I wanted to make sure that Daniel has no means to know about all the composition of the products that he is using, I said that I personally see it as a problem, because they are not able to control their own research. One more time, Daniel, not only confirmed the idea that they are not able to control, but immediately played up the idea that providers are professional: “yeah yeah yeah (.) uh:: I understand your point but u::h (0.3) yeah (.) I know we are: asking to professionals so:: normally they should provide uh” (line 7920). Finally, I wanted to ensure that Daniel’s judgment was also not scientifically based, but rather value judgemental. So I asked, in case the providers would alter

the products, what would the consequences be. Daniel now admitted the problem: “Of course but u:h I hope we can trust otherwise (.) this would change change a lot” (line 7648).

Stephan was again the only one who detrivialized this issue almost from the beginning. When I first asked him to give his opinion about the situation of trusting whatever that the providers supply, he said: “u:h y:: (.) in general I trust them (.) but I also perform some of my own checks to (.) evaluate their them being correct (.) and at times I also find that (.) may be they were not as they should” (line 6992). Stephan also mentioned that the products can be wrong sometimes, and that he would then contact the providers to understand what was exactly wrong: “sometimes I double check things (.) functionality like now (.) I also have something ordered (.) I didn't like what I see and I contacted the company like I think may be something wrong (.) can you explain me why I see what I see?” (line 6996). Then, when I asked Stephan’s to tell me his position with regard to this situation of depending heavily on providers, he said: “yeah the r:: the risk of that of course is like if u::h you're assuming that something is like one batch to the other batch should be the same but you're just assuming that this indeed correct but (.) there is a possibility that this is not (.) and sometimes you might have studies have like (.) you get variations because of the inconsistencies in the product”. Stephan went further in describing that this situation is rather risky for research: “u::h so for sure there is indeed u:h that is risk for that and (.) to some extend we cannot really avoid it but u:h (0.3) there is (.) the only way that we have to check is by (.) I think maybe other people doing similar kind of experiment but may be using them from different manufacturers and then see (.) but yeah it's it's an inherent risk” (line 7251). Finally, Stephan admitted that the products can skew their research: “it's again crazy it's so many parameters for things that can go wrong” (line 7841).

3) Hypo-entrustment: not involving other specialists

Unlike in the 2 previous topics, all the participants agreed right from the beginning that that they need to work hand in hand with clinicians and other specialists within the framework of oncology. Paradoxically, the lab is only

made up of biologists. Again, this is a clear case of stating that something is important, but in practice the situation is completely different.

Stephan admitted this need, by saying: “oh there is a BIG need of that” (line 8108). When I told him that I did not understand why things are working differently than how they themselves say they should be, he said: “yeah it's also sometimes I experience also there is (.) we're not doing the correct thing” (line 8320). Paul, who is the lab director and is perhaps the first one who decides who to recruit and how to design projects, said: “Yes at least this is the: is the (.) the multidisciplinary uh uh approach which is certainly uh the one to go if work with chemists if we work with engineers is because we cannot handle the job ourselves and that we uh u:h we learn from them and they learn from us […]” (line 8224). Paul’s latter account was much longer; he ended it with: “so yes definitely we need to have more more larger mixture of of person in the lab” (line 8240), but he did not say why in practice things go differently than as they want them to be.

Whenever I asked anyone “why”, I got more descriptions of the problem, but never anyone tried to tell me why the situation is different on the ground. Stephan explained: “we should a:sk and get advised from outside people may be that we don't have expertise on (.) sometimes you can u::h you can't see things (.) I've also seen many people in here working on things but just by chance I saw them working on something I was like I think your approach is wrong you seem to be doing this and this and this but you're also doing it wrong (.) you you you're wasting your time (.) that's it's because my bio (.) if I've a biotechnology background and very different than (.) while people here are like from pharmacy or they don't have some of my uh (.) experience” (line 8136). Even when I “tediously” insisted to solicit an answer on why things are not going as they should, Stephan preferred to refer to the future: “I think they should do more yeah” (line 8106). Perhaps the use of “they” this time is very indicative that he does not agree to how things happen, but he did not want to take the risk to critique the lab. Stephan’s accounts were completely oriented into describing the need to work very tightly with clinicians: “I want to do it further (.) you might have the problem that this compound is really toxic and things but you don't maybe have the expertise (.) so maybe it will also already

good to: (.) have some content with uh I don't know ( ) doctor or somebody in medic (.) or who can see about this toxicity (.) give you feedback like (.) this one is probably not going to be a problem (.) or they say no that really is u::h likely going to kill you because then you can say ok (.) may it works in here but (.) I can save myself the trouble because it's not worth studying anyway because we cannot put it into practice (.) because it will be stopped half the process later" (line 8116). Stephan ended with saying that the way they are doing cell culture is not not as cells live in the human body, and ended again with his opinion about what they should do in the future but not about why things are going differently now: "I also found like for example like we're doing culture of colon cells where the way we're culturing them is not representative for the body (.) so then yeah of course (.) you're finding something which is just opposite of what is happening in the body (.) and I think it would be::: sometimes better to have more interaction between biological::: like bio people and medical people" (line 8322).

4) Hypo-entrustment: not considering the role of face-to-face interactions in knowledge building

With regard to the need to strategically organize face-to-face interactions between different specialists to build knowledge, all except Stephan trivialized this idea. Stephan right from the beginning said: "I think you have to be more:: involved already in earlier stages (.) u::h like for example I was in the USA there was (.) when I was working in the cardiac sciences there was on a weekly basis every week there were people presenting (.) one day it was a clin one week it was a clinician the other day it was a clinician the other day it was more fundamental basic science u:h biologists (.) and they were:: presenting data (.) every week there was someone presenting it was talking about studies (.) and then like clinicians in biology they give (.) they share their ideas and that time together ↑ and it's much more regularly and you can get much more faster feedback (.) if you have much more people contact (.) there's much more:: they were forced to work together (.) for example I think here it would be also a good idea than it has to (.) have more joint meetings for example the research group here and may be some doctors from the hospital" (line 8585).

Stephan later accounted for the need of interactions as knowledge builders and not just moments to exchange information; he made a very clear account showing that he did understand my idea: “[It [knowledge] is created because yeah that's also whenever that does meetings sometimes they give them comments they say no but it is like this and then this is the way we::: see this mechanism they say oh but maybe it's because of that and then at that spark they came up with an idea then maybe we should do like this (.) yeah if you use this and this method you can may be see this and then they have new ideas for (.) things and uh” (line 8599). When I told Stephan that some of his colleagues said that they meet from time to time to exchange information, he again said: “no but it'll be too late”, admitting the need of everyday interactions to build research together. Stephan even added that in many times they are “re-inventing” the same thing due to this lack of regular face-to-face interactions: “so I'm more for collaborations and more interactions (.) I've seen it many times before we’re reinventing the same thing over and over or going in the wrong way” (line 8618).

The other lab members have however played down the need for face-to-face and regular interactions, for time constraints. Cédric considered that a meeting once a couple of months would be fine: “maybe it's hard to have daily but monthly or a couple of months reunion or every few time” (line 8721). Because, Cédric was referring to how projects are being designed right now (compartmentalized) and did not account on other ways of doing research (like the way described by Stephan: different specialists working on the same project from the beginning to the end), so he made a judgment again that face-to-face interactions are not always needed: “Yeah it can be (.) depend on the project but sometimes it's not (.) like the clinical stages totally different from pre clinical stage (.) so you're not trying the same stuff and not (.) observing the same stuff” (line 8726). No matter how I tried to clarify my own opinion about the usefulness of face-to-face interactions; Cédric’s next account confirm that he accepts the idea of substituting this form of interaction with just taking the advice of one committee member that he can only see twice or three times a year to give him feedback: “I mean like I have a committee and inside the committee you have physician (.) so if like during one of my committee I say ok I will try may be to use that in my next experiment (.) may be going to tell me that there is no point because if you do that (.) you'll never go to clinic with that it's:: already a toxic compound” (line 8733). Finally, Cédric minimized the usefulness of face-to-face interactions, using the alternative of “calling a

physician" to ask for his opinion, if there is a need: "if the com if this compound is already: u::h known toxic compound or some stuff like that (.) or you can just call a physician and ask him" (line 8739).

When I asked Daniel to specifically say if interactions are irreplaceable, he admitted that and said "u::h (.) I would (.) I would say that ( ) interaction is u:h it's better (.) or mo::re (.) useful most appropriate way that uh (.) yeah we cannot replace interaction between uh between people to (.) to discuss" (line 8643) but, earlier he said: "I don't know every:: two three months having a discussion all together (.) I'm not sure that u::h every day we need uh necessarily to (.) to discuss always it u::h" (line 8154). Paul made exactly the same two opposing accounts as Daniel: he first said that interactions and exchanges are very important, but then developed his idea to state that it is not necessary to organize regular and frequent face-to-face meetings with the aim to build knowledge together: "I've no problem of uh of uh the people being uh contacted but not being here physically present in the lab I think it's sometimes but people think about (.) which is too much to to imagine having uh all people together in the same building (.) ok it helps if you have only to go once one floor below but but uh to work on the on the way to connect people even though they are not physically nearby I think that this is something which could be more easily reached and easily reached" (line 8242).

(c) Why is this (de)trivialization occurring?

Four main reasons were accounted for by the participants on why they should trust machines: 1) because they are are expensive; 2) because people who made them are competent; 3) because they get advice before buying them; and finally 4) because they know how their own machines work. Elements 1, 2 and 3 are clearly not scientific, but only common sense type of judgements. Element 4 however was never explained or detailed. In the synchronic and diachronic analyses alike (sections 3.3.10.2 and 3.3.10.3), I detailed how the participants never explained or detailed their claims that they know how machines work, despite my own insistence on such explanation. Additionally, element 4 was never the initial response to my question (do you trust machines?). Only when I insisted too much on the difference between reliability and validity did the participants end up claiming that they do know how machines

inherently work and that they are able to ensure that they do measure what they want them to measure. Therefore, I concluded that the participants do not distinguish between the machines' reliability and validity, except for Stephan; and that in practice machines are only doubted when they go out of order.

Regarding the lab's hyper entrustments to external providers, all the participants justified it by: 1) that they are professionals and trustworthy; and 2) that they have no reason to cheat. In sections (3.3.10.2 and 3.3.10.3) I detailed how these accounts are again value judgemental and that the lab has no other ways to check that their purchased products are accurate, even though they said that every detail can change their own experiments' results. Additionally, all the participants insisted many times that the providers' products are more reliable than those made within the lab, praising the providers' professionalism and meticulousness. In section, 3.3.10.3, no matter how many times I said that I totally understand their choice and that if I were in their place I would do like them; but that I wanted them to account specifically on the situation of assuming that others have no reason to make mistakes or frauds, they repeatedly said that relying on the providers (even without checking after them) is a good situation.

With regard to the two hypo entrustments, the current research was not able to understand why the lab decision makers: 1) do not ensure diversity and keep working in a rather compartmentalized way, although all the participants expressed the need to work along with specialists (clinicians), in order to be able to understand cancer; and 2) do not use face-to-face interactions as a policy to build knowledge, although they accounted for the importance of such a practice. Yet, during the discussions (sections 3.3.10.2 and 3.3.10.3), one participant (whom I interviewed only 3 days after he joined the lab) explained this situation by saying that they are already diverse and that they get advice from others when they need. One participant said that clinicians are busy and another said that the clinicians' appropriate place is hospitals not labs. The detailed synchronic and diachronic discussions revealed another position that all the participants except Stephan accounted for: that it is fine to substitute face-to-face interactions with meetings every 2 or 3 months.

(d) How serious is this trivialization?

First, in terms of "when" the 4 facets of trivializations happen, the shortest answer is: all the time, for all the experiments. Our interviews discussions were not about a specific situation, but rather about: 1) the everyday use of machines; 2) the everyday use of the purchased products; 3) the totality of the labs' projects that are designed in a compartmentalized way; and 4) the lack of face-toface interactions as a strategic practice to build knowledge collectively. Secondly, in terms of "where" such trivializations are happening, the answer is in this lab, knowing that this finding is transposable to any other lab doing the same thing as this particular lab. Thirdly, in terms of "how" these trivializations happen, the answer is the following. The participants initially trivialized elements 1 and 2 (the need to ensure that machines are valid and to ensure that the purchased products are de facto accurate) unnoticeably. The Individual interviews' discussions (section 3.3.10.2) revealed that the participants did not pay attention to what I meant by ensuring that machines are really valid and what I meant by ensuring that the providers' products are really trustable. Then, except Stephan, all of them continued to trivialize the 2 elements. After discussions, all of them detrivialized elements 1 and 2, using the following arguments: for element 1, they said that it is important to ensure that machines are in reality measuring what they are supposed to measure; and that they do know for sure that they are. For element 2, they ultimately, detrivialized the issue, by mentioning that in case providers turn out to be not doing their job properly then this would be "very serious", again insisting that such a possibility is weak. With regard to elements 3 and 4; it is up to the lab's decision makers' to account why they continue to work without other specialists although the participants emphasized this need, and it is up to them too to account for why they keep not organizing regular and diverse face-to-face meetings as a strategy to build knowledge. Considering that Paul (lab director) is to the best of my knowledge the decision maker, I asked him these questions, but he never explained why. As I detailed in section 3.3.10.2, he constantly referred to the future and mentioned that they would need to make more efforts in that regard. Additionally, Paul revealed during the discussions that it is fine with him to substitute face-to-face interactions with "collaboration" and "meetings every 2 or 3 months". Daniel too made the same accounts, but Cédric too was extremist in this regard (see section 3.3.10.2, page 238). Gaelle tried to trivialize the 2 issues in the collective interview, when Stephan corrected her by mentioning the need for multidisciplinarity and for

the daily face-to-face meetings. She then stepped back (see section 3.3.10.3, starting from page 259). The other participants increasingly detrivialized issues 3 and 4 during the synchronic and diachronic interviews.

In terms of the seriousness of these 4 trivializations, and based on sections 3.3.10.1, 3.3.10.2 and 3.3.10.3, I will now state succinct answers: 1) Our researchers are building their own research findings based on machines' measurements and assuming that the machines are measuring what they are supposed to measure. If any of the machines turns out to be not in reality doing so, then their research results are at stake; 2) our researchers purchase "everything" (using their words) readymade from providers and do not ensure that the chemical composition is accurate. If any purchased product turns out to be not as described, they can expect any skews for any of their experiments; without being able to notice them; 3) our participants say that they are not able to understand cancer without involving other specialists, but keep designing and practicing in vitro experiments without them; hence this condition causes their in vitro models to shift from in vivo. Finally, 4) our researchers keep ignoring face-to-face interactions and regular discussions, which is hindering rather than optimizing their collective knowledge building.

Table. 3 The (de)trivialization dynamic (case 3)

What is (de)trivialized? (Object)	**The risks of Hyper-entrusting and of Hypo-entrusting**: • **Object 1**: the need to ensure that machines are in effect measuring what researchers want them to measure. • **Object 2**: the need to ensure that the readymade products are in effect identical to how they are described by the providers. • **Object 3**: the need to involve diverse specialists, from earlier stages, to be able to understand cancer. • **Object 4**: the need for face- to-face interactions.
Who is (de)trivializing? (actants)	**Object 1** & **2**: • Prior to interviewing: All participants trivialize with varying degrees. • Synchronic interview: All trivialized; but Stephan detrivialized. • Diachronic interview: Recurrent oscillation between trivialization and detrivialization, by everyone. **Object 3 & 4**: • Prior to interviewing: The lab's decision makers, for 3 & 4. • Synchronic interview: All detrivialized object 3; All, except Stephan, trivialized object 4. • Diachronic interview: Increasing detrivialization by everyone for object 3; recurrent oscillation between trivialization and detrivialization for object 4.
Why does this trivialization occur? (causality)	**Accounted for** **Object 1:** machines are expensive so they are valid; people who made them are competent; we get advice before buying them; we know how they work. **Object 2:** providers are professionals and trustworthy; no reason to cheat. **Object 3:** we are already diverse and we get advice from others; clinicians are busy and their appropriate place is hospitals not labs. **Object 4:** it is fine to substitute face-to-face interactions with meetings every 2 or 3 months. **From Analysis** Prior to interviewing: not identified for any of objects 1 through 4. Synchronic and diachronic interviews: **Object 1**: • Machines are trusted unless they go out of order, or break (a practice taken for granted). No distinction between machines reliability and validity. **Object 2**: • Providers are trusted because they are accounted for to be professional, precise and because they do their own controls. Providers are accounted for to be trustworthy, because their reputation is at stake. **Object 3 & 4**: • The research was not able to understand why decision makers do not ensure diversity and do not do more to boost interpersonal interactions, although all participants detrivialized their importance for knowledge building.
When is this trivialization occurring? (temporality)	• All the time; for all experiments.

Where is this trivialization occurring? (spatiality)	• In this lab for the 4 objects (results are transposable to other labs, worldwide)
How does this trivialization occur? (manner)	• **Unnoticed**: All participants for the objects 1 and 2. • **Noticeably**: Object 1 & 2: All participants trivialized except Stephan; then all detrivialized noticeably. Object 3 & 4: Lab's decision makers' choice; Cédric, Gaelle and to a lesser degree Daniel and Paul trivialized noticeably. Others detrivialized noticeably.
How serious is this trivialization? (magnitude)	• Object 1: If researchers build knowledge on machines as black boxes, they can expect any skews. • Object 2: If researchers purchase "everything" readymade from providers and do not ensure that the composition is accurate, they can expect any skews. • Object 3: If participants (biologists) say they are not able to understand cancer without involving other specialists, but keep designing and practicing in vitro experiments without them, they can expect more disappointments in vivo. • Object 4: If researchers keep ignoring face-to-face interactions and regular discussions, they are potentially compromising knowledge building.

3.3.12 Primary Findings (case 3)

As I have proceeded in cases 1 and 2, I will here outline the primary findings of case 3. In section 4.1, where I will outline the findings of the whole research, the current primary findings will be squeezed up with those of cases 1 and 2. This case was concerned with the lab's controllability of its own research, from two extremes: It explored two areas where the lab entrusts a significant part of its own research to external parties and two areas where the lab omits to entrust to other parties, whose contributions was described by the participants to be a necessity. Hence the excess of entrustment represents a threat to the lab's controllability, emanating from the situation of: 1) *basing research on assumptions that others have done their job accurately*; while the lack of entrustment represents a threat to the controllability emanating from the situation of: 2) *doing on their own what they do not control without others*.

Discoveries

First, I found that the participants take for granted that machines do measure what they want them to measure. Some said that they do not ask themselves "such questions" and that as long as machines have been recommended by peers who previously used them, they should be valid. Some said that they do doubt machines, but they only referred to their reliability (i.e. troubleshooting and calibration issues) and never to validity. When participants insisted that they do their own "checks" and "controls", I focused my questions on validity, soliciting explanations on how they can ensure that machines do measure what they want them to measure, but at no time any of the participants demonstrated that they do know how machines inherently function. Other participants said that they do trust machines, assuming that people who made them "are not stupid"; and that as long as they pay much, they should be able to have appropriate machines. Based on these positions, I found that our researchers entrust a major part of their research to machines, assuming that what happens inside them is valid; but this assumption has no scientific basis.

Secondly, I found that the participants' consume "everything" readymade (immortalized cells, medium, trypsin, plates, reagents etc.). They initially used percentages from 75% to 100%, but later on many occasions they said that "everything" is purchased readymade. Some participants discovered that they consume that much, for the

first time during the individual interviews. Additionally, when I asked if they trust providers, they said without any reservation that they do trust them, even though the question was not about a particular product (i.e unlike in case 2, which focused on the medium). They repeatedly insisted that they trust all providers of all products, except for minor mistakes from time to time. When I asked about the basis of the trust, they mentioned that providers are: a) professional, b) competent, and c) trustworthy; assuming that they have no reason to cheat. They also said (except one) that providers are more reliable than their own lab, because they are able to make consistent products. Furthermore, all participants said that they do not check after providers. Some said that they can only discover alterations, if they happen between one batch and another (i.e. inconsistent composition that they can possibly observe). Most importantly, all participants said that if some providers happen to be systematically altering the composition of any of the purchased products, their research would be open to any risk and that they would be blinded (unable to know about the consequences of the alterations).

Thirdly, I found that on the one hand the participants accounted for the importance of multidisciplinarity and diversity in order to be able to understand and treat cancer, but on the other hand this lab is made up of biologists only. Many successful in vitro experiments fail at in vivo and likewise many in vitro experiments may yield false negative findings, due to the need to improve the design of the research protocol, by involving more diverse specialists (i.e. clinicians, pharmacists, epidemiologists etc.). One participant said that he figured out on several occasions that they are “not doing the correct thing” and that they should “ask and get advice from outside people”. He additionally mentioned that in his previous lab in USA, they used to be strategically diverse and they used to design projects together at all research levels. 2 participants however said that they are already diverse and multidisciplinary, when all other participants said that they are not. One of these 2 participants said that they do have clinicians, mentioning 4 names that do not belong to their lab (but to the cardiology lab, which share the space with them). Additionally, one of the mentioned clinicians already left the pole and the other 3 work on their own projects and are not involved in this lab’s projects.

Fourthly, although the participants emphasized the need for face-to-face interactions, I found that the lab does not strategically organize interactional activities to build knowledge. During further discussions, all participants, except one, suggested that they can substitute face-to-face interaction with a meeting every 2 or 3 months, or with other forms of collaboration. I found that no matter how much the participants initially praised the role of interactions in knowledge building, they (except one) ended up making accounts that reduce *knowing* to sending and receiving (or sharing) information with others. All the participants (including the lab director) never responded to my concern on why they do not practice face-to-face interactions, while they say that they need them. They constantly diverted the discussion to the future, typically saying "we should do better in the future". During discussions in the individual and the collective interviews, 2 participants implied that they can substitute for the lack of interactions, by "sending emails" to the right person and "get their input". However, one participant was able to persuade his colleagues during the collective interview that face-to-face interactions at all research levels are irreplaceable in their research, mentioning examples where it would be too late and not sufficient to wait for occasional conferences and seminars to "share" one's knowledge and "get feedback". He additionally mentioned how, in his previous labs, knowledge emerged (not only shared or exchanged) from interactions.

(de)trivialization

During the individual interviews, all the participants trivialized the importance of the 2 hyper entrustments and the 2 hypo entrustments. While the hyper entrustments were justified by ungrounded accounts suggesting that others (to whom they entrust) are doing their job properly; the hypo entrustments were justified (only by 2 participants) by the possibility to contact others (or e-mail them) and ask for their feed back when there is a need or a question. Furthermore, the participants always diverted the discussion into what they would need to do in the future, instead of responding to my questions about what is now being done (i.e. why they now do not work along with other specialists and do not implement daily face-to-face interactions). During the collective interview one participant insisted that others (clinicians at large) do not have time to come and work with them, although the discussions were about a total restructuration of oncology, where all specialists work together at all research levels (de-

compartmentalization). The participants recurrently oscillated between defending their practices with regard to both hyper and hypo entrustments (trivializing their risks) and describing the real threats (detrivializing the risks).

The same strategies used in the 2 previous cases were also used in this case to reframe any contradictions or dissonance that I pointed out: 1) switching from “we” to the inclusive “you” when they switched from accounts illustrating that “things are under control” to accounts where there are doubts and weaknesses about the effectiveness of the described practice, suggesting that: “you too (me) would do as we are doing”; 2) using “sometimes”, “also” and it “depends” to join incongruent or irrelevant ideas, or to deviate the discussion from the exact question; 3) using the “no but yes” strategy to systematically disagree with any critique to their practice, but agreeing that there should be more improvement; 4) using the “yes but no” strategy (by one participant) to systematically agree with critiques to the practice, but suddenly disagree that there is a real skew threat and mentioning that the risk is calculated and that things are under control, without justifying this latter position. During the collective interview, the participants’ used the same strategies that they used in the individual interviews. They also: 5) avoided to disagree except once, when I pointed that the one participant said one thing and the other said the opposite; 6) prompted each other with words and supported each other’s claims with “yes” and “yeah” even when the speaker has not yet finished a simple sentence; 7) when I pointed out contradictions, they responded with new ideas, interrupted each other, overlapped and raised volumes noticeably; which suggested their interest in defending their practices more than improving knowledge.

4. Findings and discussions

4.1 Findings

When I designed the research main question (*How can the (de)trivialization practices be useful to know about knowing?*), I generated the following 3 corollary questions out of it, as I have detailed in section 3.2.1: (a) What is the role of these trivializations in the lab's knowledge construction? (b) What is the role of the detrivializations- triggered by our research- in the lab's knowledge construction? And (c) how can we know about knowing, by studying these cases of (de)trivialization, from our perspective? In this section, I will outline the current research findings, channeling them into the 3 corollary questions (a, b and c below):

(a) Trivialization: compromising knowledge construction

It is beyond the scope of our research to determine whether the 3 studied trivializations have actually been compromising knowledge construction, because this requires a number of biological experiments that may enable biologists to know the real consequences of these trivializations. However, the research found that these 3 trivializations have put cancer research under a considerable risk, at least theoretically. Thus, in principle, the trivializations over the 5% level of co2, the medium usage, and the hyper and hypo entrustments are of a huge magnitude; but in reality, the consequences of these trivializations are only accountable from a biological research perspective. If any of these 3 trivializations would turn out to have real negative consequences on the experiments, it would then be behind a real skew that is applicable to a huge number of cancer experiments; considering that the 3 trivializations are transposable worldwide. I based my conclusions on a number of elements for each case, as follows:

1) Case 1- *the use of the 5% level of co2 in cell culture*: first, all lab members did not know where this practice comes from, including the director who finally concluded that it was "dictated by the medium that was available commercially", 50 years ago. Secondly, they said that they never asked themselves "*why 5%?*" before and never tried to do other levels of co2; except one participant who said that she once used 10%, because she then did not know that 5% was a standard. Thirdly, they all said that 5% is not the co2 level everywhere in the human body,

and therefore their cell culture is possibly not mimicking the real conditions of the human body. They work on different types of cells, which they reported to require different co2 levels (and different pH levels which also depend on the co2 level), but using 5% all the time. Finally, discussions with the lab members have yielded to various accounts describing: a) the importance of this issue and how it has been trivialized or "ignored", and b) the negative consequences of the standardization of the 5% level (i.e. phenotype change; adaptability of cells; false negative or false positive findings related to drug tests, and "blinding" or not knowing the real consequences). See table 1, page 129.

2) Case 2- *the medium usage*: first, the current method (i.e. the same medium remains on cells and is only replaced after about 3 days; unlike the flowing medium system), which was initially described to be appropriate, was later accounted for to be possibly skewing experiments. The skew emanates from the nutrients' concentrations variation and the resulting cells adaptation to this variation; which would potentially shift in vitro models from in vivo. Secondly, auto-contamination emanating from the current medium usage, which was initially said by most participants not to be a possibility, was later (synchronically and diachronically) admitted to be quite possible. Finally, I found: a) that the participants trust the medium providers not on any scientific basis, but rather on a common sense type of trust (i.e. that they are serious, competent, and that they have no interest in cheating); or in case the medium composition changes from one batch to another in the middle of the same research, providing that this change would result in an observable alteration. By the same token, the research found: b) that if the purchased medium turns out to be all the time (not depending on the batch change) different from how it is described; it can potentially skew their experiments (to use the lab director's words, they would be "completely blinded"). See table 2, page 196.

3) Case 3- *the hyper and hypo entrustments*: first, the participants, who initially did not understand my concern with regard to whether they are sure that their machines in fact measure what they want them to measure, later assured that they know how they inherently function; without providing any concrete explanations, no matter

how much the interviews solicited details and examples. Therefore, I remain skeptical about their trust with regard to the machines' *validity* and not *reliability* (i.e. no matter how robust and precise the machine can be). Although, they insisted (especially diachronically) that they understand machines very well, their first reactions were completely different; i.e. they totally trust machines like black boxes (examples: "I don't ask myself theses questions", "we pay a lot for these machines", "it's not stupid people who're making these machines" etc.). Secondly, the participants said that "almost everything" is purchased readymade. They used: "almost everything", "everything" and percentages from 75% to 100%. They additionally said that entrusting "90%" or "everything" to external providers is "good" and persistently described it to be unproblematic, but this position was again based on a common sense idea; i.e. that providers would not have any interest to cheat or to do their job incorrectly, otherwise they would lose business. Furthermore, the research was able to find that if any of these purchased products are systematically altered for any reason, they would skew their research results without being able to figure it out. The researchers would only be able to discover "cheating", if the alteration takes place between one batch and another in the middle of an experiment, and when the skew happens to be possibly visible. Moreover, even when they discover glitches, they would not be able to determine it by themselves. They rather seek clarifications and solutions from the providers. The two latter situations illustrate how the lab does not control its own experiments, because it excessively entrusts (hyper-entrusting): a) to machines, assuming that their measures and calculations really correspond to what the researchers want to measure, and b) to consumables' manufacturers, assuming that they do not have interest to alter anything. Finally, on the other end of the continuum, the research found that the lab does not entrust enough (hypo-entrusting). It fails to: a) entrust aspects of cancer research to specific specialists, although the lab members emphasized the need to involve all specialists (i.e. clinicians, pharmacists, biologists, nutritionists, epidemiologists etc.), in order to be able to design research protocols or to holistically understand the cancer metabolism, and especially to ensure that in vitro models are closer to in vivo. Moreover, the lab happens to: b) trivialize the necessity of face-to-face discussions and regular multidisciplinary meetings that aim at producing knowledge collectively, regardless of how much the lab members accounted for the need to exchange knowledge with diverse specialists. See table 3, page 285.

(b) Detrivialization: a round trip

The interviews and the provoked discussions with and between the lab members during observation (sections 3.3.1; 3.3.5; and 3.3.9) have caused the emergence of new knowledge elements, for each of the 3 cases of trivialization. These elements were outlined in the "*variation of accounts*" section of each case (i.e. sections 3.3.2.1; 3.3.6.1; and 3.3.10.1), which detailed how discussions over the previously ignored matters initiated the new detrivializing positions. Shifts to new positions by every lab member in each of the 3 cases were detailed in the "*synchronic inconsistencies and contradictions*" section of each case (i.e. sections 3.3.2.2; 3.3.6.2; and 3.3.10.2). More positions' shifts, which were provoked by my questions were also detailed in the corresponding "*diachronic inconsistencies and validation*" section of each case (i.e. sections 3.3.2.3; 3.3.6.3; and 3.3.10.3). In every interview, I directly asked the participants to account and give details on whether the issue in question: a) was trivialized, and whether it was b) important. The 3 issues were described to be of considerable importance (hence detrivialized from the participants' points of view) and that they have been trivialized in the past, providing details (sections 3.3.3; 3.3.7; and 3.3.11). The analysis detailed how the detrivializations have triggered unprecedented discussions and views that evolved in a remarkable way, during the individual and collective interviews. It is beyond the scope of our research to find whether new experiments that would result from our interview discussions would lead to a significant advancement in the lab's knowledge. I am nonetheless confident that, at least the consciousness around the necessity to rethink the 3 issues has emerged and was articulated, as a result of our interviews.

Yet, as I will detail in the next subsection (along with figure 3, on page 304), the trip to detrivialization was not "one way"; it was rather a "round trip". In each of the 3 cases, I detailed numerous examples where the participants oscillated (with varying degrees) between the two positions (trivialization and detrivialization), which I explained by the instability that my questions provoked (providing details on how the participants were figuring out their answers on the go). Additionally, I detailed a number of examples where the participants were also achieving functions or defending judgmental positions through discourse. They were not only focused on the topic or on providing scientific accounts. Potter & Wetherell 1987, have theorized this idea by arguing that "the distinction

between ‘the object of thought’ and position on a ‘dimension of judgment’ becomes virtually impossible to make” (p.52). Our discourse analysis admitted beforehand that our research is not able to read people’s minds or to capture their intentions, but was able to explain how discourse functions contextually and how it helps the informants to achieve this or that function. Hence, I interpreted every trivializing or detrivializing account, in view of the corresponding function that it played contextually.

By the same token, the variation of accounts (individually and across participants) provoked by the detrivialization suggests that the studied scientific discourse is not totally scientific and that depending on the role that the person is playing in context, the accounts can be totally contradictory, and hence problematic. I have discussed in section 3.3 that people use language as a tool to achieve functions, and therefore they can change accounts depending on their contextual needs. However, I would expect the scientific discourse to be with less variation and more precision. I have detailed in the 3 cases that these trivializations are not only emanating from default conditions that are beyond individuals or unnoticed research practices that are taken for granted (prior to my research), but are also discourse practices that persisted during the interviews, possibly for the reasons related to how people wish to present themselves socially, as suggested by Potter & Wetherell (1987). In each of the 3 cases, I have detailed how people continued to trivialize a matter that they themselves had described to be important. I preferred not to target the “why” they do what they do, but I detailed how the (de)trivialization practices were used (discursively) to minimize the contradictions that popped up. The discursive tools that the participants used are detailed starting from page 301 (parts 2 and 3 of section (c) below).

Finally, the analysis captured a number of examples, where the detrivialization was being processed individually, by the group, and in relation with my own questions. Through recurrent discourse elements (i.e. hesitation, omission, repetition, volume rising, interruptions, overlapping, laughter, the use of pronouns and strategic words), the analysis zoomed in on the participant deconstruction of previously stated ideas and building new or adapted ones. I have detailed instances where understanding is affected by the questions and especially the following

questions that triggered clear breakthroughs, both synchronically and diachronically: Case 1: (question 6: *is 5% of co2 the same condition in the human body?* And question 9: *what other negative consequences can happen to the cells, due to this 5% level?*); case 2: (question 16: *If cancer is a molecule that is causing a disorder in the regulation of the cellular reproduction, would it be possible that the molecule stay in the medium, get multiplied there, and then contaminate the others that may have been cured?* And question 21: *Are there risks that if the medium components are not exactly as they are described, they would make the cells develop resistance to drugs, or different protein expression?*); and case 3: (question 27: *how do you account for the condition of depending on other parties?* And question 29: *why do you need to study cancer at the cell level first?).*

(c) The (de)trivialization study: a strategy of meta-knowing

Our third concern is related to how we can know about knowing by studying (de)trivialization. First, the fact that almost all lab members said that they have not thought about these issue before, although they are as important as they described them, is suggesting that the participants' knowing activities can be collectively affected by a hidden organizational dynamic that is perhaps constantly putting their knowing capacities at stake. This idea triggers the following question about human learning: to what extent, the participants (researchers) are free to know (to practice research) as they think they should? In this case, I found that this dynamic has 2 layers:

The layer number 1 is a hidden and underlying "a microphysical power" (Foucault, 1969), which only emerged due to the current study and was otherwise unnoticed. I found that the detrivialization of the 3 issues emanated from the problematization of what the participants call: a) the existing knowledge, b) the standards, c) the practical knowledge, and d) the previous validation. This underlying power can also be explained by the robustness of the "structure" (Giddens, 1984) and its supremacy over the "agency". I illustrated through various examples, how the participants were defending the "structure", when they embodied it against their own knowing freedom-"agency". A third reading of this underlying layer, where we can understand how the practice of free knowing is compromised is: Cooren's (2016) troika of *Text-Trace-Mark*. Using Derrida's understanding, I will apply these

concepts as follows: while the lab is the *text* that this research is studying, the usage of the 5% level of co2 is rather an example of a *mark* that has well been established in the domain of oncology or biology (the bigger text(s)) long time ago and has been visibly organizing our text (this lab) and other texts (other labs). Perhaps, because this mark is very powerful (a worldwide practice) that researchers in this lab did not try to do otherwise; i.e. to try another *trace* (ex. another level of co2). However, the current research triggered a new thinking in this lab, which may contribute to this change (tracing differently) by trying new levels of co2 until a new research finding is achieved. Only then, these traces may become a new mark and may reorganize our text (the lab) and perhaps be extended to the bigger text (the oncology domain).

The layer number 2 was illustrated through the dynamic of knowledge automatization, when participants were visibly aiming at converging, conforming and agreeing; particularly during the diachronic interview. This dynamic is perhaps similar to Janis (1982)'s *Groupthink* phenomenon, which was reported on various organizations and which caused individuals not to be able to dare to ask new questions or to genuinely practice their own knowing activity, supposedly not to destabilize the group harmony and homogeneity and to keep feeling secure. No matter how much educated any of the lab members is or how much experience he or she has, none of them was able to challenge this dynamic. At this level, if we see the practice of regulating the incubators at 5% as an element of the lab's culture or set of habits, we understand that the organizational culture is more powerful than the individual's knowing capacity.

More interestingly, our interview questions being basic and accessible (at least to me), enabled me to explore the scientists' accounts, especially to determine the non scientific accounts embedded in the scientific discourse; hence to study the participants' knowledge bases through their explanation practices. In line with Latour & Woolgar (1979), a number of non scientific accounts were used to justify scientific positions: For case 1: a) defending the need to standardize the level of 5%, even after saying that the level can skew research and that it needs to be revised depending on the cells requirements; b) justifying the 5% level by the need to replicate the same research by another lab, even after I pointed out many times that labs that are interested to replicate their own experiment can do so by

following the research protocol with whatever level it mentions and not necessarily 5%; for case 2: c) not knowing the exact composition of the medium (justifying it by the fact that the list is too long), but saying at the same time that every compound can skew the results and that the lab is particularly paying attention to these details, because it specializes in cancer metabolism; d) trusting the medium's providers not based on any scientific explanation, but on the idea that they are serious, professional and have no interest to cheat or alter the composition of the products; for case 3: e) trusting machines, because they are expensive, or because people who manufactured them know what to do; f) assuming that they can substitute for face-to-face interactions and strategic knowledge building discussions by exchanging information through "emails" or organizing meetings every 2 or 3 months.

Finally, studying the 3 cases of (de)trivialization from a discursive psychological point of view enabled me to zoom in and out between the following 3 levels that come into play with regard to how knowledge is constructed. I sequenced the 3 levels in this order, following how I happened to explore my field: 1) the (inter)organizational level (because I first started with observation and was able to find that the 3 trivializations are (inter)organizational practices; i.e. commonplace); 2) the individual level (because I later conducted individual and synchronic interviews to explore the different accounts and positions that popped up during the observation), and 3) the group level (because I later conducted a collective and diachronic interview to explore how my questions will be responded to by the group and to compare the individual responses over time). I will now detail figure 3 (page 304), which depicts the interaction between the long established trivialization from one hand and the detrivialization initiated by our research on the other hand, in the three stages of our research (i.e. observation; individual interviews; and group interview):

1) The organizational level:

   In each of the 3 cases of the current research, my first detrivializing question was confronted by a long established trivialization that is rather an organizational default condition or tradition (structural); a standard of unknown origin, than an individually-motivated or an explicit group decision. Nonetheless, the same trivializations were, to some extent, group and individual practices. No matter how 'passive' or inadvertent these

practices are, they are to some extend choices made by the participants (researchers) during the design of their own research protocols. Prior to my breaching experimental questions (Garfinkel 1967), the trivializations were unnoticeable conditions and taken for granted practices. The moment my questions came up to the surface, the trivialization started to be noticed. Noticing the long established trivialization and especially what underlies it in terms of consequences for their research was the first spark of detrivialization.

This spark was manifested in 3 different ways: for case 1, the participants immediately expressed their surprise that they have not thought about my question before I asked it (see background of case 1, page 77). They then said that they did not know how to answer my question, and the next day they provided contradictory answers, which resulted in more confusion, and which made me decide to take this case to the next level (the recorded interviewing and discourse analysis). In case 2, (see background 2, page 130) the participants were initially not at all surprised by my question and their accounts did not suggest that I was pointing at anything of interest to them. Their accounts were rather as determined as we would expect accounts from specialized people explaining matters to a non specialized person (myself). It was only when I pointed to contradictions that the one person made during the same conversation and to the variability of accounts across people that the issue started to be problematized, and only then the trivialized aspect of the issue came up to the surface; which made the need for recorded interviewing and discourse analysis more obvious. Case 3 (see background 3, page 202) was completely different from cases 1 and 2. The participants did not show any particular position (neither interest not disinterest). They rather did not understand where I was heading by my questions, simply because the issue is multi faceted and does not target a precise and tangible element of their research (such as the co2 usage for case 1, and the medium for case 2). Rather, case 3 is about whether or not they control their own research, in different regards. Clearly case 3 encapsulates many elements that are otherwise diffused. Only after long discussions with the participants that the issue of hyper and hypo entrustments was unpacked. Prior to the interview, although the discussions were increasingly illustrating that the research controllability was unnoticeably trivialized; the participants did not take any particular position and were apparently not interested to know where my questions

were heading. Interestingly, regardless of how each issue was first responded to by the participants, the 3 of them evolved in the same way during the next two phases: the individual and synchronic interviews, and the diachronic and group interview.

2) The individual level:

During the individual interviews, the participants were invited to personally reflect and account for the previously (but less formally) discussed issues. Right from the beginning of the interviews, I made it clear that accounts such as “this or that condition is a standard” would not suffice me, by responding “what is the rationale of this standard?”. Similarly, if the participants say “this is an existing knowledge”, I would reply “why do you need it? Or how useful is it for your own research”? Consequently, the individual participant embodied the whole lab and sometimes the whole cancer research domain, because he or she needed to explain the rationale of the research practices, for which they are not directly or personally responsible. I illustrated through numerous examples how they restlessly oscillated between the “cozy but imprisoning” structure and the “free but uncomfortable agency”. When the individual says that such a condition is a standard, they are certainly not the ones to blame, because the condition is beyond their control and is applicable to the whole domain; but when he or she fails to justify or explain it, he or she does no longer identify with it and would prefer to move to another position, where he or she is free to critique the first. And once he or she is well settled in this new position, he would notice that in reality much of his previous research was done differently (than how he or she now explains), which makes this new position equally uncomfortable. The oscillation between the two positions was less frequent than in the diachronic and collective interview, but was equally correlated with the contradiction that I pointed out. Following is the list of the discourse tools used by the participants to facilitate their own oscillation between the two contradictory positions: 1) legitimize the trivialization and align with the standard, the existing knowledge and the like; and 2) detrivialize the issue and distance themselves from the ongoing practice:

- Switching from the first personal pronouns “we” and “I” (also their equivalents “nous” and “je” in French) to the second personal pronouns “you” or sometimes “people” (also “tu” and “des gens” in French). I have detailed many examples in the 3 cases, where the different participants were describing how they do experiments, but when I pointed out a contradiction in the described method, they switched to the use of “you” which implies “you too” would do that and not only us. Sometimes the switch happened within the same sentence, signaling the switch from what was real and what was hypothetical and also the switch from factual to value judgments. Additionally, the use of “you” also marked that the individual participants were rather not sure about their answers and were trying to figure out a solution on the go, as opposed to the use of “we” or “I” when they were determined that their answer enables them to address the specific concern in question.

- The use of “also”, “sometimes” and “it depends” to switch between incompatible ideas. In the 3 cases, when I pointed out some problem in this or that position, participants switched to the exact opposite position simply by means of the words “also”, “sometimes” or “it depends”; suggesting that we can do things either way and that both ways are equally acceptable, when they are clearly incongruent.

- “Yes but no”: an interesting strategy used only by one, but in all the three cases is the following: he used “yes” all the time, and switched to “no” when the interview time was almost up. He was remarkably receptive to any concern that I raised and which problematized the ongoing practice, supporting his position with details that illustrate the exact problem, but all of a sudden he made opposite accounts that completely contradict all what he has been saying. This strategy was useful to suggest the following: “yes” I am aware of all that, but “no” I still prefer what we are doing here for other reasons that I cannot mention, because time is up.

- "No for yes": the opposite method of the previous one, is used by the lab members who were most of the time oriented to legitimize "the existing knowledge" negating any problem with it and mentioning its positive aspects, but at the end they concluded that "yes" there is a problem with the current system, which needs to be further researched, mentioning precise negative aspects that clearly have greater order of magnitude.

3) The group level:

The diachronic and collective interviews of the 3 cases were conducted on the same day. The participants knew in advance that the interviews were going to be about the same questions that we discussed individually during the synchronic interviews. No matter how divergent their accounts and positions were during the individual interviews, they dramatically converged during the collective interviews. The result was that they collectively and recurrently oscillated between two opposite positions, in 5 steps: 1) they first start by selecting and playing up a single aspect that shows that their practice (under discussion) is "fine", that there are no worries, and they end up there; 2) I point out another aspect that they completely ignored or played down (based on what some of them mentioned in the individual interviews to be of greater importance) inviting them to account for it; 3) they then account for the negative aspect of the discussed practice and consequently detrivialize the discussed issue; 4) once they achieve a completely different conclusion from the first one; one of them bounces up a new aspect that is clearly not scientifically based (not necessarily related to biology) but rather a value judgment type of opinion; 5) when I point out that this latter point is untenable or at least incongruent with the previous position, they step back, but no sooner do they take this detrivializing position than they restart a previous argument which they use to justify the discussed practice. Hence the oscillation between 1) and 5) was visibly recurrent and reflected the restless state of the group.

Following are the strategies used by the group to facilitate the oscillation between the two contradictory positions:

- When the first person responds to my question, the others ensure that they say “yes” “yeah” and “hum” during his talk, even before the speaker finishes stating his idea, which comforts him beforehand and tells him that his colleagues are supporting him and are not going to contradict his opinion.

- No one has at anytime critiqued his or her previous colleague’s opinion, and the subsequent talk was always to support the previous one. Nonetheless, interruptions and overlapping were too frequent, which brought about a general state of instability, during the interviews. Many times, a participant took the floor, but was interrupted by one of their colleagues after about 7 seconds, before he or she could even finish a sentence or formulate the simplest of ideas.

- When I point out a contradiction to a person, it is not him or her that responds, but immediately another person takes the floor and mentions a new idea, hence diverting the discussion from my point to another aspect of the issue. Later in time, the same person makes the same contradiction, and again when I point it out, another person interrupts and diverts the discussion to another idea, starting by the word “also”.

- The participants only disagreed twice during the 3 interviews. It was when I stressed that in the previous interviews the one participant said one thing and the other said the opposite. At that time, each of the two members started to defend his own position against his colleague’s. Paradoxically, no matter how they contradicted each other in accounts; they converge back to the same position: that the discussed practice is fine or at worst it has limitations like in any other domain, and that some peers out there must be working on improving it.

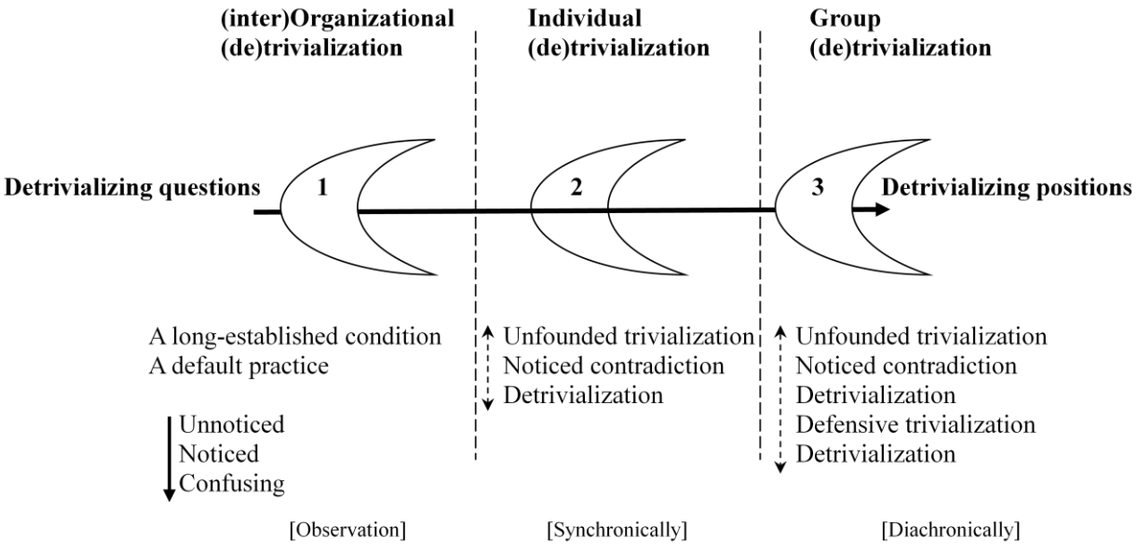


Figure 3. The (de)trivialization dynamic

4.2 Discussions

Within the framework of the sociology of scientific knowledge, the Actor-Network theory (Latour, 2005) constitutes a major breakthrough in the understanding of how knowledge emerges, how it is socially shaped, how it is contingent upon interactions, and how it depends on human and non human entities or factors. In the present section, I wish: 1) to discuss the importance of the theory and its relevance for our current research findings; 2) to situate the (de)trivialization dynamic in the broader picture of knowledge and knowing from the ANT's point of view; but more importantly, I wish to 3) emphasize that while ANT focuses on the shape of "structure" that produces knowledge, our research rather describes how knowledge is accounted for over and over again, in discourse (for a never ending "another next first time", in Garfinkel (2002)'s sense).

4.2.1 ANT and our research

I understand our cancer research lab -as an organization- to be altogether an Actor-Network nested in more and more ramified actors-networks. But where this lab's network starts and ends depends on how we conceive of an organization. From a CCO standpoint (Ashcraft, Kuhn, & Cooren, 2009; Putnam & Nicotera, 2008), the lab as an organization would encompass the largest communication practices and is not limited to the activities that take place within the building tagged "the lab". From this standpoint, the lab is not the container of communication, but is the communication itself. Communication is thereby: 1) interpersonal, inter-group, and inter-organizational; 2) it can happen face-to-face as it can be imagined, or through other means (telecommunication); 3) it is not limited to the geographical space of the lab and can be extended to wherever the "actants" (human and non human) travel physically or through imagination. The actants as human and non human entities and factors, when combined together they may make or become an actor- which has an organizing capacity within a specific network; 4) it is not limited to a specific period of time (e.g. From the day the lab was legally or fiscally established to the day it dissolves). It is rather much more extended in time and encapsulates the historical circumstances that acted together to get the lab to be established physically (e.g. the very first human and non human "actants" that shaped the idea of establishing a new lab, if ever such actants are traceable). Likewise, it is extended to the future and survives even

after the lab will no longer exist as physically or legally; because the lab as an actor-network will continue to act within the broader networks, but maybe in less structured or discernible way. From this standpoint, the actor-network is not graspable, but is imaginable. Hence, I can understand the lab to be an actor that acts within the network of oncology, or a bigger network such as the academic research, or a yet bigger network such that of knowledge-building networks at large. These networks are inter-connected, and the "actor" is only identifiable, after we situate ourselves (as observers) and be specific with regard to "acting in what?".

Now, after I considered the whole lab to be an actor that is acting along with a network (which is always contingent); within this same actor-network there might be multiple actors-networks. For example, we can imagine a lab member to be an actor acting along with a network of other people (external and internal) and other non human elements (example. machines and artifacts) to achieve specific goals to be identified contextually, on a case by case basis. From this standpoint, we do not know how many actors-networks there are "in" the lab, because a single person or artifact can every time incarnate a new role and become a new actor in a new network. Hence, an actor-network is only identifiable, after the "act" itself becomes observable and describable. Therefore, it is perhaps heuristically useful to start by identifying the "act" in order to be able to study its corresponding Actor-Network, rather than the opposite. Yet, the act can be a *will*, a *decision*, an *intention* that emanates from a person or more; but it can be a *condition* that imposes itself and is beyond the willingness or control of everyone. If it is a will, a decision, or an intention emanating from a person or more, then the actor is human. However, if the act is a condition that surpasses the humans and is beyond their control, then it is -at least partially- non-human.

Latour has faced strong criticism with regard to this latter idea of attributing the "faculty" of "acting" to non humans. Many critics did not accept- and sometimes ridiculed- the idea that a machine can be equal to humans and can act. Latour responded to this critique by stating that acting or being an actor does not presuppose intentionality; therefore a *machine-network* can act, without intention. In this case, I understand the machine, not as a corpse, but as what it represents to us (or to its own network). A machine in a cancer research lab can be transcended into a much

higher levels, intellectually and emotionally. When a researcher loves his machine or is very excited about the experiments' results that it gives out, he or she does not necessarily think about it in terms of being responsible or not, or whether it is conscious or not. Rather, the emotional effect exerted by the machine's behavior (even though it is humans that manufactured and calibrated it) is clearly much more powerful than just the "corpse". Perhaps the best portrayal of this idea is the example of the Pygmalion effect that causes performance to increase according to the expectations that humans give to things; such as in the Greek myth where a sculptor named Pygmalion fell in love with a statute that he had curved with his own hands. Hence the statute is no longer a statute, but a beloved, or may be a lover in the eyes of Pygmalion. The love that Pygmalion feels to be emanating from the statute, indeed acts and changes much in Pygmalion's life, and is thereby an actor, regardless of how "pathological" this relationship might be described. Similarly, it is perhaps important for me to try to understand what the machine represents to the researcher; what kind of relationship is built between the researcher and the machine. In other words, is the researcher able to critique the machine? When I invite the researcher to rethink his or her machine and respond to the question whether the machine does measure what it is supposed to measure (chapter 3), what the consequences of my questions would be on the researcher's expectations from the machines? If my question would trigger a new reasoning vis-à-vis machines, the researcher may face a disappointment emanating from the new doubts that machines may turn out not to be measuring what he or she has long supposed them to have been measuring. What are the emotional consequences of such a doubt? Is the researcher ready to face this disappointment, emotionally in terms of its implications on his or her own research? In chapter 3, I have detailed with numerous examples that the researchers: 1) did not doubt the machine's validity; and that 2) they did not respond to my numerous solicitations to rethink validity.

Non human actors (always hyphenated with a particular network) can be significantly more than a machine or a number of machines. They can be a combination of many "actants" of different types that act concomitantly and cumulatively, which goes beyond the control of everyone. Additionally, these actants can exercise different forms of power or can make implicit rules governing our systems of thought; a power that is buried away or "microphysical"

in Foucault's terms that it can only be identified through methodological excavation or archaeological search (Foucault, 1969). The example of the usage of the 5% level of co2 (case 1) encapsulates various "actants" that made the use of this level an obligation for the cancer research lab members. But this obligation has long been cantilevered (en porte à faux, in French), at least based on the participants' accounts and using their own words. The actants that led to the usage of this level (5%) were not identified, but the actor is well established: it is "the standard", as described by the participants. The standard is a non human "actor", because it is triumphally well established in this lab as in others, and it is visibly stronger than humans (the researchers) who are not able to change it, even though they accounted for the necessity to do so. I have detailed the researchers' accounts with regard to the negative consequences of standardizing this level in vitro, due to the need to mimic the in vivo human cells, which have different levels of co2 and not always 5%. I have also detailed that none of the lab members knew where this standard started. Furthermore, after long discussions, the lab director was the only one who provided a possible explanation on the origin of this standard, which is basically *business*. He said that the level of 5% "was dictated by the medium available commercially". This standard- being non human- has a (dis)organizing capacity (Vasquez, 2015 and Kuhn & Burk 2014) that surpasses the humans' (i.e. the researchers') will or knowledge. Yes, this standard was historically "decided" by humans elsewhere, but for decades other humans have been taking it for granted and have been adapting their research according to this standard, which imposed itself on biology research. By the same token, yes the atomic power was historically initiated by humans, but at moments the atomic plants can go out of the humans' control. At some moments, humans would not be able to control their own deeds, and thereby the atomic plants dictate their rules on humans and clearly disorganize our will and organize theirs (the atomic plants'). Consequently, humans who take decisions on what to do to avoid atomic explosions or how to restore things once disasters take place are greatly dependent on the atomic behaviors. The atomic example is an extreme case of determinism, where humans are clearly forced into what they might consider to be their own decisions (by a non human factor), but the case of the 5% standard is similarly forcing the researchers to adhere to it. While the former case (the atomic's) is perhaps easy to understand for everyone, because it is largely visible, the latter case (the 5% level standard) is less obvious without focused analyses such that I have done in case 1. In other words, it was only

through ethnomethodology and very focused interviews that the standard emerged as an actor and that this actor's rationale (or raison d'être) needs to be reconsidered.

4.2.2 The Actor-Network traceability

In line with ANT, Callon (1986) described through what is known to be "the sociology of translation", how some actors exert power on others and how this dynamic establishes and shapes an emerging Actor-Network. The study focused on the scientific-economic controversy regarding the decline in the population of scallops in Saint-Brieuc Bay (France) and followed the 4 identified actors along the negotiation dynamic, from which a new Actor-Network emerged, in 4 moments. The 4 moments, which are not necessarily sequential and may overlap, are: ''Problematisation'', ''Interessement'', ''Enrolment'' and ''mobilisation''. The 4 actors are: a) three French researchers who returned from a voyage to Japan, where they had observed a technique used to increase the stock of scallops ; b) the fishermen of Saint-Brieuc who are interested in the project of restocking the Bay, to address the economic consequences of the decline of scallops ; c) the scientific colleagues of the three researchers who are interested to advance the knowledge proposed (the project or the technique that the three researchers wish to implement) ; d) a particular species of the Saint-Brieuc scallops (Pecten Maximus), which may or not behave in the same way as the scallops observed in Japan did; i.e. they may or not respond to the proposed restocking: « 'accept' a shelter that will enable them to proliferate and survive » (p. 7). The objective of the study was to describe how "scientific knowledge" is being crafted or processed through "the progressive development of new social relationships" between the actors and the communities they (try to) represent (p. 5).

Unlike Callon's study, our current study did not trace the shaping and establishment of the actor-network and neither did it identify the "actants", beforehand. Rather, our study started from specific practices and identified the "actants" and analyzed the discourse of our participants, who were not implicated in the forming and establishment of the actor-network. The 3 practices that we studied in the current research (the 5% level of co2 in incubators, the medium usage and the researchers' controllability of their own research) are long established practices in cancer

research worldwide. The historical actors (i.e. those who first established the standard of the 5% level of co2 in the domain; those who prescribed the medium usage; and those who first compartmentalized fundamental cancer research in labs separately from clinicians and who first relied on external entrustments) remained unidentified. Furthermore, the actors that are currently reinforcing these practices are not proper to our studied lab (this is why our study is totally transposable to other labs). Nonetheless, our study was able to demystify the traces of the «actants» (not the entire actors-networks) of these practices (tables 1, 2, and 3), based on our participant observation and especially on our discourse analysis. More interestingly, our study was able to demystify Callon's (1986) 4 moments mentioned in the previous paragraph ; not as historical or sequential phases that happened during the initial establishments of these practices, but as they are currently reiterated. Therefore, I will now describe these 4 moments as they apply to our own research. The aim of the description is to emphasize that these 4 moments are *sustainable*, as they are practiced again and again, by new actants:

1) **«Problematisation»**: the level of 5% of co2 has emerged during the interviews analysis as an obligatory passage point, OPP (Callon, 1986). The participants did not know where this standard started and said that the level itself is not their aim, but that in order to keep the cells at a pH of 7.4, the 5% was found to be the optimal level (without mentioning who found that). Furthermore, the participants accounted for the negative consequences of this level. The lab director concluded that the level was decided by the commercial providers and did not emanate from research. He said that about 50 years ago, when research needed to have a medium able to maintain the cells at 7.4 pH, the providers satisfied this need, but dictated the use of the 5% level of co2. More importantly, our participants defended this level as an OPP, not as a strategy to become (themselves) indispensible like Callon's case of the three researchers, but to reinforce the indispensability of the historical actors of this practice (i.e. the commercial providers if we accept the story of the lab director). To do so, they trivialized the negative consequences of the use of the 5% level of co2, which they have already described to be negative, playing up 2 other elements to defend their practice or OPP: a) that they need standardization, without being able to respond to my concern: why accept to standardize a condition that

they say to be potentially skewing? And b) that they need to replicate experiments, without being able to respond to my concern: why replicate experiments with 5% in particular and not just replicate the level mentioned in the research protocol, after deciding which level is suitable to the cells being treated? One interesting detrivializing account made by Brian – a post doctoral researcher- suggested that it is now high time to leave this OPP aside and try something else: "may be now yes (.) because it's a it's a long time it was set up and we don't question these things (.) but I think in the past they were discussing a lot (.) a lot".

With regard to the medium usage, the participants defended their medium usage describing it to be satisfactorily mimicking how the blood feeds the cells in the human body. Their initial accounts were rather determined and suggest that their medium usage is an OPP. However, when I discussed details of how it can be able to mimic the blood while it is constant and not flowing, they then started to account for the weaknesses of this usage, framing it as a limitation, like any research has limitations. When I mentioned the alterative flow system, they then accounted for the positive effects of getting the medium to flow, and the negative effects of their current usage. Additionally, when I asked about the auto-contamination risk emanating from the current usage, they initially denied it, except one ; then they acknowledged it, except two of them. The two of them categorically refused the idea that the current system can cause auto-contamination, simply because cancer is for them not a matter of molecules and cannot be contaminated by molecules. This latter idea was said to be wrong by all the participants during the collective interview (i.e. cancer was decided to be possibly molecular and the medium usage can cause cancerous molecules to contaminate non cancerous cells). Finally, the participants said that they trust the medium providers, without accounting on the basis of such trust. After long discussions and with a lot of questions from my part, I ensured that their trust is not scientifically based, but rather a common sense type of decision. The participants defended the way they deal with the medium providers. They said that they are trustworthy and professionals and therefore, they do not need to check after them, although they said that if the providers would alter the medium, then the consequences would be open to any risk. The problematization they proposed, is that the lab is not able to

consistently produce a medium as accurate and precise as the providers', but the gap is in their proposed OPP: the "blinded" purchasing of the medium, or not being able to control one's own medium.

Finally with regard to the issue of the researchers' controllability of their own research, the participants trivialized the situation that almost all the consumables that they use are purchased readymade, even though they say that every consumable can skew their experiments, if it is not as it is described and that they are not able to find out the possible alterations, except by chance or when they make part of the experiment's tested variables. In terms problematization, the participants stressed on that the lab is not able to produce what providers produce, and that the providers are very professional and very trustworthy. The participants portrayed the excess of entrustments to be an OPP, excluding any other alternatives to enable them to control their own consumables (for example the possibility that these consumables become controlled not by the providers themselves, but by a newly established organization within the purview of research and academia and not completely independent or profit-driven). At the other end of the continuum, the participants initially did not defend the lack of entrusting to other specialists within the scope of oncology (especially clinicians whose contribution was described to be a must not an option to be able to understand cancer from the earliest stages of in vitro experiments as well as in vivo) and acknowledged the need to open up cancer research labs and get rid of the current compartmentalization. Yet, when I asked why they are still up to date working separately from clinicians (i.e. all lab members are biologists and none is a clinician), they either diverted the discussion to the future, by saying we should do better in the future, or making contradictory accounts inferring that they do not necessarily need clinicians. Further discussions, especially during the collective interview suggested that the participants still frame the current compartmentalized way of doing research is an OPP (i.e. they have to proceed as they are proceeding now). Only one lab member grew critical to this compartmentalization and accounted for the need to synergize oncology research both at in vitro and in vivo stages, by involving all types of specialists at all times, and by strategically relying on face-to-face meetings and discussions. The lab member justified his opinion by mentioning that his previous

lab in the US was so doing. Yet, as I have detailed in the diachronic analyses of our 3 cases, the participants recurrently oscillated between the one position and its opposite, or as Callon 1986 suggested (following the etymology of the term: inter-esse) to be in between. In the next paragraph, I will detail how the participants stood in between two positions.

2) **«Interessement»:** While Callon (1986) studied a case where particular actors (the 3 researchers) used strategies to get the others to be in between two positions in order to facilitate their transit to the «enrolment»; in our study the participants are already predisposed to play their role and be mobilized to defend their long established practices. The detrivialization triggered by our interviews' questions provoked a state of restlessness that I detailed in each of the 3 cases illustrating my analysis with the various discourse elements used by the participants: repetitive hesitation, interruption, overlapping, false starts, sudden higher pitch, omission, specific use of personal pronouns, contradictions and recurrent evasive accounts. During interviews, the participants have on the whole been in between: a) providing a genuine reflection (on what should be done) and b) preferring the comfortable position of where they are (what is now being done). Without our provocation, the participants' overall accounts illustrate their preference to the comfort of where they are right now over the discomfort of the new complicated thoughts of what ought to be done. The situation is very similar to peoples' resistance to change or why they first reject it, as it disturbs their tranquility; such as described by Kanter 2012 (loss of control, excess of uncertainty, surprise, everything seems different, loss of face, concerns about competence, more work, ripple effect, past resentments, real threat). In our synchronic and diachronic analyses alike, I detailed numerous examples, of how the participants were not only making accounts on the object of thought (co2, medium, and excess or lack of entrustments), but they were also reflective about the consequences of the new ideas. I correlated this double thinking with their recursive and unjustified return to square one (as we say in French: retour à la case départ) over and over again. Square one provides the lab members with the security of ''sheltering'' in their positions legitimized by the respective roles to which they have been assigned: enrolment.

3) **«Enrolment»**: In our study, the participants are not being "enrolled"; rather their roles are very well established. Our master's student enjoys the comfort of being considered a learner. She could express her own position, but when she is confronted to contradiction, she then can say "I don't know [ ] maybe if I ask I will know" inferring that being a master student it is acceptable not to know, but others should be able to explain. Similarly, a technician has the possibility to think about her own daily operations, without major concern to be critiqued, because as a clinician she is "enrolled" to apply what researchers decide, not to rethink the system. Additionally, when the technician was "inter-ested" (in a position of inter-esse) to defend her own role (job), she made accounts totally different from all her colleagues'. When I asked about the quality of the purchased medium, all the participants said that it is almost perfect as compared to the one made locally; but the technician was the only one who said that her own medium is better and that in industry products vary. From his end, the PhD student legitimized his own practices, by the fact that a doctoral student has to master the basics of researcher and apply the existing knowledge, inferring that it is not yet the time for him (in his current role) to go further. The post doctoral researchers, whenever they are confronted with contradictions or ungrounded claims, they resort to the "existing knowledge" stressing that the researchers' role is to adhere to the standards and be part of the broader research community, rather than try new things (even when they accounted for the necessity to try these new things). The lab director, who has a distinguished scientific authority, used his role to infer that he is aware of the reasons and that he knows the risks of their practices (regarding the 5% level of co2, the medium usage and the over and lack of entrustments), but these risks are calculated, without accounting how. He used most of the interview time to express that these practices have risks and negative consequences on research, but suddenly at the end of the interviews, he said "it's minimal, it's minimal" without further explanations, because time was already up.

4) **«Mobilisation»:** Again, unlike in Callon's (1986) case, our research did not observe (witness) how some actors used particular processes to mobilize others to speak in the name of others. Rather, our participants are self mobilized to speak in the name of the "standards", "the existing knowledge" and the "previously

validated" research practices, even at the expense of their own opinion (as they had accounted for their skewing risks emanating from the standardization). It was not clear why they did that, but it was totally clear how they did it (see sections 3.3.2.3; 3.3.6.3 and 3.3.10.3 in particular). Particularly during the diachronic interviews, the participants were clearly prone to defend their practices. They embodied the rules, no matter how constraining they had said they were. No matter how they were contradictory during the individual interviews, the participants completely converged into one point during the diachronic interviews: that the current practices are fine. To do so, no one contradicted his or her colleague, except when I personally put it boldly that the one had previously said one thing and the other said its opposite. They never started with exposing the complexity of the discussed issue, and preferred to answer with short answers, even though we have long discussed such complexity during the individual interviews. It was always me who oriented the discussions into the controversial issues (hence trying to open Pandora's box, as per Latour, 1987), while they were oriented into the "the controversy settlement" (Latour, 1987 and 2005). Every time, I opened up these controversies, interruptions, overlapping and higher volumes became significantly apparent. Many times, the one interrupts his or her colleague even before this latter finishes one sentence or a simple idea, hence cuing him or her and orienting the discussion into a specific direction. Many times, when I addressed the question namely to a particular participant, another one takes the floor and answers on his or her behalf, especially when this latter was inconsistent with his or her previous accounts. Additionally, when inconsistent ideas were pointed out by myself, each time one of the participants diverted the discussion into other points, just by means of «also» and «sometimes». They switched the discussion off topic, just using one of these two words at the beginning of the sentence. Finally, a number of discourse elements suggested that their mobilization has something to do with me, as an outsider. On many occasions, I detailed how the participants were reflective about my own opinion, assuming that I am trying to convince them to change their practices. Hence mobilization emerges in our case to be a sustainable condition ; as it is not only triggered by the historical actors (such in Callon 1986), but fueled by contextual factors (possibly for face saving or in order to defend one's domain, with which they identify compared with outsiders to the domain).

4.2.3 From Callon's "mobilisation" to Cooren's "Ventriloquism"

Cooren (2010) introduced the metaphor of ventriloquism to portray the idea that people speak on behalf of others (humans and non humans), and reversely use them to speak on their behalf; as ventriloquists would subtly and skillfully do along with their puppets. While Callon (1986) too does not exclude the role of non humans in this dynamic, he is oriented into the description of the actor-network during the construction or structuration (through the 4 translation moments described in the previous subsection). Cooren (2010), who argued that research analysts should not care about "structures" and should focus on describing practices, widened the scope of the mobilization, in space and time. Cooren's examples illustrate the idea that mobilization is a communication practice that can happen anywhere and anytime; and is not limited to the early stage of the construction of actors-networks. Cooren's example of the hospital director who mobilized the "protocol" to subtly claim his own authority to reject or negotiate his 2 interlocutors' suggestion; illustrates how he speaks on behalf of the protocol (and is thereby keen on respecting it), but at the same time puts his own opinion in the "mouth" of the protocol (i.e. it is the protocol that may not accept the suggestion made by the interlocutors).

In the 3 cases that we studied in the current research, Ventriloquism was the ultimate technique used by our participants to explain –and sometimes justify- their practices. In the following paragraphs, I will detail how ventriloquism was used as a discourse strategy to explain or justify the trivialization of the negative consequences emanating from the 3 practices under question (the standardization of the 5% level, the inappropriateness of the medium system, and the excess or lack of entrustments):

In case 1, the 5% level of co2 was said to be a "standard", to which participants gave the floor; i.e. from the moment the practice was framed as a standard, the participants "warned" me that if my question was intended to be a critique, then the critique would be applicable to the whole domain, not only to their own lab. Interestingly, only one participant used this argument (that the 5% is a standard) upfront; and then started to explain the rationale of such standardization. All the other participants first explained or justified this level; when discussions resulted in

contradictions and inconsistencies, they then mentioned that the standard is responsible for this situation, stressing on the need to have standards in order to make “tidy” research (sections 3.3.2.1 and 3.3.2.2). Hence, no matter how detailed the discussions were, or how divergent the initial arguments made by the participants; the final destination of the discussion is that the standard is responsible for this 5% level (i.e. regardless of whether the participant justified the practice or not). Therefore, such explanation or justification is a form of ventriloquism, which is identical to Cooren (2010)’s example of the hospital director who used the “protocol” to refuse or negotiate a proposal made by his interlocutors. Using the participants’ accounts, I have thoroughly detailed in sections 3.3.2.1, 3.3.2.2 and 3.3.2.3 that the standardization of the co2 level can have negative consequences on their research.

Another form of ventriloquism related to the same case is the following: the 5% level is being used to allow for other labs to replicate this lab’s experiments. By mentioning this reason, the participants are talking on behalf of other labs that may eventually need to replicate an experiment made by this lab. Paradoxically, other labs (if they happen to be interested to replicate a research that is confirmed to be successful or worth replicating) would use the research protocol and see whatever level of co2 mentioned, not necessarily the 5%; especially that the participants said that this level can have negative consequences on cells that may require other co2 levels. Thus, using this reason too is a form of ventriloquism, because the participants spoke on behalf of other labs and mentioned an excuse that has no raison d’être.

With regard to the medium usage (case 2), no matter how divergent the initial discussions were regarding its constancy (not flowing as the blood in the body), all the participants ultimately resorted to the following reason: the unavailability of better technological solutions. Therefore, the drawbacks of the current medium usage are not their own fault, but that of those who are supposed to develop better technologies. Those people who we do not know who they are in particular are the ones to blame, because they are still unable to provide labs with better technologies. Obviously, it is up to biologists to express their specific needs (in getting their in vitro models to better mimic in vivo conditions) not to other professionals (such as mechanical or technological engineers) to imagine these needs and

design better medium usage systems. Interestingly, when I reminded them of the already existing alternative system (the flowing system which they themselves mentioned to me, prior to interviews), they then said that this system is expensive, and that they do not necessarily need it all the time; and from there switched to completely contradictory accounts: that they do not always need to mimic the in vivo conditions, and that the current system is sometimes better than the "yet to exist" systems (see discussion in section 3.3.6.3, on page 164 in particular).

Now, with regard to the trust that the participants accounted for vis-à-vis the medium's providers; when I pointed out the risk of advertent or inadvertent alteration of the composition (which they admitted to be serious), all of them immediately spoke on behalf of the providers. They all stressed that the providers are: professionals, serious, competent, and above all trustworthy and have no reason to cheat them. I could not imagine better spokespersons for these providers, although as customers (especially that they are researchers), I expect them to be prone to doubt than to reassure others and dissipate their doubts. Obviously, the participants' accounts in this regards, had no scientific bases and were only common sense type of accounts (see section 3.3.6.3, on page 168 in particular).

A less obvious case of ventriloquism is related to the excess or lack of entrustments (case 3). The participants (researchers) justified their entrustments of their own research to machines by the fact that they "are expensive", that "people who made them are not stupid", and that they "have been validated by others". Hence, instead of accounting for how (or whether or not) they ensure that the machines do measure what they want them to measure, the participants gave the floor to the following parties to tell me why machines are trustable: a) their prices, b) the intelligent people who made them; and b) the intelligent people who validated them. With regard to the excess of entrustments, ("90%", "95%", "everything" is purchased readymade, according to them), all the participants stressed that there is no risk from this situation. This time, even though the discussion was not about particular providers (unlike in case 2), but rather about all their providers at large, the participants again assured that all of them are professional, competent, trustworthy and have no reason to cheat (see section 3.3.10.3, in particular); which is counterintuitive. Basically, what the participants did is that they merged all their providers altogether and voiced

them at once. To adapt Cooren's (2016) use of the term polyphony, the participants converted the naturally polyphonic providers (i.e. they are plural) into a monophonic voice (they became one single voice). Hence, the ungrounded trust was defended first by merging the plural into singular and secondly emphasizing that this merged entity is perfect. If this is not an obvious case of ventriloquism, it is clearly a case of mobilization in Callon's (1986) sense; because the participants are not only speaking in the name of their providers and are also blending them together.

I will end this discussion with the ventriloquism used to justify the lack of entrustments. When I asked the participants why they keep omitting to entrust parts of their research to specialized people (especially clinicians), although they said that their contribution is a must to be able to understand cancer, one of them said that clinicians do not have time to work with them (see section 3.3.10.3; page 267). While the discussion- which started from the need for others' expertise, as they had expressed- was about restructuration and de-compartmentalization of cancer research (and not only about inviting clinicians in their spare time), this participant who was the least experienced during the collective interview was the first to decide that clinicians do not have time. Her colleague responded to her by saying that in his previous lab in the USA, diverse specialists (including clinicians) used to work together during all the stages of cancer research, and that they did not totally separate between biology research and clinical research; hence they completely de-compartmentalized the oncology research. Once more, saying clinicians (in general, not specific persons) do not have time to work with us (as if she was referring to a couple of people that she knows), is a form of ventriloquism, similar to the previous case. It builds on: 1) merging all the clinicians that we can imagine into one entity as one person; and then 2) claiming that they do not have time to work with us. Again, if this is not an obvious case of ventriloquism in Cooren's (2010) sense, it is clearly a case of "mobilisation", in Callon's (1986) sense. Whether it is "ventriloquism" or "mobilisation", it was strategically used to justify the trivialization related to the lack of entrustment.

4.2.4 Research merits and limitations

In this section, I will outline the philosophical and methodological merits and limitations of this piece of research, detailing the pertinence of our findings in enriching the debate on what makes knowledge and knowing.

In line with the Community of Practice (CoP)' perspective, this piece of research favors the gerund *knowing* over the noun *knowledge,* not to confuse it with *information* and *data* (Kuhn, 2014). It is oriented into the action of knowing as opposed to knowledge as a product. Therefore, it crosses 2 main areas of interest: 1) the learning theories, which have long been focusing on how humans learn and how to improve teaching (i.e. oriented to didactic and pedagogical purposes); 2) the sociology of scientific knowledge and the philosophy of science, which have long been interested in how scientific knowledge is or should be constructed (using group deleneation or by critiquing the scientific reasoning). However, our perspective favors to enlarge the scope, by being interested in how humans know at large (i.e. not being focused on how people (intentionally) learn or how to teach; and not necessarily to critique the scientific reasoning). Hence, our perspective is rather interested in observing and describing how *knowing* happens both intentionally and unintentionally, which is why *knowing* is larger than *learning*. Because I understand communication to be omnipresent, or as Bateson 1972 put it "one cannot not communicate" (i.e. an individual or a group of individuals is always in communication, alone or accompanied, intentionally or not, being present or absent, physically or imaginatively); I understand that the action of knowing takes place within communication (i.e. simply because there is no room named "no communication" where one can go to learn leaving communication outside). Therefore, the epistemological originality of this research is in its approach to knowing and communicating as inextricably related. I remain skeptical about whether we can distinguish between the two concepts or why we should do so, because from this stance I cannot imagine a knowing act that is not at the same time a communicating one.

Narrowing it down to *knowing in the scientific context;* our research which singled out a cancer research lab as an example of organizations whose main task is to produce knowledge happens to be situated within the framework of Organizational Communication. From this perspective, our research is well positioned to enrich the

Organizational Communication debate on knowledge and knowing in a highly situational context. Although knowledge and knowing in any organizational context are gaining increasing interest within the framework of Organizational Communication (Kuhn & Burk, 2014), cases such as research labs are of particular importance, due to their special focus on learning and knowledge advancement. Furthermore, the current research introduced the idea of studying the (de)trivialization practices as a strategy to gain insights about knowing in organizational contexts. I argued that the (de)trivialization practices are key discourse elements, as they reveal the bases on which individuals, groups and organizations prioritize matters and trivialize others.

However, one limitation of this research is its inability to record and analyze authentic conversational interactions made by our participants, as they construct knowledge in their daily activities. This limitation emanates from 3 main reasons: 1) the participants' daily interactions are very brief, too technical and encompass too much inferencing, which I found inconvenient for recording and transcribing; and if I happen to do it, it would not be accessible at least to readers from my research area (communication). As a matter of fact, I recorded a number of meetings, but I later realized that transcribing them was virtually impossible, for me; 2) I did not request the permission to record the researchers during their informal discussions (at work, but outside meetings), because I knew through my observation experience that such a practice would bother them intensely and may result in their revoking of my access to the lab (especially that I never had an official permission to be in the lab); 3) I ended up to realize that trivializations do not necessarily happen in discussion; they are rather more implied (internalized). When people trivialize a matter (i.e. when they implicitly or unnoticeably ignore it), they do not necessarily talk about it. They rather talk about what they favor (i.e. what they play up or practice).

Therefore, the research compensated for this limitation and was able to transform this shortcoming into strength. It used Garfinkel (1967)'s breaching experiment to exteriorize what the participants internalize (assumptions) and which I found to be governing their practices. For instance, prior to this research the participants happened to trivialize the importance to discuss the use of 5% of co2 in cell culture or to discuss the medium's

provider trustworthiness, focusing on other issues; but also implying ideas on why it is important to keep the 5% level and why they trust the provider. If, I would try to change the co2 level or get the purchased medium to be checked in-house, I would at least expect them to tell me not to do so, followed by an explanation (i.e. exteriorization of what they have been assuming about these two situations). Hence, exteriorizing the internalized instead of being contented with the verbal exchanges, emerges as one of the merits of this research. But because I am not entitled to touch any device or change anything in the lab, my breaching experiment (as an exteriorization tool) was limited to questions.

Another merit of the focus on specific trivialized matters (our 3 cases) is the possibility to switch between 3 levels of analysis where we can capture knowledge construction: a) the (inter)organizational level (trivialization as an observed common practice happening all the time and everywhere in this lab and as reported to be a standard shared by other labs); b) the individual level (trivialization as a discourse practice that happened during the individual and synchronic interviews); and c) the group level (trivialization as a discourse practice that happened during the collective diachronic interviews). Zooming in and out on knowledge construction at the 3 levels is an interesting technique, as opposed to studying learning at one level assuming that one level can be sufficient to know about knowing. More interestingly, this study did not care about these 3 levels beforehand. It analyzed discourse inductively and these levels emerged along with the patterns. Moreover, our analysis did not try to categorize participants, except when groups emerged (momentarily) in terms of how they accounted for a particular question.

To respond to Latour (1987)'s call to study science in its habitat (during construction) and to go beyond Latour & Woolgar (1979)'s research, which they themselves conducted within a biology lab, this study combined observation with discourse analysis as a unique strategy to access and document the scientific discourse construction. The scientific discourse is by excellence exclusive; as it does not allow outsiders to discuss it. However, interviewing and recording scientists in situ, and using their own words to support the analysis is a unique way to minimize flaws emanating from the researcher's own interpretation of the participants' accounts. The current research analysis was

strategically oriented into richly providing verbatim accounts for every idea that I tried to outline, leaving limited doubts that the analysis is based on my own interpretations and that the participants did not meant to say what I said.

Additionally, during the interviews, I ensured that I understood the participants' accounts quite well, by saying what I understood from the participants' accounts and asking them to confirm or reverse that what I said was what they wanted to say. I also asked the same question in different ways, when I happened to hear opposite accounts. Moreover, when I noticed inconsistencies or contradictions, I stated them during the interviews to solicit the participants' responses about them, which I made sure to use in the analysis, as a way to validate my conclusions or rebut an idea. Also, during the collective interviews, I told to the participants about the accounts variations that I captured in the individual interviews, asking them to account fror my conclusions (whether they were in fact contradictions or that I misunderstood them). In line with Latour & Woolgar (1979), the current research found that the scientific discourse is not purely scientific, but that it also encapsulates common sense and value judgments. It is sometimes very hard for the scientists to distinguish between the two, because they are tightly intermingled. Hence combining ethnomethodological observation with interviewing and focusing on trivializations in situ was from our standpoint a unique strategy to analyze science under construction.

5. Wrap-up

1) I started this study from the idea that Communication as an area of interest happened to be established after a long history of: a) learning theories; and of: b) fossilized misconceptions and simplistic views of human communication. This heavy legacy has made it difficult for the Communication discipline to deal with knowledge and knowing on an equal footing with philosophy, psychology, and to a lesser degree sociology (section 1.1).

2) I then unpacked my epistemological standpoint with regard to the inextricable relationship between knowledge and communication, by disclosing the following: a) my philosophical foundations (social constructionism) assuming that knowledge is a collective construction that we build inter-subjectively ; b) my views about human communication, especially with regard to its organizing characteristics and how it is inextricably related to knowledge and knowing (CCO); and c) my theoretical framework of inquiry, especially with regard the usefulness of discourse analysis in understanding aspects of knowledge and knowing- Discursive Psychology (section 1.5).

3) In the literature review, I focused on the major contemporary learning theories starting from the early roots of Pavlov's (1927) physiologist experiments up to Kuhn's (2014) outline of the Organizational Communication contribution to the knowledge theories. The aim was to trace back the progression of how knowledge construction has been described in the dominant literature of the learning theories; from behaviorism (conditioning), to cognitivism (transmission), then to constructivism (construction), humanism (learner centered construction), up to social constructionism (intersubjective construction). This latter perspective paved the way for: a) distributed cognition, b) the sociocultural approach; and to c) actor-networking, which made the inextricable relationship between knowledge and communication more obvious (section 2.1).

4) From there, I emphasized the need to study knowledge and knowing, as a communication construction, building on a number of different approaches that reinforce this orientation; i.e. Community of Practice, CCO, dialogic learning, Luhmann's model, as well as a myriad of distinct studies (section 2.2).

5) Then, building on a number of past research findings reporting how 'scientific' knowledge can be compromised or distorted (Latour's 1979 and 1987), Callon's (1986), and how knowledge construction is exposed to pitfalls (Groupthink, triviality law, circular reasoning, cognitive dissonance, Feyerabend's dogmatic and sectarian practices of 'science' etc.), I questioned the usefulness of studying the trivialization and detrivialization in a specific context: an organization whose main job is to produce knowledge. I decided that a cancer research lab would be a judicious case, where the trivialization of ideas can be observed, and where detrivialization can be provoked (section 2.3).

6) I defined *triviality* and *trivialize* distinguishing between 3 uses of the term (a. common sense, b. insignificant and c. popularization), and identified the etymological meaning; i.e. trivia, trivium and trivialis (section 1.3). Then, building on Latour's (1979, 1987, and 2005), I wondered how *trivialization* would function within this cancer research lab. Using Callon's (1986) translation theory model, I enquired how I would trace back knowledge building over the long established trivialized ideas, and how I would distinguish between the scientific and non scientific constructs intermingled together (section 2.3).

7) Drawing from the bases of my theoretical foundations (Discursive Psychology- Potter & Wetherell 1987), I favored the ethnomethodological approach (Garfinkel 1974) and identified various cases of trivialization, through participant-observation. I then focused on the identified cases and asked more specific questions as per (Garfinkel, 1967)'s breaching experiment, to: a) explore the underlying dynamic behind the identified trivializations and to: b) study the emergence of new detrivializing accounts as triggered by my questions (3.2.1 and 3.2.2).

8) I then singled out 3 cases, based on the following: a) the transposability of the 3 trivialized issues; and b) varying cases according to the participants' first reactions to my breaching questions: case one was first received by a remarkable surprise, case two was first said not to be trivialized; and case 3 did not seem to be understood by the participants (section 3.2.3).

9) After gathering a number of contradictions and inconsistencies for each case, during the observation- which suggested that these trivializations have been hiding away a number of controversies- (sections: 3.3.1; 3.3.5 and 3.3.9; respectively)- I conducted semi-structured tape recorded individual interviews with the 10 lab members, which I analyzed synchronically. Approximately six months later, I conducted a collective tape recorded interviews over the same 3 issues with 4 lab members, which I analyzed diachronically. The analysis was conducted using Potter & Wetherell (1987)'s methodological tools.

10) Using the individual interviews, I identified a number of variations of accounts across persons and a number of inconsistencies made by the same person during the same interview. I detailed where 'scientific knowledge' is interwoven with ungrounded knowledge: 'common sense' and 'value judgments'. I also identified how the individuals' accounts and positions regarding the issues (previously trivialized) were developing on the move in relation to my questions or to how the ongoing discussions were progressing. I additionally, identified a number of discourse strategies used to avoid dissonance. Hence the detrivialization prompted by our research triggered new accounts and positions, which have important implications on the participants' own research (case 1: section 3.3.2.1 and 3.3.2.2; case 2: sections 3.3.6.1 and 3.3.6.2; and case 3: section 3.3.10.1 and 3.3.10.2).

11) Using the diachronic and collective interviews, I identified the same variations and controversies identified synchronically and individually. The interminglement between 'scientifically grounded knowledge' and the 'common sense' and 'value judgments' that emerged during the synchronic interviews persisted in the diachronic interviews, and the ungrounded trivialization practices in discourse grew stronger. Hence, depending on my

questions, the participants recursively and restlessly oscillated between trivializing and detrivializing positions. I additionally outlined a number of dissonance avoidance (Festinger 1957 and 1962) strategies used by the participants to bypass controversies without settling them (case 1: section 3.3.2.3; case2: section 3.3.6.3; and case 3: section 3.3.10.3).

12) I then detailed the elements of each (de)trivialization (who trivialized what, why, when, where, and how?) in tables 1, 2 and 3 respectively (case 1: section 3.3.3; case 2: section 3.3.7; and case 3: section 3.3.11). Based on my analyses, I drew 3 main clusters of findings illustrating: a) the knowledge compromises correlated with the 3 trivialization practices; b) the knowledge emergence correlated with the 3 detrivializations initiated by our research; c) the usefulness of ethnomethodology and discourse analysis in understanding aspects of knowledge building (section 4.1). In figure 3, I described the (de)trivialization dynamic in terms of 3 levels: (inter)organizational, individual and group (following the chronological sequencing that I undertook: observation, individual and then group interviewing). Figure 3 portrays the back and forth movements of trivialization and detrivialization at each of the 3 levels.

13) In terms of discussions (section 4.2), I first related my research findings to the Actor-Network theory (Latour, 2005) and particularly with regard to the non humans' role in the process of knowledge building. I additionally, traced the elements of what Callon (1986) called the sociology of translation's 4 moments, in my own research findings. I argued that these 4 moments are in my current research not triggered by what I called the "historical" actors, but can presently be sustained by the current "actants" humans and non humans; at the 3 levels: individual, group, and (inter)organizational (sections 4.2.1 and 4.2.2).

14) I established a link between Callon's (1986) "mobilization" and Cooren's (2010) ventriloquism and detailed a number of examples where my participants practiced obvious and less obvious cases of ventriloquism and or mobilization. The cases illustrate how humans use discourse to speak on behalf of others and how they make

others speak on their behalf to achieve specific goals. I detailed how the use of mobilization and or ventriloquism was strongly correlated with the trivialization practices (section 4.2.3).

15) Finally, I concluded with an outline of this research merits and limitations at the phiplosophical and methodological levels and in terms of the pertinence of our findings. From there, I stressed on the importance of examining trivializations as a strategy to enrich the debate on how to better study knowledge and knowing at the organizational level (section 4.2.4).

Glossary of biology terms [58]

Acidity: The level of acid in substances such as water, soil, or wine. Acid is a substance with particular chemical properties including turning litmus red, neutralizing alkalis, and dissolving some metals; typically, a corrosive or sour-tasting liquid of this kind.

Bacteria: A member of a large group of unicellular microorganisms which have cell walls but lack organelles and an organized nucleus, including some which can cause disease.

Cancerous: Affected by or showing abnormalities characteristic of cancer: a cancerous tumour. If stem cells are unstable, they can become cancerous.

Cell: The smallest structural and functional unit of an organism, which is typically microscopic and consists of cytoplasm and a nucleus enclosed in a membrane.

Cell culture[59]: the maintenance and growth of the cells of multicellular organisms outside the body in specially designed containers and under precise conditions of temperature, humidity, nutrition, and freedom from contamination. In a broad sense, cells, tissues, and organs that are isolated and maintained in the laboratory are considered the objects of tissue culture. The techniques of cell culture have allowed scientists to use cultures of cells for experimental studies and for biological assays of many types.

Cell line: A cell culture developed from a single cell and therefore consisting of cells with a uniform genetic make-up.

[58] Downloaded from : http://www.oxforddictionaries.com
[59] Downloaded from : http://www.britannica.com/

Co2: A chemical formula of a colorless, odorless gas produced by burning carbon and organic compounds and by respiration. It is naturally present in air (about 0.03 per cent) and is absorbed by plants in photosynthesis.

Cultivate: to grow or maintain (living cells or tissue) in culture.

Culture: A preparation of cells obtained by culture.

Dish: (see *Petri dish*, below).

DNA: Deoxyribonucleic acid, a self-replicating material which is present in nearly all living organisms as the main constituent of chromosomes. It is the carrier of genetic information.

Fungus / Fungi: Any of a group of unicellular, multicellular, or syncytial spore-producing organisms feeding on organic matter, including moulds, yeast, mushrooms, and toadstools.

Gene: A unit of heredity which is transferred from a parent to offspring and is held to determine some characteristic of the offspring.

Hood*: An extractor hood*, one that removes particles usually by sucking them up.

In vitro: (Of a process) performed or taking place in a test tube, culture dish, or elsewhere outside a living organism.

Incubator: An apparatus used to hatch eggs or grow microorganisms under controlled conditions.

In vivo: (Of processes) performed or taking place in a living organism.

Medium: The substance in which an organism lives or is cultured.

Metabolism: The chemical processes in living things that change food, etc. into energy and materials for growth.

Metastasis: The development of secondary malignant growths at a distance from a primary site of cancer.

Molecule: A group of atoms bonded together, representing the smallest fundamental unit of a chemical compound that can take part in a chemical reaction.

Petri dish: A shallow, circular, transparent dish with a flat lid, used for the culture of microorganisms.

pH: A figure expressing the acidity or alkalinity of a solution on a logarithmic scale on which 7 is neutral, lower values are more acid and higher values more alkaline.

pH meter[60]: electric device used to measure hydrogen-ion activity (acidity or alkalinity) in solution.

Pipette: A slender tube attached to or incorporating a bulb, for transferring or measuring out small quantities of liquid, especially in a laboratory.

Proliferate: Reproduce rapidly.

[60] Downloaded from : http://www.britannica.com/

Protein: Any of a class of nitrogenous organic compounds which have large molecules composed of one or more long chains of amino acids and are an essential part of all living organisms, especially as structural components of body tissues such as muscle, hair, etc.

RNA: Ribonucleic acid, a nucleic acid present in all living cells. Its principal role is to act as a messenger carrying instructions from DNA for controlling the synthesis of proteins, although in some viruses RNA rather than DNA carries the genetic information.

Southern Blot: A procedure for identifying specific sequences of DNA, in which fragments separated on a gel are transferred directly to a second medium on which assay by hybridization may be carried out.

Spectrophotometer: An apparatus for measuring the intensity of light in a part of the spectrum, especially as transmitted or emitted by particular substances.

Sterilize: Make (something) free from bacteria or other living microorganisms

Therapeutic: Designed to help treat an illness.

Trypsin: A digestive enzyme which breaks down proteins in the small intestine, secreted by the pancreas as trypsinogen.

Virus: An infectve agent that typically consists of a nucleic acid molecule in a protein coat, is too small to be seen by light microscopy, and is able to multiply only within the living cells of a host.

Western Blot: An adaptation of the Southern blot procedure, used to identify specific amino-acid sequences in proteins.

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