Proposals for Designing and Controlling a Doctoral Research Project in Management Sciences

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Abstract: Analysis of available literature on the design and control of a doctoral research project has led to the updating of a certain number of "constants" concerning the difficulties PhD students encounter during this process. These "constants" revolve around the crucial yet arduous issue of what comprises a "good thesis". The present article offers a response to this question. It begins by highlighting the characteristics of the institutional context in which a doctoral study is carried out before offering three topics for thought (definition of the research project, deployment and evaluation of results). It concludes by focusing on the tension underlying all scientific research between "dissidence" (i.e., the ability to question "normal science") and "conformity" (the study's fit in an institutional context).

Keywords: Designing and controlling a research project, doctoral research in strategic management.

1. Introduction

Conducting and piloting doctoral research in Management, and maybe especially in the field of management (due to the porosity of this concept and its fundamentally paradoxical nature, de Woot, 1995 - Lauriol, 1999 – Dery, 2001 – Greenwood et al., 2004) is tantamount to "managing" the dynamics of an intellectual journey that is at best heuristic, and which more often than not involves an exploratory and even chaotic type of learning (Van de Ven, 1999).

This journey, which targets the fulfilment of a knowledge project concerned with an object of research, fits into a particular context, one defined by the rules, procedures, principles, paradigms and conventions that codify the conditions under which scientific knowledge is being produced in a given discipline (with knowledge being construed here as an "Institution", Cheng et al, 1996). It is no easy thing to fit into this context, since the difficulties punctuating the insertion process are subject to debates and controversies that can at times become somewhat acerbic. Moreover, they feature a "situated" aspect, meaning that they are tied to the places where the research is actually being conducted (the Supervisory Laboratory, the Research Supervisor, the jury members, real activities out in the field, etc.).

Such debates can be organised into questions and answers that the present article will attempt to analyse. Our ambition is to establish a (necessarily partial) synthesis of these lines of questioning by situating them, first of all, in a four-dimensional space, one we deem capable of characterising the institutional context of research in Management today (Section 1). Section 2 will deal with those issues we consider to be the most important, and will try to offer a few elements of response. Three main themes will be developed: that of the definition of the research project in relation with established knowledge, that of the implementation of the project itself and of the mastery of apparently "chaotic" dynamics thrown up by this implementation. Finally, there is the delicate matter of the evaluation of a completed Doctoral project. All of this should enable us to conclude with a personal assessment of the difficulties awaiting those who try to design and pilot a doctoral research project. These include the existence of a sort of "dilemma" resulting from the tension between "dissidence" (i.e., an attitude of critical distanciation required for an assessment that will be "correct" in "common sense" and "preconception" terms (Bachelard 1983), on one hand, and "conformity", on the other (given that research is part of an institutional context that cannot be ignored). What this implies is an ability to fit in (or comply) with the rules or conventions characterising the context, ones that more or less stringently define the criteria for developing and assessing a "good thesis".

2. An institutional context for doctoral research in management

This institutional context can be characterised by an architecture comprised of four poles (cf. figure 1). The first involves research question itself and the object that the study purports to investigate. The issue here is knowing what one is looking for, i.e., defining a research question that will allow the researcher to build and develop a knowledge project relating to the object under study. The formulation of this question is closely linked to disciplinary field of reference, and obviously to the researcher's mastery of the knowledge characterising said field. This first pole is tightly

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embedded into a whole set of elements pertaining to the epistemological and ontological stances that the researcher has taken (i.e., his/her intention). At an epistemological level, all research projects must fit into the major paradigms (naturalism, positivism, interpretativism, constructivism and postmodernism) upon which modern scientific research approaches are based (Lincoln and al. 2000, Wacheux, 1996). Each of these paradigms states an ontological hypothesis relating to the nature of the reality that it would like to study, and regarding the conditions in which the outcome knowledge is produced and validated.

In addition to a simple “conformity check” approach featuring these established paradigms, the “choice” of a positioning drives the researcher’s own ontology (Campbell, 1988). Researchers make their determinations in light of their own beliefs, representations and existential positions, all of which allow them to formulate a research intention, as well as the finality of their study (see the second pole). By so doing, they can offer a “justification context” (Boltanski, Chiapello, 1999) to legitimise their study within the disciplinary field to which it belongs.

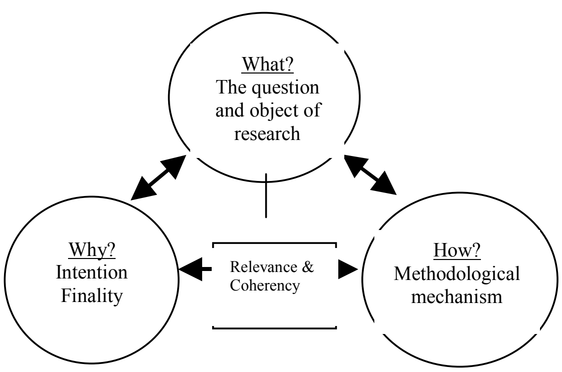

Figure 1: The institutional architecture of a research project in the field of management

The third pole combines all of the questions relating to “how?” What this involves is thinking about the methodological research mechanisms that are going to determine the validity and reliability of the knowledge being generated by the development of a research project. The choices made here, in terms of data production, compilation, processing and analysis methods will express or reflect the epistemological stance that the researcher has more or less explicitly assumed. They help to deploy paths that can be used to explore the object (be it theoretical, empirical or a construct) and enable the modes of (deductive, inductive and/or abductive) reasoning that will be mobilised to achieve the study’s objectives (KOENIG 1993).

Piloting a research project is therefore, at the very least, tantamount to producing an “optimal” equilibrium between these three poles. This dynamic equilibrium, which is probably relatively close to the punctuated equilibrium that M. Tushman and E. Romanelli have proposed (1994) determines to a large extent the fourth pole of this architecture, which concerns the relevancy of the project and the coherency of its outcomes.

The relevancy of a project can be assessed in two main dimensions:

- Its legitimacy in terms of the paradigms, schools of thought or research programmes that characterise a given disciplinary field.
- Its legitimacy in terms of a “social demand”. This results from an equilibrium between expectations that are academic in nature, as they are expressed in a disciplinary field at a given moment in time, and expectations of a world that can be qualified as “professional”, being the world of actors engaged in real activities (the world of practices and practitioners being affected by this research project). These expectations are not necessarily convergent and can pursue logics that are complementary yet also concurrent and even antagonistic.

Coherency can be analysed in terms of the research project’s architecture (or design), which in turn conditions the validity and the reliability of its outcomes. The articulation between a research approach and literary analysis, the validity of the proposed conceptual construct, the methods applied when gathering and processing the data used plus the analysis and interpretation of the research outcomes are all part of this coherency.

The study’s assessment necessarily fits within a framework that is defined by a host of controversies concerning the conditions under which scientific knowledge is being produced. These controversies express strong oppositions between positions that are more or less contrasting (positivism versus constructivism, contents versus process, etc.). In this way, they ask questions about two essential dimensions of the knowledge production process: the causality established between the observations made and the analysis offered thereof; and its objectivity, which can be based on recognised protocols (explanation of facts depending on criteria of verifiability, confirmability and refutability). To design, organise and pilot a doctoral research project, it therefore seems crucial that the proponent seeks (and finds) a “satisfactory” positioning within this context and the different constraints that characterise it. It is in the search for this point of equilibrium that the researcher faces the delicate problem of determining a position along a sort of continuum that is comprised of the two aforementioned extremes: dissidence on one hand; and the ability to manage "pressure towards institutional conformity" on the other. To cope with this problem, we suggest three dimensions that appear to be crucial for the design and conduct of a research project.

3. Three questions and a few answers regarding the definition and assessment of a 'good thesis'

These three questions relate to three key steps in the design and completion of a research project:

- Definition of the project itself, based on a researcher's "footing" in the paradigms or schools of thought that specify established knowledge in a discipline.
- Project Deployment, i.e., the researcher's ability to manage a dynamic process necessitating the establishment of specific support modalities and mechanisms.
- Evaluation of the project once completed, meaning the delicate question of assessing whatever work has been done.

3.1 How to define a project in relation with established knowledge

This question, which relates to the researcher's positioning in terms of a certain number of paradigms considered to be more or less established (or, to the contrary, emergent), can cause many problems. First of all, which criteria help us to define a paradigm? When can we consider that a paradigm is established and has become part of a "normal science"? Inversely, when must it be treated for the "anomalies" that characterise a crisis period, one that can be the precursor of a "revolution" and may bear an emergent paradigm (Kuhn, 1983)? Moreover, many Management Science disciplines are characterised by their great openness to a number of ancillary disciplines (economics and its many branches, psychology, sociology, etc.). This situation creates an additional problem, which is the project's footing in a mono-disciplinary approach, or inversely in an inter- or multi-disciplinary one.

Of course, it is not easy to answer these questions. First and foremost, they depend on the researcher, and on the options s/he selects when defining and completing the study. They may also be a sort of "tactic" involving an adaptation of the disciplinary context to the project (or the opposite, with an adaptation of the project to a disciplinary context). Lastly, they are often initially codified within a given research structure (a laboratory or research centre), meaning that the researcher fits into an already established theoretical and methodological corpus. As an individual, the researcher must be "clear" about his/her ontological orientations. That is to say, s/he must be able to formulate the beliefs, representations or postulates underlying these orientations. This clarification is indispensable since it makes it possible to understand and justify the epistemological anchoring chosen to design and develop his/her study.

In other words, these are beliefs, and it is by clarifying them that one's epistemological positioning can be justified and argued. For example, taking a constructivist position pre-supposes that the three postulates proposed by A. Giddens (1987) be "internalised", or at least considered by the researcher. This is due to the need to be able to discuss the role that is delegated to a given agent or actor ("the return of the subject… and his/her subjectivity" Giddens .14) during the course of a collective action. It also means accepting and "recognising the fundamental character of agents' practical conscience … their competency… their reflective capacities…" (ibid. 33.41) whilst underlining "the contextuality of the social interaction… the diversity of the action contexts" (ibid. 35.74) as well as the social systems' production and reproduction function, which is driven by this interaction. This "self-aware" choice defines an actor (a manager, for example) as something more than a mere processor of information, an agent simply reacting to changing conditions. More than anything else, s/he is "an active participant in the construction of his own environment" (MIR et al., 2000.945).

This preamble to the formalisation of a research project often results from an interactive approach between the researcher's lines of questioning: with knowledge established through literature, and with the paradigms (disciplinary ones, this time) that characterise a discipline. Here we find ourselves in a dual situation characterised by two poles: interdisciplinarity; and multidisciplinarity. This type of dilemma is frequent in the field of Management Sciences. Just consider the diversity of the theoretical approaches that become possible in scientific experiments when one focuses on questions like organisational revitalisation, change or transformation. The same applies to managerial and organisational cognition, and to Resource-Based strategic theories (Koenig, 1999). This diversity can induce the researcher to opt for a particular approach (a Resource-Based View instead of one formulated in terms of Dynamic Capacities), to seek complementarities, or to compare each of these approaches so as to test their explanatory, predictive or operative potential.

This insertion in a approach that has been codified by a support structure mechanism which benefits from accumulated experience partially resolves the issue of paradigmatic choice. But only partially, since it does not dispel the problem of the researcher's ability to work within the framework of these mechanisms and theoretical and methodological orientations, nor his/her ability to accept the "consequences" (i.e., participating advantageously in the controversies that derive from this positioning, supporting positions that can be relatively tenuous in terms of the "normal science" of a given era, etc.) In general, what is important with regards to this question of paradigmatic positioning is to avoid "caricatural antinomies" between different perspectives (Tsoukas 2000). These lead to the formulation of overly reductionist alternatives when the point is to seek complementarities and factors of coherency that make it possible to design and deploy a knowledge project that can be relevant (David, 2000).

Ideally, this positioning should help to ensure the development of a process that is necessarily iterative, non-linear and exploratory. Such a process targets stability, to be developed throughout this phase through the building of an internal coherency between the research study's design, the issues it raises, the analytical framework that has been chosen and the outcomes. This brings us to the second approach under consideration, which is the management over time of a research project's deployment dynamic.

3.2 Deploying a research project

This second dimension mainly concerns the problem of the study's potential "feasibility". It refers to the competencies mobilised by the researcher but also to the nature of the project itself (is it "actionable"?), its target audiences and the various support mechanisms at the researcher's disposal when s/he is deploying the project. A research project's feasibility dimension first raises questions about the competencies that the researcher is able to mobilise when developing the study's various phases. This refers to his/her ability to acquire and to leverage assets (i.e., the knowledge that specifies a given disciplinary field), and to "coordinate them durably … so as to achieve his/her objectives" (Sanchez 2000.66)

It also refers to the theoretical understanding, which the researcher has of the disciplinary field in which s/he is operating. What are the main controversies, which (sometimes "implicitly normative") postulates are they based upon, which theoretical or practical issues are at stake, to what extent is this of interest to today's scientific community, and what is the "life expectancy" or strategic horizon of these controversies? (Weick 1999). These assets should enable the development of a cognitive maturation process revolving around the "three phases that structure a research process: the intuitive; the comprehensive; and the demonstrative" (Wacheux, 1996.2). Intuition results from a process of perceiving observed facts. Through analysis and reasoning (based on an epistemological anchoring), comprehension enables the formation of an initial yet "comprehensive problemation" of the research question being studied. This "problemation" should be questioned by testing facts relating to the initial representations that the researcher has developed, leading in the end to "reasoned intuitions" (ibid.7) that are the very basis of the "progressive construction of an ultimate explanatory proposal" (p.8).

When approaching a particular field of study, such analytical capacities should be linked to relational ones. This notion is strongly correlated with the project's epistemological positioning. A "positivist" orientation necessitates a great deal of neutrality and a position of maximal externalisation. A constructivist orientation, based on the "acceptance of a universe built with actors' representations" (de Bruyne 1974.13), mobilises observational qualities and aptitudes, as well as the ability to listen and to ask questions – all of which will allow the researcher to wield a certain control over any cognitive biases by which s/he may be motivated. These elements also raise questions of access to the field of real activity. This dimension is linked to the nature of the object and to the question of research. Can the relevant facts be modelled at what we can consider to be a reasonable level of complexity (multiplicity and diversity of variables)? Can they be manipulated out in the real world, and are there situations where this can be envisaged? Are they acceptable for a group of actors or for a target organisation, and what interests might they have in this process?

Aspects of this conjunction of interests between researchers and given fields of real activity introduce the notion of "social demand" into research. This concept can be expressed at two levels: the suitability of academic types of demands, based on an increase in knowledge in a given disciplinary field; and the expectations of the "pragmatic" world (Dery 1989), to wit, of a "client" (the host organisation, which generally demands the resolution of concrete and practical problems). These expectations are not necessarily convergent or synchronous, and therein lies the rub. The problem's treatment clearly depends on the nature of the study in question, and on its finalities (i.e., are we dealing with a realistic finality that seeks to describe the world such as it is, or else an "instrumentalist" approach that tries to develop predictively-oriented theoretical mechanisms to enhance our understanding of reality).

In terms of this "social demand", one should remember the distinction proposed by M. Latour (1987) and B. Callon (1989) between science and research. Research involves a science "that is in the process of being built" and which is based on a "scientific practice", whereas science is "built" out of facts that have been established and validated through epistemological protocols. As a scientific practice, research cannot be conceived of outside of the context in which it develops (a mechanism or a network of relations). As a result, a "scientific" research project can only exist when it can be of "interest" to strategic actors involved in its network. In other words, "social demand" mainly results from the researcher's ability to build a mechanism that mobilises these different actors. This brings us to the question of the researcher's competencies - but also to the support mechanism where such competencies can be built and deployed. Indeed, it is within these systems (i.e.: Supervisory Laboratory) that a project takes shape and develops. These are also the places where we can build a "general agreement of minds" concerning the responses to be provided for the various points and issues raised previously.

This context can (sometimes dramatically) constrain the knowledge production process that brings the researcher to a certain intelligence of the object with which s/he is concerned. As such, and whenever possible, a study's context must be chosen with care. This choice is a difficult one, due to the diversity of existing framework modalities and mechanisms. Such diversity reflects the varied nature of Research Supervisors, whose epistemological and theoretical stances towards one and the same problem can be quite diverse. This situation can be seen as something beneficial, due to the potential richness it creates in terms of the approaches' (and therefore the projects') diversity. But it is also risky, since it expresses the state of controversies characterising a disciplinary field at a given moment in time. To cope with these problems, the researcher must develop an ability to assess the general context so as to identify its "local rationality". This assessment begins with the choice of a Research Supervisor, a decision that depends on a number of criteria (Xuereb 1999): his/her intellectual proximity to the project in question, academic reputation, availability, working methods and experience in supervising a thesis - not to forget the "relational proximity" as perceived by the PhD candidate. This dimension means that trust (a system of mutual expectations that is more or less calculated in nature) becomes the main mode for governing such a relationship.

Although the Research Supervisor constitutes the key element in this support mechanism, we should not neglect the resources that any particular structure has at its disposal (material or academic resources, membership in networks, etc.). This "stock" of resources constitutes the foundation of an "economy of accumulation" (resources that are complementary and additive, Cool 2000) and they cannot help but be beneficial to the Ph.D. research project's development dynamics. Evaluations of a project's feasibility are therefore situated in a three-dimensional space: the competencies that the researcher can mobilise; the subject's relevancy; and its ability to produce results that can be activated at an academic and pragmatic level. There is also the question of access to a mechanism that will provide sufficient support for a research project's deployment dynamics. It is within this space that a project can be designed and shaped in a way that leads to its being seen as a "good thesis".

3.3 A few criteria for assessing a "good thesis": the question of evaluation

Issues like what constitutes a "good thesis" or appropriate assessment criteria express the difficulties that young Ph.D. candidates face in developing a representation of what is expected of doctoral research. The problem is the same for Research Supervisors who have to codify requirements to frame an approach that fundamentally remains a process of (exploratory and heuristic) discovery. Thus, the response that is generally provided to the question "what constitutes a good thesis?" is that "this depends" - mainly on the subject and on the researcher. A "good thesis" is to a large extent a subject that is considered to be "interesting" or relevant in terms of the questions and controversies characterising its disciplinary field, the interests of the Research Supervisor and the "social demand" (the "pragmatic world" or societal subjects). A "good thesis" is obviously one that has been completed, i.e. one where a successful viva voce has been given. This is a dimension that could appear trivial but it is seemingly not very easy to "complete" a thesis. After all, this is a process whose ambition

is to enrich the world of knowledge, and bringing a study to conclusion despite all the difficulties encountered requires confirmed competencies, time and a degree of tenacity.
What this means is that a doctoral student has to learn to separate him/herself from any ambition that can be described as “excessive”, at least temporarily, by accepting the intermediate stage that is the viva voce. Doctoral research is training in research through research. Like all learning processes, one needs time to get close to the truth (“truth is the daughter of time” Pettigrew 1990. 271). Doctoral research constitutes the first step of this process, one whose objective consists of enabling learning, and of evaluating outcomes. This means having the ability to prepare oneself for the aforementioned separation, progressively detaching oneself from any overly precocious ambitions in order to avoid falling prey to “the melancholy” of a “lost object” (Freud, 1915).

A “good thesis” is also a research study that supports a thesis, insofar as it takes a position. When the project is underpinned by a subject and by an approach, the researcher has taken a position towards the debates and controversies relating to this approach. This implies a clear presentation of such tensions and means that it should be possible to justify the significance of the research questions being associated with them. “Taking a position” also means offering working hypotheses and a plan capable of covering the question under study. It also means the ability to offer precise responses, even if they are incomplete or full of ambiguities. This dimension raises the issue of the outcomes achieved within the study’s framework. Above and beyond purely methodological questions, outcomes mainly reflect epistemological positioning’s but they also involve presentation issues. A “good thesis” uses a high quality argumentative process to highlight the study and its outcomes. Here one considers the clarity of the overview and the rigorousness manifested in its manipulation of concepts - as well as the style of the narration, which should convince the reader of the authenticity, plausibility and critical nature of the work being done. Lastly, there is the ability to produce a text that is “limpid” at a semantic level, facilitating access to complex contents and allowing for an analysis that is “economic” at a cognitive level (Czarniawska, 1999).

In short, all of these argumentative processes must try to highlight the value of the thesis and its contributions, “defending” and affirming a “point of view” relating to a given problem whilst underlining the difficulties encountered; the study’s limitations, and the avenues of development it envisages. The conception and deployment of a research project thus appears as a singular approach with a permanent tension between “dissidence” and “conformity”.

4. A few elements of conclusion

Designing and conducting a doctoral research project: between Dissidence and Conformity? Designing and piloting a doctoral research project consists of managing a knowledge production process that revolves around three main sequences:

1. The building of whatever “Positioning” is needed to situate the project within a world of controversies, comprised of theoretical and epistemological stances that can be more or less in competition with one another. The idea here is to lay foundations or to outline a perspective based on an analysis of such controversies’ “social contents”, thereby allowing the researcher to assimilate this context, and to form an initial line of questioning.
2. Management of the research project deployment process. This second sequence is generally tied to the interaction between the researcher and the field of real activity. It often leads to new lines of questioning that can consolidate things or make them more fragile (during the study’s starting phase). The goal is to manage the evolutionary dynamic of a process that can be based on an incremental logic or on “punctuated equilibrium” (periods of continuity, incremental or fluctuating changes, sometimes leading to radical transformations (Romanelli et al. 1994).

The question here is what mechanism is in place to manage and regulate such tensions. The support system should be able to accommodate (i.e., by incorporating new schemas of thought and action into pre-existing schemas. Piaget. 1992) any new data considered to be problematic. It should also help to restore a renewed equilibrium, one that will be based on an “enrichment” of the researcher’s cognitive structure.

3. The “discursive structuring” of the project and its contributions to scientific knowledge. The point here is to render intelligible complex phenomena that are of interest to actors characterised by varying expectations. This involves a translation formulated by the key actor in this process (the researcher) who, after analysing the context and the legitimisation of his/her framework of analysis, offers a problematisation for the observed facts and “states” “intelligible links between heterogeneous activities (or facts)” (Callon. 1992.65).

In short, designing and piloting a research project is tantamount to one's ability to manage the dynamic process of building an equilibrium between two poles: "dissidence" on one hand, and "conformity" on the other. By equilibrium, we mean "a situation where individuals do the best they can for themselves given … the institutional framework that defines the options they are being offered, and which interconnect their actions" (Kreps. 1990.6). "Dissidence" probably constitutes the main factor activating this tension, for as long as the project runs. This is an attitude or an intellectual stance that tries to ask questions about "normal science", whilst uncovering the sometimes implicitly normative theoretical postulates being conveyed by certain paradigms or schools of thought. It protects people from the "fads" that can characterise a theoretical context at a given moment in time and helps them to resist any "hegemonies", as suggested by B. Latour (1995), who considers that there is a need to "protect researchers and science itself from hegemony … fads or domination" (81) but specifies "the need to be simultaneously independent and to know how to resist hegemonies", as well as the necessity of having "colleagues you can depend on" (ibid).

This stance implies the mobilisation of intellectual competencies that are based on an excellent knowledge of the disciplinary field concerned by the project. It also implies distanciation and the creative criticism capabilities that will make it possible to forge a project that is relevant and whose legitimacy can be established. The project legitimacy dimension can only be conceived of outside of an "agreement of minds", as expressed by an academic community (with research being construed here as an institution). A researcher cannot "be the only one to get it right" and preserve critical capabilities whilst affirming his/her thesis. What s/he can do is to act upon any social influence, thus guaranteeing the scientific nature of his/her approach and suggested conclusions. This harks back to the aforementioned dimensions (coherency and relevancy of the project, coherency of the "discursive structuring", quality of the argumentative processes and validity or plausibility of the outcomes, etc.). To these qualities, we can probably add the researcher's manifest ability to wield "a minority influence".

This ability, largely analysed in social psychology (Levine et al 1990), results from a style of behaviour that a reference group perceives as being consistent (i.e., constantly affirming a position, without contradictory statements, and based on the development of a system of proven logic, in reference to a logical framework of analysis). This ongoing consistency, when perceived by the group, means that the individual will be awarded a certain right to exert a social influence on the milieu in which s/he is acting, thus legitimising his/her study. It is probably in this capacity for minority influence that we find the key to building a dynamic equilibrium between "dissidence and conformity". It is a process that helps to "renovate" the study's context (all else remaining equal) by preserving the possible development of a (more or less dissident) "thesis", and by engaging a testing process that is in tune with established canons of epistemology and methodology (pressure towards conformity).

In lieu of a conclusion, we can probably consider that designing and piloting a doctoral research project involves a truly strategic approach (devising a position based on resources and competencies, as well as action plans that are meant to act upon an environment instead of accepting its constraints). In other words, this project of discovery involves a modicum of pleasure (like any intellectual journey), spiced by a host of difficulties (and pains). It is probably by situating oneself within such an orientation that the researcher can optimise his/her management of the separation dynamics to which, as aforementioned, s/he is subject.

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