# A Field of Veiled Continuities
## Studies in the Methodology and Theory of Educational Research

## Corrado Matta

## Abstract

Empirical educational research enjoys a methodological and theoretical debate that is characterized by a number of unresolved and lively debated controversies. This compilation thesis is an attempt to contribute to this debate using the toolbox of philosophy of science.

The thesis consists of an introductory chapter and four essays. In the introductory chapter I identify three methodological and theoretical controversies that are discussed within the field of educational research. These are: 1) the controversy concerning the scientific status of educational research; 2) the controversy between cognitive and sociocultural theories of learning; and, 3) the controversy between realist and constructionist interpretations of theories of learning.

I provide in the essays a critical assessment of the claims behind each of these controversies, and argue for an alternative reconstruction of these issues.

In *Essay I*, I criticize a view about the interpretation of human action, labeled in the text as interpretivism. This view posits a sharp separation between the natural and social sciences, to the effect that the methods of the latter cannot be applied to the former. The first controversy seems to rest on this position. As I argue, the arguments in support of interpretivism are contradicted by actual research practice. I conclude that the interpretivistic claims lack support and that the general separation claim appears as problematic.

A further debate has fueled the first controversy, that is, the supposed distinction between qualitative and quantitative methods. In *Essay II*, I argue against this distinction. More specifically, I discuss the concept of empirical support in the context of qualitative methods (for short, qualitative support). I provide arguments that although there are two specific and non-trivial properties of qualitative support, there is no methodological separation between quantitative and qualitative methods concerning empirical support.

Considered together, the first two essays indicate two points of methodological continuity between educational research and other scientific practices (such as the natural sciences). I therefore conclude that the controversy concerning the scientific status of educational research rests in large part on unjustified claims.

*Essay III* focuses on the second controversy. In this article I argue that Suárez' inferential approach to the concept of scientific representation can be used as an account of scientific representation in learning, regardless of whether learning is understood as a cognitive or social phenomenon.

The third controversy is discussed in *Essay IV*. Here, I discuss some ontological aspects of the framework of the actor-network theory. Reflecting on the use of this framework in the research field of Networked Learning, I argue that the assumption of an ontology of relations provides the solution for two puzzles about the ontology of networks. The relevance of my argument for the third controversy is that it suggests a point of connection between constructionist and realist interpretations of the ontology of learning.

The last two essays suggest two points of continuities between theoretical frameworks that have been and still are argued to be incompatible.

**Keywords:** *Methodology of Educational Research, Educational Theory, Educational Philosohy, Philosophy of the Social Sciences, Causal Explanations of Actions, Qualtative Methods, Scientific Representation, Learning, Actor Network Theory, Ontic Structural Realism.*

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*Mi soledad se alegra con esa elegante esperanza.*

Jorge Luis Borges:
"La biblioteca de Babel"

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# List of Essays

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Matta, Corrado 2015. "Interpretivism and Causal Explanations A Case from Educational Research." *Philosophy of the Social Sciences* 45 (6): 543–67.

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**Essay IV (Conference Proceedings - Published)**
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# Introduction

> Probably much more than for other branches of applied research, empirical educational research is a hotly contested, sometimes reviled, ideologically saturated field that possesses enormous and yet largely untapped potential for contributing to the improvement of society (Phillips 2009, 382)

Empirical educational research enjoys a methodological and theoretical debate that is characterized by a number of unresolved and lively discussed controversies. This doctoral thesis is an attempt to contribute to this debate using the toolbox of philosophy of science.

In the quotation that opens this introduction, the eminent educational philosopher Denis C. Phillips depicts a field that is "hotly contested". The contentions he describes that motivate this dissertation are both internal and external in the field of empirical educational research. The internal disputes concern the ways in which empirical educational researchers conceive the status of their field. Some contend that empirical educational research is methodologically distinct from other empirical endeavors, while others reject this distinction. Some claim that the different ways in which the objects of educational research are theoretically constructed cannot be regimented under a broader meta-theoretical framework, while others reject this internal separation.

Parallel with these internal disputes, empirical educational research has been under attack from external forces. One example of these external attacks has been carefully discussed by Lather, who denounces the occurrence of "governmental incursion into legislating scientific method in the realm of educational research" (2004, 759) in the US, the UK and Australia. One major example of such incursions is the No Child Left Behind Act, which, according to Feuer, Towne and Shavelson, "requires federal grantees to use their funds on evidence-based strategies" (2002, 22). The problematic aspect of this policy is that it suggests how scientifically based research should be carried out. Among the other requirements, scientific research is

> evaluated using experimental or quasi-experimental designs in which individuals, entities, programs, or activities are assigned to different conditions and with appropriate controls to evaluate the effects of the condition of interest, with a preference for random-assignment experiments, or other designs to the extent that those designs contain within-condition or across-condition controls (No Child Left Behind Act 2002)

The consequence of this legislation is a concrete restriction of public funding in educational research for proposals that plan to use certain methods (with a preference for randomized control trials (RCT)), with the effect of potentially excluding the largest part of empirical educational research methods from access to public economic resources.

This dissertation aims at using the resources of philosophy of science to try to resolve these controversies by criticizing the claims that lie behind both the internal and external disputes and by providing an alternative reconstruction of these issues that proves that the controversies are only apparent. In the case of the external attacks, my contribution will consist in providing philosophical arguments against the claim that only the use of certain research methods can grant education the status of scientific research (such as quantitative effect size studies or randomized control trials). In the case of the internal disputes, I will argue that the analysis of concrete methods indicates that there are certain crucial points of continuity where many people have argued we should find sharp theoretical distinctions.

Hence, in this dissertation, I provide philosophical arguments for internal and external continuity in the methodology and theory of educational research. In the essays I argue that these continuities are somehow "already there" but are hidden under the veil of political discourses and disciplinary traditions. The tools of philosophy of science are used to break the spell of these discourses and traditions and unveil the methodological and theoretical continuities.

The main reason for using philosophy of science as an instrument for the resolution of these controversies is that, as I argue, they rest on conceptual disagreements that can be resolved by applying a thorough analysis of the terms that are involved in them. Such conceptual analysis is the main methodological tool of philosophy of science.

The philosophical arguments provided in the essays are not the kind of general, broad and abstract arguments that are sometimes discussed in certain philosophical literature. The kind of philosophical analysis proposed here focuses instead on how methods actually work in the empirical researcher's practice. The essays are case studies of applications of methods and theories in empirical educational research. This specific methodology has the potentiality to fill a gap in educational literature. In the same chapter as the opening quotation, Phillips criticizes the current state of philosophical analyses of empirical educational research. Specifically, he criticizes the lack of detailed analyses of concrete methodologies and the philosophical approaches of the few existing studies. As he states, in the philosophical discussion of educational research "the detailed examination of empirical educational research is given short shrift—if any shrift at all" (2009, 390). Concerning the existing philosophical discussion of empirical educational research, Phillips laments that these studies often assume that empirical researchers can learn from philosophical discussion and never the opposite.

As he puts it: "Rather than initiating a dialogue in which philosophers and researchers both might learn something, the language implies that only the researchers stand to learn anything" (2009, 390).

Assuming that Phillips's strong criticism is well motivated, the essays in this dissertation help to fill precisely the gap in the literature about methods in education that Phillips discusses. This is done by providing detailed philosophical studies of concrete methodological practices in education. Furthermore, the thrust of all four essays is to analyze concrete cases of methods in order to learn about philosophical concepts such as explanation, interpretation and evidence. Hence, in this thesis philosophy tries to learn about educational science by looking at concrete scientific practice.

This thesis consists of an introductory chapter and four essays. The essays constitute the core of the thesis, and it is there that I draw my main conclusions. The introductory chapter has the main function of providing a background for the essays.

This introductory chapter contains, in Section 1, a discussion of the aims of the thesis. Section 2 presents a number of controversies concerning the methodological and theoretical discussion in educational research, the controversies that are the subject matter of the four essays. Section 3 spells out the philosophical claims that lie behind these controversies. Section 4 discusses the methodology used in the essays. Section 5 summarizes the essays. In Section 6 I wrap up the conclusions of the essays and discuss possible future developments of the discussions in the essays.

## 1.1   Aims of the Thesis

The main aim of the thesis is to provide a critical assessment of the claims behind four methodological and theoretical controversies in educational research and to argue for an alternative reconstruction of these issues. More specifically, this thesis aims at providing:

a. A critique of *interpretivism* and a defense of methodological naturalism in educational research, concerning the issues of *causal explanations* (Essay I)

b. A critique of *separatism* and a defense of methodological naturalism in educational research, concerning the issues of *empirical support* (Essay II)

c. A discussion of the issue of the theoretical incompatibility between cognitive and sociocultural theories of learning, based on the concept of *scientific representation* (Essay III)

d. A proposal towards a *partial realist interpretation* of Actor-Network Theory in educational research (Essay IV)

Let us spell out these aims in some further detail and define the terminology used in them. The main aim states that there are a number of methodological and theoretical issues that are relevant for and debated among educational researchers. These controversies rest on methodological and theoretical claims. I criticize these claims and propose an alternative reconstruction of the issues that rest on these claims. The result of this reconstruction is that the controversies reveal themselves as being only apparent (Essays I and II); and an alternative perspective that might potentially lead to a solution of the controversy is proposed (Essays III and IV).

Let us now look at how this argumentative scheme is concretized in the four sub-aims. The first two sub-aims (*a* and *b*) concern the controversy between methodological unification and separation. In simple terms, some researchers have argued that the methods of education are, in some respects, methodologically distinct from those of other research practices, and in particular with those of the natural or quantitative sciences. This claim of distinction is referred to as *methodological separatism* throughout the thesis. The opposite claim, which is called in this thesis *methodological naturalism*, contends that there is no methodological separation between educational research and the natural or quantitative sciences. The naturalist claim is considered in two respects: Essay I looks at an example of methodological separatism about causal explanations, that is, the claim that the empirical analysis of educational phenomena cannot be aimed at producing causal explanations. This is a separatist claim, since it assumes that other research practices aim by definition at causally explaining their particular phenomena.

Essay II considers the issue of methodological separatism about empirical support. Here I discuss the separatist claim according to which qualitative methods cannot be used to support or contradict hypotheses in the same fashion as quantitative methods.

Essays I and II both criticize methodological separatism in educational research and defend *methodological naturalism,* in that they both indicate methodological continuities between different research practices.

The third sub-aim (*c*) concerns the issue of representation in learning. By *representations* I mean here the concrete artifacts that in the context of educational practices are assumed to symbolize or indicate learning. Intuitively, the process of learning science involves a representational component since, in the context of educational practice, we do not have access to the actual learning but only to learners' artifacts. Science learners use concrete representations as *a means to* and as *signs of learning* (Selander 2008). Now, different theoretical approaches to science learning entail different theoretical assumptions about representation. In Essay III, I use the philosophical discussion about the concept of scientific representation and discuss the role of representation in learning in terms of some philosophical theories of scientific representation. The benefit of using this discussion as a tool for dissecting the concept of representation in learning is that, compared with the current literature, it presents a clearer explication of the elements that are involved in the phenomenon of representation in learning and

where the differences between different approaches to learning really are. By doing so, I identify the origin of the incompatibility between two main approaches to the concept of learning.

The final sub-aim (*d*) concerns the issue of the ontology of learning. Just as with the other issues, this is also a controversial question for educational researchers. Several contrasting conceptions of the ontology of learning have characterized the historical development of educational research, resulting in a multiplicity of different standpoints. Essay IV considers a recent methodological approach in educational research known as Actor-Network Theory (ANT). This approach is informed by a number of strong assumptions concerning the nature of social reality, and, consequently, about learning. This ontological standpoint sees learning as a socio-material phenomenon, that is, the result of a network of relations that are both social and material. Actor-Network Theory has been argued to rest on a strong form of anti-realism, that is, the claim that scientific theories do not describe the world as it is. However, I argue that ANT is consistent with a form of scientific realism (the thesis that the world is at least partially as scientific theories describe it) called Ontic Structural Realism (Ladyman and Ross 2007).


## 1.2   How did I end up exactly here?

The reader might wonder why, of all the possible themes and issues that are related to the general discussion about empirical educational science, I chose precisely the four themes discussed in the essays.

This question is well motivated, since some of the issues I discuss are not directly related to one another. The issues discussed in Essays I and II are indeed related, since the former introduces the standpoint of interpretivism, which is qualified in the paper in terms of limitations to the overall testability of interpretations of action. Essay I criticizes this form of interpretivism by pointing out how educational researchers use data to evaluate interpretations.

Then, in Essay II, I dig deeper into the issue of testability, focusing on the more basic concept of empirical support. Therefore, the first two essays follow the same thematic track: the issue of the methodology of educational research in relationship to the tension between methodological naturalism and interpretivism.

In contrast, Essays III and IV are neither clearly connected with the first two essays nor with one another. Whereas the first two essays provide a clear argument in favor of one standpoint and against another, the latter two essays are more speculative: they suggest the origin of possible problems, identify relations and suggest analogies. Moreover, whereas the first two essays focus on the methodology of educational research, Essays III and IV focus on more abstract matters such as the ontology and the role of representation in learning.

If I had to give a short answer to this variety of themes, I would simply say: I ended up here!

More specifically, this collection of essays is the result of the paths I took and left during my doctoral studies. When I began, I had a strong interest in metaphysical issues and especially in the ontology of empirical social theories. The issue of scientific realism has been one of my main interests since I was a student and I found the later developments of structural realism and its relation to the social sciences a promising and unexplored land. This motivated Essay IV, which is chronologically the first essay I wrote during my doctoral studies. Essay III was initially planned to propose a structuralist interpretation of learning. This interpretation was planned to discuss the ontological differences between opposed ways of empirically theorizing learning, that is, as a psychological or sociological phenomenon. Eventually, I encountered the problem of conceptualizing the relationship between learning (either as a psychological or sociological problem) and the artifacts that are used to evaluate or identify it. I searched the literature in education for an account of this relation without satisfaction. Representation is a term that is commonly used in educational research, but it lacks a thorough understanding. It is a commonplace that the concept of representation was criticized by post-modern continental philosophy. However, I was dissatisfied by that discussion. In my opinion, it rests on a basic conception of representation that is too difficult to accept, and the arguments that are presented against this conception are too vague. I therefore decided to attempt to find a solution to this problem before going any further. In the end, I arrived at the conclusion that the ontology of learning and the nature of representation are indeed related. Empirical theories of learning entail different concepts of representation. Hence, the ontological interpretation of empirical theories of learning will plausibly involve a representational component.

Parallel to my interest in ontological issues, I developed a strong interest in methodological questions. This interest had different sources. First of all, there was my experience in teaching social research methods. Secondly, as I started to go deeper into the problem of realism in social science, I came in contact with a number of methodological issues, especially those of interpretivism and naturalism. Finally, working in a department where many different methodological approaches are represented, I started to consider and discuss with my colleagues the points of conjunction and unity in a field with so many different methodological practices. This resulted in Essays I and II. In these essays I started to use another methodology; that is, I analyzed in detail some delimited cases of application of methods (this is discussed in Section 4), which resulted from a closer contact with practice compared with Essays III and IV.

As I approached the end of my doctoral studies, I began to wonder what could connect the four studies and how could I justify such theoretical diversity. In the end, I realized that the diversity of themes and issues might be an asset rather than a weakness of the dissertation. The diversity of methodological, ontological,

theoretical and ethical issues that are relevant for educational research is nicely represented by a thematically diversified selection of articles.

# 2 Controversies in Educational Research

The previous section briefly introduced the methodological and theoretical controversies that are discussed in the essays. In this section I locate these controversies within the ongoing methodological and theoretical discussion among educational researchers. The aim of this section is to provide indications that each of the issues discussed in the article has relevance for educational research.

## 2.1 Educational Research and Methodological Separatism

The *scientific* status of the methods of educational research is a controversial issue. As Feuer, Towne and Shavelson assert:

> We do have evidence to support the contention that educational research is perceived to be of low quality. […] Educational researchers themselves are often their own harshest critics […] They are often joined by a chorus of social and physical scientists, engineers, and business leaders who lament weak or absent theory, accumulations of anecdote masquerading as evidence, studies with little obvious policy relevance, seemingly endless disputes over the desired outcomes of schooling, low levels of replicability, large error margins, opaqueness of data and sources, unwillingness or inability to agree on a common set of metrics, and the inevitable intrusion of ideology at the ground level. (Feuer, Towne, and Shavelson 2002, 26)

This quotation comes from an article that summarizes a 2002 report by the US National Research Council entitled *Scientific Research in Education*. The main aim of this report is to describe the scientific basis of educational research and to call for a concept of science that encompasses the diversity of research practices that characterizes educational research.

Despite its inclusive thrust, many educational researchers criticized this report. For instance, Erickson and Gutierrez (2002) argue that the claim that educational research should be methodologically in line with the hard sciences is the result of a far too restrictive and old-fashioned concept of science as only aimed at control, prediction and explanation. The point made here is that such attempts at pushing educational research in the direction of certain practices in the natural sciences rest on the view that scientific methodology consists of one or just a few methods (possibly some variation of randomized control trials). Therefore, Erickson and Gutierrez do not argue against the call for scientific rigor in educational science

per se but against an idea of science that does not reflect the complexity of scientific enquiry, both inside and outside the field of education. Erickson and Gutierrez's view is not an example of separatism. They seem to criticize conceptions of science that are too narrow while affirming the scientific status of educational research.

In contrast, commenting on the same report, St. Pierre (2002) presents a different view. Her criticism of the scientific status of educational science is not based on the idea of expanding the general idea of science so as to include more practices but on the idea of a proliferation of *local conceptions of science*, all in possible conflict with one another. Whereas Erickson and Guitierrez argue for the unity of science, St. Pierre argues for separation. According to St. Pierre, education should rest on an alternative and competing set of scientific virtues than those suggested in the NCR report (which suits the natural sciences better). This claim is an example of separatism, since it rests on the idea that educational research is epistemologically separated from the natural sciences. The idea that different scientific practices rest on separate sets of epistemic virtues is commonly conveyed by the popular concept of *paradigm* (Guba and Lincoln 1994). A paradigm is the set of ontological, epistemological and methodological assumptions that characterizes a research community. Separatists who discuss educational science have often claimed that educational science and the hard sciences belong to different and irreconcilable paradigms.

While discussing the relationship between philosophy of education and empirical educational research, Phillips (2009) provides some further examples of methodological separatism in education. One of the examples discussed by Phillips is Carr's (2002). According to Carr, the issues that are relevant for educational research are "not really apt for scientific or empirical study at all" (2002, 54–55). Instead, Carr suggests that the issues that are relevant for educational research (such as human learning) are settled in terms of conceptual and normative analysis.

The next two sections will review two specific separatist claims that have been discussed in relation to educational research.

### 2.1.1 Causal explanations in educational research

Phillips discusses some examples of claims against the use of causal explanations in education. One of these claims is defended by Smeyers, who claims that:

> What can be found in so-called quantitative empirical research is indeed often of very limited use in an educational context. It belongs to a paradigm of causality, which cannot (or only at great pains and by changing the meaning of 'causality', i.e. incorporating 'reasons') give a place for the reasons human beings invoke for doing what they are doing. (Smeyers 2006, 103)

Smeyers claims that the social theories that characterize educational research rest on the concept of reason rather than on that of cause. A reason for an action is the complex of internal considerations that an individual enacts when deliberating whether or not to act in a certain way. Reasons have been characterized as theoretical re-descriptions rather than causes for action. Rather than discovering reasons in the way we would do if they were causes, many educational researchers have suggested that we interpret them. As the educational philosopher Jerome Bruner asked:

> Are not plausible interpretations preferable to causal explanations, particularly when the achievement of a causal explanation forces us to artificialize what we are studying to a point almost beyond recognition as representative of life? (1990, xiii)

The question is rhetorical here. Bruner argues that the most appropriate methodology for educational phenomena consists in the development of narrative interpretations building on *folk theories*, that is, on the weak kind of generalizations that characterize everyday interaction between individual human beings and that are typically about the meaning of social action. To give an example, an individual who is involved in the practice of using the subway forms a series of weak generalization about what people intend with their subway-related action. For instance, moving sideways at the opening door of a carriage indicates that the person intends to let someone step off the train. Such theories are interpretations of actions, but cannot (according to methodological separatism) be considered causal explanation of actions. They are a way of making sense of the way people act, but there is no way of using folk theories as general causal statements. In this way, Smeyers and Bruner are methodological separatists: they claim that the methods of the natural sciences cannot be applied to educational phenomena because a) (Smeyers) causes cannot provide reasons and b) (Bruner) reasons cannot be causes.

The aim of this subsection has been to exemplify how the criticism of causal explanations has been a part of the methodological discussion in educational research. Essay I examines an example of educational research in which an assumption is made that causal explanation is not possible in educational research. By examining the methodology used in the case, I argue that the interpretation of the data can and should be interpreted as an example of causal explanation. I therefore conclude that the interpretation of action can rest on causal explanation. This conclusion constitutes a case against separatism, since it means both that causes can provide reasons and that reasons for action can be described as causal connections.

The issues of separatism, casual explanations, reasons and interpretations have been discussed extensively in the philosophical literature. Section 3 will review this philosophical discussion.

## 2.1.2 Using qualitative methods to confirm or disconfirm hypotheses

Together with the issue of producing causal explanations, there is another methodological issue that has received major attention in educational research: the methodological status of qualitative research. This methodological issue, just like the problem of causal explanations, is relevant for the claim of methodological separatism. In fact, as I will summarize in this section, the discussion concerning qualitative methods has centered on the question of a methodological separation (or lack thereof) between quantitative and qualitative methods.

Until recently, the separation between qualitative and quantitative has been a commonplace. Many researchers (Kerlinger 1966; Smith and Heshusius 1986) have argued that qualitative and quantitative methods rest on different and incompatible conceptions of science. Typically, quantitative methods are associated with a positivist conception of science, whereas qualitative ones are most typically accepted among interpretivism-oriented researchers.

With time, this separatist position crystallized in a claim about a methodological difference between qualitative and quantitative methods. Bryman (1988) illustrates the standard approach to this issue, found in the methodological literature, describing one of the most recurrent claims concerning qualitative and quantitative methods. According to this claim, qualitative methods cannot be used to test hypotheses but only to generate them.

Bryman qualifies this as an empirical claim, stating that "qualitative researchers *often* reject the idea of using theory as a precursor to an investigation" (1988, 97). However, he offers no examples of effective arguments for the thesis that confirmation is conceptually inconsistent with qualitative methods. Nor does he provide evidence for his empirical claim. Furthermore, the volume of literature about qualitative methods makes it even more difficult to draw conclusions, but the suspicion is that a clear conceptual argument for the claim that qualitative methods cannot be used to test hypotheses has never been formulated.[1]

However, another kind of argument for the claim that qualitative methods cannot be used to support hypotheses can be reconstructed by using a number of further claims concerning qualitative methods typically discussed in the literature. This argument is based on certain axiological prescriptions about what social researcher *should* aim for. In simple terms, the argument is set to support the claim that although the concept of qualitative methods is not *per se* inconsistent with the concept of hypothesis testing, there are certain values that *should* be fundamental for the social researcher that make hypothesis testing unfit for a proper methodology of social science.

Some terminological stipulations are required in order to present this argument. First of all, in the methodological literature, the terms *deductive design* or

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[1] The problem of poor and unsophisticated claims has led some researchers to reject the distinction between qualitative and quantitative methods (Allwood 2012).

*deductive data analysis* are typically used to mean the process of theory testing. The use of the term *deductive* could suggest that only a particular template for hypothesis testing is intended, that is, the hypothetico-deductive method of hypothesis testing. However, the term deductive is used in a much broader sense in the methodological literature, applying to all types of hypothesis testing design.

Conversely, the term *inductive design* or *inductive data analysis* is used to mean the process of theory building, broadly constructed.[2]

In a number of works that have become influential in the methodological debate in educational research, Lincoln and Guba (2011; 1994; 1988; 2005) have argued that there are some axiological prescriptions that social researchers should accept and that these prescriptions are better suited to inductive design. Two examples of such prescriptions are:

- Social researchers should consider the social world as consisting of multiple realities.

- Social researchers should favor methods that entail a close interaction between observer and observed subject.

According to Lincoln and Guba, under the assumption of these prescriptions, researchers should *avoid* hypothesis testing for the following reasons:

> [The researcher accepting the anti-positivist paradigm] prefers inductive (to deductive) data analysis because that process is more likely to identify the multiple realities to be found in these data; because such analysis is more likely to make the investigator-respondent (or object) interaction explicit, recognizable and accountable; […] (Lincoln and Guba 1985, 40)

> [The researcher accepting the anti-positivist paradigm] prefers to have the guiding substantive theory emerge from […] the data because *no* a priori theory could possibly encompass the multiple realities that are likely to be encountered; […] because a priory theory is likely to be based on a priori generalizations, which, […] may provide a poor idiographic fit to the situation encountered. (Lincoln and Guba 1985, 41)

The first quote contains two claims that are relevant for our discussion: a) theory building is more likely to encompass multiple realities than theory testing; b) theory building is more likely to facilitate interaction between investigator and respondent than theory testing.

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[2] For reasons of space, I cannot enter into the discussion of the soundness of the claim that hypothesis testing is characterized by deductive inferences and that theory building is characterized by inductive inferences. However, these claims seem to be too strong. It is therefore more cautious to consider the terms inductive and deductive as mere labels.

The second quote states something similar: hypothesis testing does not fit the assumption of multiple realities since it requires a formulation of a theory *ex ante*. However, Lincoln and Guba claim that no such theory could possibly account for all the realities that are involved in the phenomenon.

Although this is an improvement on Bryman's statement of the problem, it is still a problematic claim. The key term in the first quote is 'likely': there is something about hypothesis testing that makes it unlikely to account for multiple realities. However, it is not clear what makes this connection unlikely. The authors do not provide any example or argument for this claim. The problem with the second quote is the claim of impossibility: What makes this connection impossible? Is it a conceptual or empirical impossibility?

Therefore, although Guba and Lincoln provide a more qualified claim about the disagreement between qualitative methods and confirmation, they fail to provide adequate arguments for this claim.

This section has mainly worked as a way of introducing the discussion about confirmation and testing in the context of qualitative methods and to provide a summarized showcase of the main claims found in the literature. The picture that emerges from this summary is that although the issue has been (and still is) lively discussed, and although qualified claims have been presented, there is a lack of substantial arguments in favor of or against the thesis that qualitative methods cannot be used to confirm or disconfirm hypotheses.

Essay II attempts to make up for this deficit and discusses some necessary properties of support (which I qualify as a specification of confirmation) in the context of qualitative methods. In the light of these properties, I will argue against methodological separatism; that is, I will argue that qualitative methods can be and are used to support hypotheses. This attempt will strongly rely on a specific philosophical approach to evidence and confirmation. This approach will be reviewed in Section 3.

## 2.2   Theoretical Issues in Educational Research

Whereas the aim of the previous section has been to present some examples of methodological controversies in educational research, this section tries to introduce some theoretical controversies in the field. Essays III and IV, from different vantage points, deal with the concept of learning. Learning is a theoretical issue that has been and still is debated in educational research, with consequences for both educational practice and empirical educational research. The next two subsections will present the theoretical issues related to learning that are discussed in these essays and how educational researchers have addressed these issues.

Providing anything near to a complete survey of these controversies is an impossible task, given the diverse character of educational research. If anything characterizes educational research more than any other social science, it is perhaps the proliferation of theoretical frameworks and of the controversies among

them. What I can attempt to do is isolate the specific issues discussed in Essays III and IV and locate them in the educational field.

### 2.2.1 The controversy between cognitive and social theories of learning

When it comes to theories of learning, one of the most animated controversies is that between cognitive and social approaches to learning. In order to give a glimpse of the importance of this debate, Packer and Goicoechea (2000) list some examples of such discussions, including Anderson, Reder, and Simon (1996); Cobb and Yackel (1996); Confrey (1995); Greeno (1997); Nuthall (1996); Prawat (1996); Sfard (1998).

All these contributions rest on the claim that cognitive and social approaches to learning differ concerning what learning ultimately is. The former approach identifies the process of learning as an individual mental event that result in the development of abstract representational and propositional content. Learning, from this point of view, consists in the process by which an individual, rational agent builds a theory of her neighboring phenomena. The latter approach considers learning as the result of negotiations, interactions and interrelations within a community. Learning, according to this approach, involves a process of socialization into a practice.

The objects of disagreement between the two approaches have probably been as many as the different contributions about this issue in the literature. Some have argued from the point of view of reductionism (that is, whether one position can be ultimately reduced to the other). Others have instead focused on the compatibility of the two approaches, that is, whether the two rest on incompatible theoretical assumptions. Essay III focuses on this latter issue, and so will the remainder of this section.

For instance, Lave and Packer (2008) point out a possible source of incompatibility between social and cognitive approaches to learning. In their opinion, the two theoretical frameworks make ontological assumptions that are incompatible. These assumptions are directly related to the issue discussed in Essay III, scientific representation. Lave and Packer argue that a minimal theory of learning must consist of three elements: a) A relation between subject and object of learning, b) a *direction* towards which learning points, and c) a learning mechanism. Regarding (a), Lave and Packer argue that cognitive theories of learning are *representational*. This means that, according to these theories, learning entails the agent constructing a mental or concrete picture of the learning object. Lave and Packer seem at first sight to claim that social approaches to learning are not representational and that learning does not essentially entail the development of propositional content that is intentionally related to an external learning object.

However, after a closer examination of Lave and Packer's argument, it can be seen that they do not reject the representational component of learning. In contrast, they claim that the relationship between learning subject and object in sociocultural approaches to learning is "dialectical" (2008, 34). This term indicates, in the words of the authors, that there is a sort of exchange or negotiation between the subject and the object of learning. The subject, in Lave and Packer's approach, is not a primitive epistemic agent but rather a historically and socially located practitioner. Therefore, the representational aspect of learning is not excluded but rather substituted. According to the cognitive approaches, representation is a hierarchical and unidirectional relationship: the object precedes the subject and the direction of the representation is *towards* the object (e.g., the skull in Renaissance paintings is an allegory *of* death). Instead, in the social approaches both these features of representation are absent, since the space of conceptualization of the learning subject is the result of the social relations the subject entertains within her social context. There is, in some sense, no categorical difference between subject and object of representation; the subject of learning can be the object itself. Representation is a semantic relation rather than a rhetorical one, referring to all the possible substitutions (or tropes) between representing vehicles allowed within a system or practice.

What makes Lave and Packer's discussion relevant for this dissertation is that they make a claim of theoretical incompatibility between cognitive and sociocultural approaches to learning. In this way, they argue for a separation

Lave and Packer's text is rich in potentially philosophical content, but it is not a piece of philosophical work. It does not provide differential arguments for one or the other conceptions about the nature of learning. What it does is spell out the characteristics of the two theoretical frameworks and claim that they are not compatible, as they rest on different ontological assumptions (for instance, about the nature of the representational component of learning).

Lave and Packer's discussion gives us enough grounds to claim that the theoretical controversy between cognitive and social approaches to learning has been and still is a fundamental discussion in educational research, and that this controversy is related to the concept of representation.

Section 3.3 presents some of the main proposals for a definition of scientific representation that are available in the philosophical market. Then, in Essay III, I will employ the conceptual apparatus provided by the discussion around the concept of scientific representation to argue that the cognitive and social approaches to learning might not be incompatible.

### 2.2.2    Socio-material theories of learning

Essay IV discusses the theoretical framework of Actor-Network Theory (ANT) in education. Actor-Network Theory (Latour 2007) is a theoretical framework that has been applied to many different social sciences. ANT is one of the approaches that fall under the umbrella of *socio-material* theories of learning. Fenwick

*et al.* (2015) have summarized the common denominator of these theories, claiming that:

> [Typically, in educational research] what is material is often taken to be the background context against which educational practice takes place or within which it sits, and material artefacts are often taken to be simply tools that humans use or objects they investigate. [Socio-material theories contend instead that] everyday educational activity and knowing are critically shaped through the material. [M]ateriality is not consolidated within artefacts, but is distributed, such that social as well as physical processes can be understood as material. [T]hings matter not as discrete and reified objects with properties, but as effects of dynamic materializing processes that cause them to emerge through gatherings and to act in indeterminate entanglements of local everyday practice. It is this relational materiality that is often overlooked in educational research.

Actor-Network Theory is an approach that is applied to the study of educational *practices*. This approach rests on the assumption (typically labeled as the *symmetry principle*) that the distinction between material and non-material entities should not be assumed at the outset of empirical investigations of practices but should be seen as the result of the investigation. Such terms as artifact, intention, belief, desire and, more importantly, *learning* are considered to be emergent phenomena that result from the complexity of the social and material relations that are established in an educational setting.

Concerning the nature of learning in an ANT framework, Fenwick states that:

> It is by now a commonplace in educational theory to understand learning as more than the purely individual, cognitive and acquisitive process that has driven some educational approaches. […] Notions of learning as socio-cultural participation, embedded in particular joint activity, tools and routines, have become ubiquitous in educational writings. […] However, such conceptions still tend to focus on individual learning subjects, and on their particular development through the processes of mediation and participation. [In contrast, according to ANT] Learning is an effect of the networks of the material, humans and non-humans that identify certain practices as learning, which also entails a value judgment about learning [as being] something worthwhile. (Fenwick, Edwards, and Sawchuk 2015, 6)

What makes ANT-oriented approaches in education controversial is the centrality they attribute to their ontological commitments. When it is claimed that learning is constituted in a network of different agents (or actants), the typical interpretation of the advocates of ANT is that the claim that is made is ontological rather than methodological or instrumental. ANT does not suggest an interpretation that is instrumentally fruitful and that rests on some ontological stipulations. As Jensen (2010) suggests, ANT entails a shift "from epistemology and representation to practical ontology and performativity" (2010, 7).

This claim is problematic in one sense; it is unclear whether ANT is a theoretical framework for empirical research or if it rather is a philosophical meta-theory concerning the ontology of scientific theories in general. In other words,

it is unclear how the ANT-oriented researcher should consider other approaches to learning. Should she or he consider these to be empirical descriptions, on a par with other descriptions? Or should she or he claim that the best ontological interpretation of all empirical descriptions is in terms of networks of relations?

Some of the first proponents of ANT seem to have suggested the latter. For instance, Law (2004), one of the first proponents of ANT, seems to endorse a form of constructionism about the entities and processes that are the object of scientific theories (which would include learning):

> [I]t is not possible to separate out (a) the making of particular realities, (b) the making of particular statements about those realities, and (c) the creation of instrumental, technical, and human configurations and practices, the inscription devices that produce these realities and statements. Instead, all are produced together (Law 2004: 31)

If these claims are restricted to the issue of the nature of learning in relation to the theoretical frameworks in education, we can conclude that learning is not a phenomenon that precedes our theoretical and empirical understanding of it. Instead, learning emerges from a network of relations in which empirical and theoretical concepts of learning have an agential role. Such a form of scientific constructionism that this would entail would then characterize ANT as a strong anti-realist position.

This further and final controversy is therefore a relevant matter for educational researchers who approach learning from the perspective of ANT. Should they endorse scientific anti-realism? In Essay IV, I argue that this is not the case. As I see it, the ANT-oriented researcher could even accept much of the constructionist claims of ANT, without embracing antirealism. However, in order to argue for the claim that there can be a meta-theoretical continuity between ANT's strong constructivism and scientific realism, I need to introduce a philosophical theory about the ontology of scientific theories, called Ontic Structural Realism (Ladyman and Ross 2007). This theory is presented in Section 3.4 of this introductory chapter.

The last two subsections, 2.2.1 and 2.2.2, have served as a brief showcase of the controversy between cognitive and social theories of learning on the one hand and the issue of the foundations of socio-material theories of learning on the other hand. This section has provided examples of how the issues have been discussed among educational scientists. However, I have indicated that some of these discussions rest on claims that lack reference to substantial arguments in their favor. These arguments are discussed in Section 3.

# 3 Philosophical Foundations of the Educational Controversies

Each of the issues that have been presented in the previous section has some connection with a current philosophical debate, such as that concerning causal explanations in the social sciences or the issue of confirmation and evidence. In this section, I connect each of the issues in educational research discussed in Section 2 with the philosophical discussions about naturalism, scientific representation and structural realism.

## 3.1 Methodological Naturalism and Social Science

The controversy concerning the scientific status of educational research is deeply rooted in the philosophical discussion about the methodological distinctness or continuity between the natural and the social sciences. This section introduces and qualifies the main claims that are involved in this debate. Specifically, the standpoints of naturalism and interpretivism are introduced in this section. This will also serve as a background for Essay I, in which a form of naturalism is defended and interpretivism is criticized.

Typically, naturalism is defined in terms of continuity. As Mantzavinos (2012, 224) puts it: "Naturalists since Mill have argued that human actions have to be viewed as phenomena on a continuum with other phenomena in nature and that they should thus be studied accordingly". This is, however, an ontological formulation that states that no categorical separation exists between the proper object of the natural sciences and that of the social sciences. I am not concerned with this kind of ontological continuity in my thesis. In contrast, I discuss a methodological form of naturalism. Methodological naturalism can be understood, analogous to its ontological counterpart, as a continuity thesis. Being a methodological naturalist involves claiming that there is no methodological separation or distinction between the natural and the social sciences. Without qualification, this claim looks too strong and too vague at the same time. For instance, the fact that the social and the natural sciences employ different methods seems a triviality. Therefore the thesis, intuitively, looks too strong. At the same time, no clear

definition exists as to what exactly the natural and the social sciences are supposed to contain, which makes the claim vague. Thus, for this claim to be meaningful, it is necessary to further define the claim of continuity.

### 3.1.1 Interpretivism

In order to qualify the naturalist claim, I require some help from the direct opponent of methodological naturalism: the standpoint of methodological interpretivism. In this way, I first sketch naturalism in a contrastive way and then define it in independent terms from interpretivism.

The main methodological dispute between interpretivists and naturalists has historically been that about the contrast between explanation and understanding (von Wright 1971; Henderson 1993). Contemporary surveys of the naturalism/interpretivism debate typically agree that the mark of methodological interpretivism is the claim that *understanding* is the appropriate methodology for the interpretation of human action (Roth 2003). Understanding is, however, not a univocal term but a concept with a complex historical and cultural background. A complete historical reconstruction of the term falls outside the scope of this dissertation. Therefore, our discussion will require some stipulations and an unavoidable degree of simplification. [3]

Considered in methodological terms, understanding is often defined as an *operation*. More specifically, it refers to the operation of conceptualizing, rationalizing or interpreting someone else's behavior in a way that entails that this someone has a *mind* and that this mind is capable of aim-directed action. Understanding consists in formulating a theory about an observable event and requires that this theory be expressed in terms of the aim-directed mental life of a rational agent involved in the event. For instance, explaining the fact that an inanimate object or a simple biological being such as a bacterium reacts to its environment requires a kind of theorization that lacks the kind of aim-oriented, mental life required for the operation of understanding.

If we consider understanding only in these terms, then it is intuitively uncontroversial. It only says that social science works by producing theories of the mental experience of individuals. What makes the idea of understanding contentious is its methodological distinctness from the explanations of natural phenomena. Interpretivists argue that, thus defined, understanding is methodologically separated from explanation. In summary, interpretivism implies two claims: a)

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[3] Feest (2010) provides an excellent collection of historical and geographical reconstructions of the sources and the development of the debate between explanation and understanding, including a clear distinction between the German, British and French contexts of this debate. The book successfully clarifies the fact that the different uses of the term understanding rest on different ontological, epistemological, moral and political assumptions.

the proper methodological operation of social research is that of understanding, and b) understanding and explanation are methodologically distinct.[4]

What does the separation between explanation and understanding consist of? Historically, the first phase of interpretivism was characterized by a conception of explanation as strongly dependent on the concepts of causality and law of nature. In this first phase, the bone of contention is about the possibility for interpretations of action to fit the models of explanation that at the time were considered to capture the essence of scientific explanation. In short, the difference between explanation and understanding, according to the first phases of interpretivism, is that explanation elicits causal connections and these causal connections are instances of laws. In contrast, understanding is a different kind of inference that does not elicit causes and is not couched on universal regularities. Therefore, the early interpretivist contends that the naturalist is wrong in the continuity claim, since whereas natural science aim at explaining natural phenomena by formulating causal claims that are instances of laws of nature, the social sciences aim at understanding human action, by trying, in each different situation, to conceptualize the action in terms of the local and contextual motives that might have informed the action. These motives are not causes for actions, since the same motive can lead to different actions in a different context, and the same action can be conceptualized as motivated by different reasons in a different situation.[5]

The characterization of natural science in terms of causality, laws, prediction and control has been an object of criticism and has today little popularity among philosophers of science, for many reasons that can only briefly be reviewed here. Therefore, it is better to provide a conceptualization of the debate that is grounded on a more reasonable conception of natural science.

For instance, one of the terms that has somehow lost its status of a hallmark of scientific knowledge in the philosophical debate is that of *the law of nature*. Some philosophers have argued that there is nothing like a law of nature at all. For example, Ronald Giere (1999) has argued that the generalizations that we usually consider as laws of nature work more as templates for more specific models, but are by themselves not true. Thus Giere's position is that, even though the kinds of generalizations that we would call laws are still considered to play an important role in the sciences, the capacity of identifying laws is a not necessary condition

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[4] Some more moderate interpretivists grant that understanding is a kind of explanation but they still maintain a sharp divide between the natural and social sciences. This divide consists in the fact that explanations of action are *sui generis*. The divide is motivated by the special character of intentional explanations, which makes explanations in the social sciences distinct from all explanations in the natural sciences. This position has been defended by Collingwood (1946), Dray (1957), and von Wright (1971).

[5] Many have argued for this claim, but as McIntyre (1999) claims there is a certain consensus that the main argument against the possibility of social scientific laws should be attributed to Davidson, and in particular to Davidson's *Mental Events* (1970). It should be noted that MacIntyre argues that Davidson's arguments are not efficacious.

common to all natural sciences. Other, such as Cartwright (1983), have maintained that laws could be true, but only if equipped with a host of provisos that would make them vacuously true.

Parallel to a generally more flexible approach to laws, philosophers of science also revised their views concerning the role of laws in scientific explanations. From a situation in the 1950s when the deductive nomological model of explanation was supposed to be a good candidate for the role of general model for scientific explanation, we now see a progressive proliferation of different explanation models and approaches. The contemporary debate seems to be inclined towards a form of pragmatic pluralism, fairly close to the approach to explanations defended by van Fraassen (1980). According to this approach, the different sciences produce different kinds of explanatory models depending on the problems that they need to solve. Therefore, in the same way that biology can employ functional models to explain biological phenomena, we can have statistical models in meteorology, qualitative models in sociology and so on. None of these can capture the ultimate structural features of all explanations. In the essays, I adopt this pragmatic approach to scientific explanation. I argue that, even though it may be possible that some explanations in the social sciences might not be causal, it is not the case that causal explanation are conceptually impossible in the social sciences.

This form of pluralism has led some philosophers (Bohman 1991; Little 1991; Roth 1987) to argue that the dispute between naturalism and interpretivism is unmotivated. Endorsing explanatory pluralism means that different scientific needs require different explanatory practices. I do not subscribe to this view. In contrast, in the first two essays I argue that it is possible to look for methodological continuity in the plurality of practices.

### 3.1.2   Contemporary naturalism

The contemporary debates concerning laws and explanations that I have briefly summarized here provide us with some conceptual grounds for a more updated conception of the claim of continuity which can serve as the basis of a plausible formulation of naturalism.

First of all, naturalism should not rest on a continuity claim as strong as the claim that *all* social sciences must rest on laws, or that we need to identify law-like generalizations in the social sciences in order to support the claim of continuity, since these claims are too strong even for the natural sciences. It may be true that some of the social sciences make use of generalizations that have similar properties to what we usually call laws of nature. However, this would only indicate that the social sciences, in some cases, require these generalizations as a part of their models (in the same way as Giere argues in relationship with natural science). Therefore, the lack of law-like generalizations in some social sciences cannot be a sufficient indicator for a methodological separation between the social and the natural sciences.

Secondly, the same can be said about explanations. Naturalism does not require all explanations in the social sciences to subsume particular events under universal generalizations: some explanatory practices might, but others might explain it in different ways (by referring to functions, structures, mechanisms, systems and more).

So far, I have only discussed what we do not reasonably need to require about methodological naturalism. The next step involves providing a positive qualification. Given that there are certain things we do not require from methodological naturalism, what are the components that we can reasonably require of it?

A plausible proposal in this sense has been defended by Harold Kincaid (1996). According to Kincaid, naturalism can be summarized in two main claims:

(1)  The social sciences can be good science by the standards of the natural sciences.
(2)  The social sciences can only be good science by meeting the standards of the natural sciences. (Kincaid 1996, 3)

This brings us back to the problem of defining what these standards might be. As I mentioned above, the natural sciences are characterized by various methodological practices, which makes us think that the standard of good science may vary considerably from science to science. The alternative would be to go back to the tradition of logical positivism and opt for some very abstract demarcation principle such as falsification. However, because of the problem I mentioned, this is not a viable solution.

Kincaid's proposal to solve this *impasse* is to shift to concrete criteria or *virtues* of good science. The fruitfulness of Kincaid's solution consists in the fact that these virtues are not abstract philosophical principles such as falsification but more concrete criteria related to the actual methodological *practices* of the natural sciences. As he explains:

> I make no claim to have the individually necessary and jointly sufficient criteria that uniquely divide all inquiries into two groups, namely, the scientifically respectable and the pseudoscientific. Instead, the criteria I advance are the symptoms of good science; they are indicators of good science that make no claim to completeness or perfection. To argue for naturalism, I need not show that there is a sharp distinction between the scientific and pseudoscientific. Rather, I need to show that the social sciences share the basic virtues common to paradigm instances of good science – that the social science are on the scientific end of the science/non-science continuum, if there be such. (Kincaid 1996, 47)

According to Kincaid, these scientific virtues (such as empirical adequacy, fruitfulness, coherence) are realized in the concrete practice of *empirical testing* (Kincaid 1996, 51). Good science (empirically adequate, fruitful and coherent science) can be and is tested empirically. Empirical testing is the concrete and practical process of assessing the claims made by other researchers by means of empirical data. As Kincaid argues, notwithstanding the heterogeneity of methodological practices

that characterizes the natural sciences, none of these are exempted from testing. Thus, if the knowledge generated by the social sciences is to be located at the same end of the spectrum as that of the natural sciences, then it must be possible to account for how and when the claims of social sciences can be tested and are tested.

In order to better qualify for the concept of empirical test, Kincaid focuses on three features of empirical testing: independent testing, fair testing and cross testing. *Independent testing* implies that the claim being tested does not entail as one of its assumptions the claims that inform the method of data collection and data analysis. *Fair testing* implies that the researcher is as transparent as possible concerning the assumptions of the claims to be tested. *Cross testing* implies that good scientific claims resist comparison with different sets of data and different background assumptions. These three features together constitute the practice of empirical testing, which is the mark of good science.

We thereby obtain a practice-based qualification of methodological naturalism: the continuity between the natural and social sciences consists in the fact that both share a core of virtues or symptoms of good science, in the light of which both are evaluated. These virtues or symptoms are realized in the practice of empirical testing.

Now, what is the scope of this kind of naturalism? That is, what claims should be amenable to empirical testing? Saying that the practice of testing realized the virtues of good science poses the problem: What claims in social science theories must be testable (at least in principle) for the social sciences to be regarded as being on the same side as the natural sciences? The strongest form of naturalism would claim that naturalism must go all the way: all relevant claims in a social theory must be amenable to empirical testing. This would require that social theories could not depend at all on assumptions that are justified *a priori*. However, this qualification might be too strong. It can be argued that all sciences, including the natural sciences, must at some point contain claims that for various reasons cannot be tested.

Furthermore, scientific models can contain claims that might be testable in principle but that are known to be false. These claims might be included in models for instrumental reason, because, for instance, they facilitate prediction. Hence, their testability might be possible but not desirable or even not informative. The role of claims of this kind has been discussed in biology and economics (Odenbaugh and Alexandrova 2011); they are plausibly found in all sciences.

Therefore, I suggest that a more cautious position is more plausible. For instance, I tend to assume a position similar to Quine's "web of belief" (Quine, Ullian and Ohmann 1978). All *strong* claims in a theory must be testable, where the term strong means a claim that is central to the aim of an empirical investigation. If I draw a conclusion about the role of the interaction between teacher and student for the learning outcome of a definite teaching situation, then that conclusion must be the result of testing and it must be open for further testing. However, as I discuss in Essays I and II, drawing an empirical conclusion requires

the assumption of a theoretical framework. I take it that this framework should also be *globally* testable and tested. This means that we should have some confidence about the empirical adequacy of the theoretical framework *as a whole*, and that this confidence should come from the result of some empirical testing. However, it would be too much to require that every particular claim in the framework has to be tested or is even just testable. I suggest distinguishing between the *main* claims of a theory, that is those that have the major impact in terms of empirical consequences, and all other claims. The former should be both testable and tested. All other claims should be at least indirectly testable, meaning that they are either testable or indirectly testable (they rest on other claims that are testable or indirectly testable). Some of these claims will be accepted only for their instrumental values, other will only work as definitions or stipulations, but we might require that their justification rests on claims that are tested or testable.

With this qualification of methodological naturalism in our hands, we can redefine the interpretivism/naturalism debate in new terms. What the naturalist contends (and the interpretivist denies) is that the *strong* claims made by social scientists are empirically testable. In contrast, interpretivists claim that even the strong claims of social science are either not testable or, if they are testable, they do not satisfy the requirement of understanding. In the former case, interpretivists argue that these strong claims rest on main assumptions that are not empirical, and therefore not testable (either directly or indirectly). In the latter case, the interpretivist would simply deny that such strong claims help in understanding individual actions.

An example of the former kind of interpretivism is found in the discussion about the role of rationality assumptions in the explanation of action. According to this kind of interpretivism, these assumptions must be considered conceptual and justified *a priori*. This makes them non-refinable and untestable. This position has been defended by, among others, (Collingwood 1946; Winch 1958; Davidson 1970; Rosenberg 1988) and criticized by Henderson (1993; 2002).[6]

Two substantive and widely discussed arguments against the testability of causal claims about action have been presented by Rosenberg (1988) and are discussed and criticized in detail in Essay I. In this essay, I analyze an example of social research to show how *a priori* justified assumptions have no measurable methodological consequence in the interpretation of data. This provides, as I

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[6] My approach to naturalism implies necessarily that some accounts of explanations in the social sciences are categorized as interpretivist even though they do not endorse full separation. This is the case of Risjord's explanatory coherence approach (Risjord 2000). Risjord claims that on a high level of abstraction both natural and social sciences employ explanations. This entails a form of continuity between natural and social sciences. However, at a lower level of abstraction, Risjord maintains a form of explanatory pluralism. However, both in his 2000 book and elsewhere (1998; 2005) he has claimed that norms play a necessary role in action explanations. As I discuss in Essay I, we can argue that the normative claims Risjord refers to are not testable, which means, according to my definition, that Risjord's approach fits into the interpretivist category.

argue, a case against Rosenberg's claims about limitations of testability in action explanations.

The issue of testability as a symptom of continuity between the natural and social sciences is discussed in detail in Essay II. In this essay I discuss the role of theoretical framework in support of qualitative methods in education. As I argue, the issue of the testability of these frameworks is one of the main potential difficulties in meeting the continuity requirement.

## 3.2  Confirmation and Evidence

Essay II proposes a conceptualization of empirical support that is specific for qualitative research. This section reviews the theoretical context of the discussion of this essay. As explained in 2.1.2, this issue has been discussed extensively in the debate concerning the methodology of educational research, but no substantial argument has been presented for one or the other claim. In rough terms, the aim of this section is to clarify the philosophical problems about confirmation and qualitative methods. There are two main problems:

1) The first philosophical issue concerns confirmation in the social sciences (regardless of the method used). This issue can be rephrased as the problem of using observable behavior to confirm or disconfirm hypotheses concerning reasons or intentions. This problem provides a good introduction to the problem discussed in Essay II.

The philosophical discussion about confirmation in the social sciences rests largely on the same concept of confirmation that is assumed in Essay II. This concept has been discussed by Clark Glymour in his *Theory and Evidence* (1980; 1983). I will therefore start with a short summary of Glymour's theory of confirmation.

Glymour's theory (sometimes called "bootstrap confirmation" (BC)) rests on important intuition about the relationship between observation and hypothesis. In ordinary scientific practice, hypotheses seldom share the same vocabulary as observation statements. For instance, spikes in a graph can indicate the observation of a chemical reaction, even though nothing in the graph is expressed in terms of the chemical reaction. For this reason, Glymour suggested conceptualizing confirmation as a ternary relationship between evidence, hypothesis and theory. Evidence supports hypothesis only relative to a theory that translates the vocabulary of the evidence into that of the hypothesis. Glymour's theory makes confirmation *contextual*, since evidence can confirm or disconfirm hypotheses depending on the theoretical context.

Glymour's theory works in the following way. Given a set of sentences $T$, a body of evidence $E$ and a hypothesis $H$:

BC1) $E$ and $T$ entail an instance of $H$

> BC2) It is possible to (empirically) conceive an alternative body of evidence *E'* that, in conjunction with *T*, entails an instance of 'not *H*'.

Glymour argues that the theories that are used to confirm hypotheses must be empirically testable themselves. It is not necessary that the same evidence that is used to test the hypothesis can be used to test the theory, but there must be a conceivable way to test these hypotheses. The reason for introducing this is that earlier proposals about the role of operative sentences in confirmation assumed that these statements were justified *a priori*. Glymour rebuts this claim, arguing that theoretical statements are just hypotheses that are tested on other occasions.

Glymour's conception of confirmation has formed a fruitful framework for the discussion of confirmation in the social sciences. From the separatist side, Rosenberg (1988) has argued against the testability of intentional explanations. As mentioned in the previous section, Rosenberg provides two arguments for the claim that in order to use behavioral observations to confirm intentional claims, interpreters must necessarily use generalizations that are not testable. As this is a violation of a necessary requirement for confirmation, Rosenberg concludes that hypotheses about human action cannot be confirmed, tested or refined. Therefore, the point made by Rosenberg is that the generalizations that are required for confirmation are not conceptually possible in the social sciences.[7]

From the naturalist side, Henderson (1991; 1993) has formulated a comprehensive objection to Rosenberg, arguing that observation of behavior can indeed be used to confirm, refine and reject intentional claims. His discussion starts from the acknowledgment of Rosenberg's main claim. As he states:

> [T]here are no plausible candidates for the role of interesting psychological generalizations that will get us from a set of characterizations of behavioral events to characterizations of intentional events and states. (Henderson 1991, 597)

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[7] This aspect of the issue of confirmation and evidence in the social sciences falls back into the discussion of laws in the social sciences introduced in Section 3.1.1. In this short review I only consider the claim against the conceptual possibility of universal generalizations in the social sciences. However, Salmon (1989) argues that this claim comes in three versions (the first of which is the version defended by Rosenberg):

- (Interpretivism) Universal generalizations are conceptually impossible in the social sciences. Salmon attributes this position to Collingwood (1946) and Winch (1958). McIntire (1999) has provided a detailed discussion of Davidson's argument against the possibility of law-like generalizations in the social sciences.
- (Nomological Skepticism) There are no suitable universal generalizations in the social sciences, but nothing says that there will be in the future. This position is sometimes attributed to Popper.
- (Critical theory) Universal generalizations are not desirable in the social sciences (as they violate the ethical value of human autonomy). This position is attributed to the exponents of the Frankfurt school and to MacIntire (1981).

However, Henderson claims that there is a way of testing or refining theories of interpretation. Such tests:

> are possible when we take into account how a generally adequate first-approximation interpretation can be developed that does not rely on those refinements in attributing the relevant states to the subject. (Henderson 1991, 604)

Henderson argues that the problem can be avoided thanks to the complexity of theories of interpretation. Given a theory of interpretation of a given theory *T*, what we need is a proper part of *T*, a rudimentary core that consists of rough generalizations. These generalizations simply need to fix some basic attribution of desire and possibly some concept of competing desires. Such attributions need not to be as sophisticated as the theory we want to test. So when we want to test *T*, we can do so by taking this core as a stable ground on which we can judge the theory of interpretation. Therefore, even without broad empirical generalizations about human action, it is possible to test hypotheses about intentional states.

This discussion prepares the way nicely for the discussion in Essay II. The main claim made in this essay is that there are certain features of theories used in qualitative methods that make it possible to use weaker generalizations in evidential reasoning. I argue that the universally quantified conditional sentences that are often assumed to work as law-like generalizations are not strictly necessary conditions for using qualitative behavioral data to support hypotheses. Weaker generalizations will do. Whereas Henderson's claim is epistemological (how to conceive the possibility of knowledge about intentions), mine is methodological and concerns the structure of inferences that are involved in evidential reasoning when we use qualitative methods in the social sciences.

2) The second problem concerns the concept of warrant. This term requires some words of introduction in which I will make use of the terminology introduced and discussed by Reiss (2014). It is plausible to say that all evidence instances of a hypothesis entail an increase in the degree of belief a rational agent has about that hypothesis. For instance, if I smell burning, then my belief that there is a fire somewhere close increases. If I also see smoke, my belief increases rather more. Both the smell and the smoke support the hypothesis that there is a fire somewhere. I do not know which of the two is a stronger sign of the presence of fire. The concept of support only tells me that both signs support the hypothesis, but this is not rich enough to specify a difference in belief increase. The concept of warrant has that latter function. Something warrants a hypothesis to a certain degree. If I can estimate the degree to which the two signs warrant the hypothesis, then it is possible to define which of the two signs entails the greatest increase in belief and the total increase that the two signs together generate.

Glymour's confirmation theory is a theory of support. Other approaches impose a quantitative structure over the concept of confirmation, making their language sufficiently rich to define warrant. When presenting a theory of warrant,

Swinburne (1973) detected a problem concerning the kind of evidence instances and hypotheses that are typically used in qualitative research. As is it typically is the case for theories of warrant, Swinburne defines confirmation in terms of probability. To increase the credibility of a hypothesis is to increase its probability. Now, probabilistic theories of confirmation require an estimate of how plausible a hypothesis was before we obtained new evidence (this estimate is called *prior probability*), in order to compute the degree of warrant that evidence instances entail for a hypothesis. However, Swinburne considers the case of the hypothesis that Caesar crossed the Rubicon in 49 BC. Given the historical evidence, it makes sense to say that this hypothesis is more warranted than the hypothesis that Caesar crossed the Rubicon in 39 BC. Yet, without an estimate of the prior probability of the hypothesis, we cannot compare the different hypotheses and we cannot ultimately determine how much more warranted the 49 BC hypothesis is compared to the 39 BC hypothesis. This means that if there is no way of resolving this problem, the best we can get is a theory of support for qualitative methods, but not a theory of warrant.

In a number of articles, Miller and Fredericks (Fredericks and Miller 1988; Miller and Fredericks 1989; Miller 1990; Miller and Fredericks 1991; Miller 1992; Miller 1999; Miller and Fredericks 2008) discussed Swinburne's problem for qualitative methods. The claim that is defended in these papers is basically the same: the concept of support is not problematic for qualitative methods but that of warrant might be, because of Swinburne's problem about prior probabilities. However, Miller and Fredericks claim that it is possible to develop further methods that can be used to translate qualitative statements into quantitative ones. The important feature of Miller and Fredericks' approach is that it rests on the acknowledgment that qualitative methods alone cannot ground warrant. Some further methods are required.

Essay II concerns only the concept of support and does not discuss warrant. However, it has relevance for Miller and Frederick's discussion. First of all, if the conclusions drawn in Essay II are correct, then Miller and Fredericks are wrong in stating that the concept of support is not problematic for qualitative methods. As I argue, support is indeed problematic, to an extent that it requires a specific conceptualization for qualitative methods.

This makes the issue of confirmation and qualitative method a special case of the issue of methodological continuity versus separation. If it is not possible to conceptualize either support or warrant for qualitative methods, then the separatist has a strong case. If, as argued in Essay II, support can be conceptualized for qualitative methods, but it requires a specific conceptualization, then the naturalist has the burden of proof about why we should interpret this new conceptualization in naturalist terms (I argue for such an interpretation in Essay II). Finally, some researchers might find the impossibility of conceptualizing warrant for qualitative methods a sufficient case for separatism. If this is correct, then Essay II only does part of the job of a naturalist interpretation of qualitative methods.

## 3.3 Scientific Representation

This section attempts to reconstruct the philosophical problem of scientific representation, which is the core concept of Essay III. This will help to clarify the terminology used in the essay and will work as the background for a short postscript in the next subsection.

The term scientific representation is used in everyday language to mean either the relation between the artifacts of science (models, theories, equations, graphs and so on) and the world of phenomena or the artifacts themselves that are used to represent the world. In philosophy of science, most of the attention has been directed towards the relational use of the term.

First of all, we can distinguish two main groups of approaches to scientific representation. On the one hand, those that claim that representation is best understood as a direct relationship between what is represented (the *target* of representation) and what is used to represent the target (the vehicle of representation). This relationship is supposedly dyadic and *objective*; that is, it is a relation that does not obtain if the vehicle is related to something else than its intended target. For instance, a model of a bridge is a representation of a bridge if and only if it has the relevant properties of its target. Thus, representation is the relation that is exemplified by the relation between a model bridge and a full-scale bridge. This relationship has been defined in various ways, but the two main poles in the spectrum of characterization of representation are similarity (SIM) and isomorphism (STR). According to the former:

SIM – *A* represents *B* if *A* is *similar* to *B*.

Whereas, according to the latter:

STR – *A* represents *B* if *A* is *isomorphic* (or some other weaker *structural* relation) to *B*.

On the other hand, we have those that have suggested that, however defined, a dyadic and objective view of representation will be unsatisfactory. These attempts have remarked how both similarity and isomorphism are neither sufficient nor necessary conditions for scientific representation (Suárez 2003). This second group of concepts of representation has instead focused on the kind of *work* done by the vehicle of representation in relation to its target. Many of these approaches have stressed the ability that models have to allow *surrogative reasoning*. For instance, the possibility of manipulating a model bridge makes it possible for us to draw conclusions that can afterwards be applied, *ceteris paribus*, to the full-scale bridge. This is an example of how models allow surrogative reasoning.

Among the latter group of approaches there is one that has gained certain popularity. This approach, sometimes labeled as *inferential*, has been defended by Suárez (2004), Swoyer (1991), Contessa (2007) and Rodríguez and Bonilla (2009).

According to this approach, representation relation between a target *B* and a vehicle *A* is best understood as:

> INF – *A* represents *B* only if (a) the representational force of *A* points towards *B*, and (b) *A* allows competent and informed agents to draw specific inferences regarding *B* (Suárez 2004, 773).[8]

Here, some complications in defining different positions arise. Whereas it is clear in what way INF opposes all dyadic accounts, many advocates of dyadic positions (especially advocates of similarity) make claims that overlap with INF.

This asymmetry has its origin in yet another distinction, that is, between *successful* scientific representation and scientific representation *simpliciter*.[9] As I mentioned earlier in this section, defenders of the surrogative reasoning approaches like Suárez have criticized the criteria for successful representation, such as similarity and isomorphism, arguing that they are neither sufficient nor necessary for scientific representation *simpliciter* (Suárez 2003). On the other hand, many defenders of SIM concerning successful representation seem to endorse a functionalist view very similar to Suárez's as defining necessary conditions for scientific representation *simpliciter*. For instance, Giere (1988; 2004; 2006; 2010) and Weisberg (2007; 2010) argue both for the intentional character and for the inferential function of representations. Giere and Weisberg agree with Suárez that the polyadic and inferential characters of representation are necessary conditions for scientific representation *simpliciter*. The contention against Suárez is that further criteria are needed in order to account for successful scientific representation, and, *pace* Suárez, this criterion is *generally* that of similarity. Therefore, in the economy of Suárez's critique of similarity, the real disagreement regards the possibility of introducing necessary (and/or sufficient) conditions for successful representation *over and above* the necessary conditions for scientific representation *simpliciter*.

The situation is clearer in the case of the other major dyadic approach, that is, the position that I denoted as STR, which sees isomorphism, or some other weaker mapping, as constitutive of the representation relation. This is at present the only strictly speaking dyadic approach in that it takes a structural relation as constitutive of both representation relation between target and vehicle and of successful representation. In the past few years isomorphism has lost popularity in favor of weaker structural relations. For instance, Bueno and French (2011) discuss what is probably the most developed version of STR. The proposed approach in this paper is that of *partial structures*, and partial isomorphism. Bueno and French discuss furthermore in a very detailed manner the relation between

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[8] It may be remarked that some educational researchers have investigated the concept of representation in similar terms. For instance, the concept of *affordances* could be analyzed in inferential terms. For a discussion of the term in an educational framework, see Kress ( 2001).

[9] This term was introduced by Contessa (2007).

INF and their version of STR, arguing that (a) the inferentialist is right in attributing to surrogative reasoning a central role in representation but is wrong in claiming that the role of surrogative reasoning constitutes a case against the structural approaches; (b) the inferentialist might be right that the structuralist approach is unable to account for the intentional character of representation but is wrong in attributing a central role to the agent's intentionality.

There is a further group of approaches that, in the economy of my argument, can be grouped together with INF. This is indeed a group of functional approaches in that they stress the importance of the use of representations as ways for learning about the world, but, unlike INF, it makes stronger claims about the independence of models from the world. These approaches see models as independent artifacts that are constructed and manipulated so that we can learn from them (Knuuttila 2011; Morgan and Morrison 1999). These artifacts are fundamentally problem-oriented. Some of these approaches still maintain that models have representational properties, whereas others maintain that some models are strictly speaking non-representational.

Let us try to schematize the situation.

| Account-family | How are models understood? | What do models do? | How successful modeling is understood | Who argues for it? |
|---|---|---|---|---|
| Informational | In terms of the structural relation between vehicle and target | | The relevant morphism defines successful representation | Bueno and French |
| | In terms of the intentions of the modeler | | Similarity | Giere, Weisberg |
| Functional | | Allow surrogative reasoning | No general criterion | Suárez, Bailer-Jones, Frigg, Contessa, Swoyer |
| | As concrete epistemic artifacts | Allow modelers to learn, either about the model itself or about an external target | | Knuuttila, Morgan & Morrison |

*Table 1 – Positions in the contemporary debate on scientific representation*

This very brief introduction is aimed at providing an explication of what is meant by the term representation in Essay III. The next subsection will try to address a number of issues that remain unresolved in the essay.

### 3.3.1 A short postscript for Essay III

Essay III deals with the issue of scientific representation in science learning and is a discussion on its own. Therefore, this essay might need some clarification concerning how to connect it to the context of this doctoral dissertation.

Intuitively, the phenomenon of learning, considered as an abstract 'philosophical' concept, involves representing. This means that it is essential to the concept of learning that it involves some act of representation. The essay restricts the scope of learning to science learning, and consequently the scope of representation to scientific representation.

In this essay I analyze two different and contrasting theoretical frameworks in empirical educational research and look for the concept of 'scientific representation in learning' that can be reconstructed from the analysis of the application of these frameworks. The first approach is the constructivist one, exemplified by Halldén's study (1999). This approach focuses on the cognitive processes involved in learning. The second approach is the sociocultural one, exemplified by Roth and colleagues' studies (Roth 1996; Roth and Tobin 1997). This approach focuses on the role of social interactions in learning. From the analysis of how these studies model learning processes, I reconstruct two properties that, from the perspective of these two approaches, seem to be necessary for learning.

> LP1)  Learning is an intentional act
> LP2)  Learning is embedded in social norms

With these two properties at hand, I now go on to examine three theories of scientific representation and look for a theory that can account for both. Here, the gist of the argumentation is that if we conceptualize the representational part of learning, then this conceptualization should account for these two properties. However, each property is derived from a theoretical approach. Therefore, it is not clear if it is possible to formulate a conceptualization of the representational part of learning that can account for both properties. It is possible that a conceptualization of representation in learning can only account for one of the properties. This would indicate that the two frameworks are indeed incompatible, and that their incompatibility is somehow related to the representational component of learning.

Here comes the first connection between Essay III and the whole doctoral dissertation. If there is a compelling argument that some theory of representation is able to account for both these properties of learning, then we can claim to have an indication of a theoretical bridge between the constructivist and sociocultural approaches to learning. If a theory *T* accounts for both of the properties above, then we can claim that constructivist and sociocultural approaches to learning both rest on a common concept of representation.

In Essay III I argue that Suarez's INF is capable of accounting for both properties, whereas two other theories of scientific representation (Giere's intentional

theory and Woolgar's Science Technology and Society (STS) approach) cannot. I therefore conclude that INF is a better candidate for a theory of representation in learning than the other two theories.

If the essay had ended here, we would have had a nice, straightforward way to place Essay III in the general narrative of this dissertation: the essay points out a point of connection between two theoretical approaches that have been argued to be irreconcilable. Hence, we would have had a further example of veiled continuity.

However, there is a complication that I introduce briefly in the final section of the paper, and for which I sketch a brief solution in the same section.

The complication orbits around the age-old issues of the sources of normativity in social theorizing (for a recent overview, see Risjord 2016). In short, the problem is the way in which social norms are empirically investigated within the two theoretical approaches.

Even though the cognitive approach to learning is reconstructed as the source of LP1, this approach stresses the importance of the normative dimension of learning too. The learning agent constructs a conceptualization of the learning objects depending on what the agent deems to be *appropriate* for the situation. The agent interprets the task and forms hypotheses about the genre-specific way of inferring a solution. Therefore, misconceptions might originate in the fact that the agent interpreted the task as belonging to the *wrong* disciplinary context. As I argue in Essay I, in the context of the cognitive approach to learning, disciplinary norms are analyzed in terms of the beliefs of the individual that are involved in the situation. This concept of disciplinary norms fits the philosophical approach that takes social normativity to consist ultimately in some coordination of many individual beliefs (Bicchieri 2006). This approach claims that social norms are complex types of conventions that emerge when enough individuals in a group share two types of belief:

a) a belief that a norm is accepted within a group (the expectation that most people will follow)
b) a belief that violating that norm will lead to consequences (the expectation that most people will expect everyone involved to abide by the norm)

In contrast, sociocultural theories (from which I derive LP2) typically approach disciplinary norms as *practices* (Reckwitz 2002; Rouse 2007). Practices are complex social phenomena that are taken to be irreducible to, and typically conflicting with, the beliefs of the individuals involved in the practice itself. A practice will emerge when each individual observes the behavior of a community concerning some issue, interprets signals of good and bad performance, forms a conceptualization and acts according to it. What makes the concept of practice a complex one are the following properties of practices:

a)   the individual's conceptualization can be tacit and *practical* rather than explicit and verbal (Collins 2010);

b)   each individual will form a different conceptualization (which might or might not be verbal), with typically little or even no internal coherence among the different conceptualizations within a community;

c)   as soon as each individual forms a conceptualization and starts to act, other individuals (whose behavior might have influence on the first individual) might slightly update their conceptualization in the light of the behavior of the new member, making the practice a constantly changing, open phenomenon.

Thus the practice of school mathematics representing will depend on the practice of assessing school mathematics representing, which will depend on the practice of assessing this assessing practice and so on.

Cognitive and sociocultural approaches disagree about how to understand LP2. According to the cognitive approaches, LP2 means that the normative beliefs of the participants of a learning situation have consequences for learning, whereas the sociocultural approach understands LP2 as the claim that learning is the result of a practice.

We arrive finally at a formulation of the problem: the disagreement does not only concern the *formulation* of LP2; it is a deeper one. A number of philosophers (Brandom 2009; Rouse 2007) have proposed compelling arguments that the concept of practice is irreducible to and incompatible with the belief-based conception of social norms. Briefly, if we try to formulate a statement that summarizes verbally what norms are accepted, based on the observation of the practice, then there will be infinitely many alternative and incompatible formulations that fit the practice equally well. Therefore, according to the practice theorist, the belief-based approach rests on the (wrong) assumption that social norms can be 'summarized' in a consistent set of normative statements. Similarly, if instead we try to describe the practice in terms of regularity of behavior, we will be able to formulate infinitely many reciprocally incompatible descriptions that fit the observed behavior.

Hence, if (according to LP2) learning is embedded in social practices, and if the practices can and typically do diverge from the individual intentions of the learner, then it is unclear how INF can incorporate LP1 and LP2, given that they seem typically to conflict with one another.

The detection of this problem means that the claim of a suggested hidden connection between sociocultural and cognitive approaches to learning must be taken with caution.

In the final section of Essay III, I attempt an argument that deals with this contradiction. I argue that INF is formulated in a way that incorporates two types of normative content, belief and practice-based. Recall the definition of scientific representation according to INF:

INF – *A* represents *B* only if (a) the representational force of *A* points towards *B*, and (b) *A* allows competent and informed agents to draw specific inferences regarding *B*.

As I argue, the term "competent" appearing in condition (b) should be interpreted according to the practice theory of norms. Therefore, an agent is competent if she or he is a part of a practice. The learner is part of a teaching/learning practice, since she/he is immersed in a complex of normative relations of assessment and counter-assessment. The learner is *competent* for the situation if she/ he uses *A* and *B* in a way that other participants acknowledge as relevant for the practice. The competent learner is *fluent* in representing if other individuals involved in the practice see her/ his use of *A* and *B* as appropriate. Appropriate should not, however, be confused with correct, since a practice requires all varieties of correct and incorrect uses. Besides, the concept of practice means that different users might formulate different personal interpretations of the concept of "correct use". As a result, what makes a competent user can without contradiction diverge from what an individual learner intends to do, using *A* to represent *B*.

The second normative content is instead that which specifies the *intended use* of a vehicle of representation. This is what established the direction of representation (the fact that *A* points towards *B*). This direction is also the result of a social norm of representation; however, as I argue in the essay, this normative content is better analyzed according to the belief-based conception of social norms.

If the intended use of representation is interpreted according to this conception of normativity, it is consistent with the intentionality of learning (LP1). Therefore, we get a conception of representation that incorporates two notions of normativity and includes two terms that are both involved in representing but that can occasionally diverge from one another: the concept of a competent user and that of the direction of representation. Both are normative concepts, but of a different kind of normativity.

This attempt to find a solution to the incompatibility of LP1 and LP2 is intended as a suggestion, and might be considered as too good to be true. Therefore, even if it can still be said that Essay III conveys the idea of a connection between sociocultural and constructivist models of learning, this connection should be considered only as tentative.

In conclusion, in the light of this consideration, I think it would be more accurate to say that Essay III individuates the locus of the incommensurability between constructivist and sociocultural theories of learning, that is, the different sources of the normativity of representation in learning, but it does not succeed all the way in finding a solution for this incommensurability. It suggests that INF could work as solution for this incommensurability, since INF seems to incorporate both sources of normativity of representation in learning, but this solution

is only a suggestion since it does not discusses how INF can incorporate both kinds of normativity without generating a contradiction.

In the last two sections I have provided a philosophical context and a lexicon for Essay III and discussed the way in which this essay is connected to the whole dissertation. The next two sections will do the same for Essay IV.


## 3.4   Ontic Structural Realism

Essay IV proposes a philosophical approach to the ontology of learning. This approach, called Ontic Structural Realism, is introduced in this section.

Let us introduce Ontic Structural Realism (OSR) by means of the claims that are common to all its proponents. These introductive words are grouped into three main questions: a) What are OSR's main claims? b) What motivates OSR? and c) What justifies belief in OSR's main claims?

First of all, OSR is a theory about how the world is. As a form of scientific realism, it claims that successful scientific theories correctly represent the world. However, it is a moderate form of scientific realism, since it implies the claim that only a certain part of scientific theories actually represents the world. It is a form of structural realism, in that it claims that the part of theories that successfully represents the world is the structural part (typically understood as the mathematical structure of physical theory). Finally, in contrast to Epistemic Structural Realism, it does not claim that structure is all we can know about the world, but that structure is all there is. Hence, OSR commends us to ontologically commit to all and only the structural part of our best scientific theories. This leads to OSR's most controversial and discussed claim: there are no genuine individual entities. If structure is all there is, then the basic constituents of reality are relations and not individual entities. The status of these latter is a matter of debate among OS Realists.

Further, OS Realists seem to agree about what *motivates* OSR: the tension between the pessimistic induction argument and the no-miracle argument. Roughly, the pessimistic induction argument says that the fact, indicated by history of science, that the basic ontology of scientific theories is not retained when theories change commends us to maintain a skeptical attitude towards the ontology of our best theories. This is of course an argument against scientific realism. Equally roughly, the no-miracle argument says that skeptical and constructivist theories of science make the technological and predictive success of our best theories a miracle, and should therefore be rejected. OSR is an attempt to solve this tension by accepting constructivism towards individual entities but not about structure. This allows us to account both for the success of science and for the change in ontology that occurs with theory change. In fact, contrary to individual entities, structure seems to be retained with theory change (Worrall 1989).

This last claim brings us to the last aspect of OSR: the advocates of OSR seem to agree that OSR is supported by the analysis of our best physical theories. First

of all, structural continuity has been observed with the shift between physical theories (Worrall 1989). Secondly, as argued by Ladyman "Ontic structural realists argue that what we have learned from contemporary physics is that the nature of space, time and matter are not compatible with standard metaphysical views about the ontological relationship between individuals, intrinsic properties and relations" (Ladyman 2014). Votsis and Frigg express the same claim: "OSR is forced upon us by modern physics, since many parts of modern physics are in fact incompatible with traditional metaphysical views" (Frigg and Votsis 2011, 265). The crucial point of these arguments is that, at the level of fundamental physics, structural interpretation seems to be the best interpretation of the ontology of physical phenomena. Therefore, in all these examples, the claim is not only that we should assume OSR in order to avoid the pessimistic induction, but a stronger one according to which we cannot interpret the ontology of fundamental physics otherwise.

These three points give us the core of OSR. Outside of this core, there is still a great deal of disagreement on a number of important matters. For instance, defenders of OSR still disagree about how to intend structure, what the basic ontology of a structural world is (the question whether a structural world should consist only of relations and no individuals, relations and individual objects, or some further alternative (Ainsworth 2010)); and, finally, whether OSR can/should be extended outside the scope of physical theories, that is, to the special sciences. This latter issue implies the question whether we should extend the relational ontologies to all levels of reality. Accepting the extension of OSR to the special sciences amounts therefore to the following thesis: all that exists, at all levels of reality that are described by scientific theories, is structure.

The framework of OSR is used in Essay IV to analyze a particular methodological and theoretical approach for the study of networked learning, viz., Actor Network Theory (Latour 2007; Fenwick and Edwards 2010). This approach is usually claimed to entail certain anti-realist and constructivist commitments. Essay IV examines a case of application of the Actor-Network Theory in education, arguing that, assuming OSR, it is consistent with scientific realism.

### 3.4.1   Some comments on Essay IV

Before closing this section, it is useful to devote a few words to the scope of Essay IV. With the educational and philosophical background spelled out in Sections 2.2.2 and 3.4, this discussion can now be clearer.

What I propose in the article is an OSR interpretation of ANT. That is, I consider some claims of ANT and I argue that OSR can provide an account that justifies these claims. Moreover, I argue that assuming OSR as an ontological framework for ANT, an ontological problem of circularity affecting ANT (identified and discussed by Collin (2011)) can be solved.

An interesting consequence discussed in the essay is that ANT is typically considered to be an anti-realist form of constructivism, whereas OSR is a form of scientific realism. Therefore, the possibility of framing ANT in OSR suggests a connection between realist and anti-realist approaches to sciences and, as in the case discussed in the essay, in the scientific investigation of learning.

Section 2.2.2 provided examples of claims that ANT rests on anti-realist claims. It is my experience that the researchers using ANT tend to commit to similar claims. However, it is important to remark that Fenwick and Edwards, two of the main advocates of ANT in education, have made some claims that point in a different direction. In their opinion:

> ANT's ideas are best utilized as an approach, a sensibility and a method for understanding, not a totalizing theory of the world and its problems. (Fenwick and Edwards 2010, 5)

The term 'sensibility' might be perceived as vague. I think that an alternative formulation that might do a slightly better job would be to say that ANT provides a *language* for a certain kind of investigation of the world. Since it is not completely implausible that sharing a language might be realized in different individual commitments, we could take that precisely as the case of ANT. ANT-oriented researchers might share a language of relations and performative ontologies but might commit to different claims about the status of scientific claims. If this is correct, the conclusion that ANT is consistent with OSR might not be as theoretically surprising as I suggest in the paper.

I think that this remark is plausible, but I still believe that the problem is not what ANT contains but rather the commitments that are typically ascribed to ANT. In other words, even if I can concede that ANT is not per se an anti-realist framework, it is typically used by researchers with anti-realist commitments. However, even in the case of a downsized scope of Essay IV, there are some interesting remarks that can be made in relation to these conclusions.

The first remark is that the consistency between OSR and ANT points to some unexpected and interesting analogies between an ontological framework for physical theory and an approach to social research. The fact that realism about physical structure rests on some considerations that are similar to those of ANT suggests an analogy between the study of social and physical phenomena that is worth deeper investigation.

The second remark is that one of the main elements of the language of ANT is the stress on the relational nature of the objects of scientific investigation. The focus on relations is a feature that is shared by a number of socio-material approaches (Barad 2003). Now, although terms such as relational, relationality and relational ontology are used by a growing number of researchers, the main objection is that these approaches typically fail to provide an account of what these terms ultimately refer to. In particular, when meeting similar claims, it is plausible to expect an account of the following:

- The researcher should provide an account of the nature of relations. Do they exist without *relata*?
- If relations exist, then the researcher should clarify the status of *relata*. They might exist on a par with relations or be a bi-product of the latter.
- If *relata* are a bi-product of relations, then the researcher should clarify whether they are fictions (or maybe social constructions) or simply ontologically secondary (they exist, but relations are ontologically more primitive).

The advantage of OSR is that it can work as a framework which, taking physical theory as a point of department, provides an account of what relational ontology might amount to, answering all of the points above. Ainsworth (2010) provides an extensive taxonomy of the different accounts of the metaphysics of relation assumed by different varieties of OSR.

Therefore, even if we concede that ANT is not *by default* a form of anti-realism, there are anyway a number of relevant consequences that can be drawn from the main conclusion of Essay IV, that OSR can consistently be used as a framework for the ontology of ANT.

# 4 The Methodology of Philosophy of Science

From a methodological point of view, this doctoral dissertation is a work in philosophy of science. By this, I mean that the methods I use are those of the philosophy of science. In this chapter I discuss some issues concerning the methodology of philosophy of science that have relevance for the four essays that are included in this dissertation.

The term 'methodology of philosophy of science' can be deceptive. Someone might interpret it as suggesting that there is one or one main way of conducting philosophical investigations of scientific issues. Instead, it is more correct to say that philosophers of science can work in different ways, including the analysis of concepts, the employment of logic and other formal methods, case studies, experiments and simulations (Crupi and Hartmann 2010).

For quite a long time, logic has been one of the main toolboxes of philosophers of science (Horsten and Douven 2008). The main function of using logic in philosophy of science is to eliminate all references to contextual aspects of science (such as the specific content of theories or observations) and to highlight the structural relations between the different items involved in scientific reasoning (such as observation, theory and hypothesis). In order to highlight and isolate the structural features of reasoning, philosophers have introduced several artificial languages in which all reference to contextual facts is eliminated in favor of variables and constants. The validity of reasoning is then defined in terms of the logical operators that connect the variables and the constants. Formalization is only the first step of this methodology: it provides a formal representation of the scientist's everyday language. The crucial step is instead the analysis of the logical validity of the reasoning, expressed in syntactic or semantic terms. In this way, philosophers provided a *justification* of scientific reasoning based on logical validity: scientific reasoning is rational because it is logically valid. This kind of philosophical justification is normative: it entails an evaluation concerning correct reasoning, such as logical reasoning, according to which one ought to reason in a logically sound way.

Although formal methods are still an important tool for philosophy of science, the aptness of formal methods as a justification for scientific reasoning has in the past few decades been subject to criticism. One problem of formal methods is that although they might indeed explicate the rational foundation of scientific reasoning, they present a model of it that is so abstract that it becomes uninformative. This means that some contextual facts (such as the actual content of theories, or the individual beliefs of the researcher, or the cultural context in

which research is located) might be too important to be disregarded. We might agree that correct scientific reasoning is logically valid but still contend that explicating the sole formal features of reasoning is not very informative. As a result of this problem, some researchers have suggested that philosophy of science should instead look, for instance, at how imperfect agents go about reasoning correctly, that is, how to approximate correctness when reasoning is not completely valid (as some argue is the case of scientific reasoning). Giere (1985) proposes a model of scientific decision-making based on bounded rationality that goes in this direction.

Other philosophers of science have endorsed a radically different view. In their opinion, the fact that scientific reasoning is logically valid is neither a necessary nor a sufficient condition for *good science* (Bogen and Woodward 2005). Therefore, philosophy of science should instead look at the particular and contextual conditions that make different scientific enterprises successful. This branch of philosophy of science favors the explanation of scientific practices through empirically adequate causal analysis. Instead of looking for the logic of correct reasoning, these philosophers might look at the ways cognitive science can be used to explain scientific decision-making. Or they might look at how the interaction between scientists and the practices of scientific communities allow for scientific discoveries and explanations.

It is important to remark that many of these practice-oriented philosophers still agree with the formally oriented ones about what the main aim of philosophy of science should be: providing an account of the rationality of science. Bogen and Woodward (2005) and Giere (1988) are, in my opinion, examples of this tendency. They might, however, disagree about the foundation of this rationality. The former might argue that this foundation is an abstract idea of correctness of reasoning; the latter might contend that it is a matter of causal empirical facts.

In contrast, some traditions have taken the path of denying that justifying scientific reasoning is possible or desirable (many sociologists of science, such as Latour and Woolgar (1979) have argued for similar claims). In these cases, the causal explanation of science does not aim at a justification of science but only at providing an empirically adequate description of scientific practices.

Independent of the aim of the investigation, the practice-oriented development in the study of science has led to an increase in the use of case studies as a methodology of philosophy of science. Case studies are intuitively appropriate for a descriptively accurate description of science, since they provide detailed analyses of practices. Essays I and II are examples of case studies of scientific practices used to evaluate philosophical claims.

As with formal methods, the use of case studies is not free from methodological problems. In particular, case study methodology is affected by a *vexata quaestio* concerning how to generalize from a single case. This issue affects Essays I and II as well. For this reason, I will use the remainder of this methodological chapter to discuss this issue.

In Essays I and II, I use the analysis of single cases to defend general claims:

*Essay I*: The analysis of the case of the study by Pettersson and Scheja (2008) is used to formulate a general claim about the methodological separatist qualification of interpretivism. The general claim is that causal explanations are possible in the social sciences.

*Essay II*: The analysis of the case of the studies by Rostvall and West (2001) is used to formulate a general claim about qualitative support. The general claim is that data can be used to support hypotheses in qualitative methods.

Hence, the question is: How can general claims be inferred from single cases? I believe that two arguments can be provided that can justify this inference.

First of all, we could say that knowledge from case studies is never really restricted to the chosen case. We always learn something general, even when we look at single cases. If the analysis of the single case is detailed enough to identify disturbing factors and relevant causal connections, then inference from case to population can be justified.

The second and more cautious argument is that it is true that we typically cannot make inferences from single cases to populations. However, in certain cases, this inference is justified. These are the so-called *critical cases* (Flyvbjerg 2006; Ruddin 2006). These cases are those that are capable of showing fundamental or crucial facts that are of such relevance for general claims that one case is informative enough (given that one can argue for the accuracy of the single account).

This latter justification is more appropriate for Essays I and II. In both cases, I consider basic claims that are supposed to apply to all cases. In the case of Essay I, I consider the basic claims of interpretivism:

    i)       Causal explanations of actions are not possible.
    ii)      Causal explanations of actions are normatively deficient.

These claims are very broad and strong: they are supposed to apply to all cases of attempted explanations of actions. Now, from the analysis of how Pettersson and Scheja moved from data to conclusions, I formulated an argument that, in this case, the kind of inference made by the researchers could be considered a causal explanation of action. Likewise, I provided an argument that this explanation is normatively efficient.

If the arguments I provide in Essay I are sound, this is enough to defeat i) and ii), just because the fact that they are supposed to cover all cases of explanations of actions makes a single defeating case enough to draw a general conclusion that i) and ii) are not valid. For this reason, the case considered in Essay I is a critical case for i) and ii).

Similar considerations apply in the case in Essay II. Here, I put forward a criticism and a positive claim. I argue against the claim that qualitative methods

cannot be used to support hypotheses, and I put forward a conceptualization of qualitative support. This latter consists in the claim that:

a) Qualitative support rests on default rules as a specific form of generalization.

b) Qualitative support is contrastive: observation can, at best, discriminate between two hypotheses.

In this case, the argument I propose does not aim at defeating a claim but at substantiating claims a) and b). In this case, the concept of crucial case is even more relevant. The arguments that I present in favor of a) and b) aim at showing that qualitative support *could not work otherwise* than in the way described in a) and b). I argue that if a) and b) did not obtain, then certain steps in the process of applying qualitative methods would be impossible. Therefore, the case I discuss is critical in that it is able to show the features that are necessary for the application of the method and are such that the method could not work without them. Therefore, one case is enough, because it is capable of showing features that are so crucial that they must apply generally to all cases. Hence, the case shows that a) and b) are used in the case to allow certain inferences, and I provide an argument that without a) and b), those inferences would not be possible.

# 5 Summary of the Essays

## 5.1 Essay I: *Interpretivism and Causal Explanations*

This article criticizes a view about the interpretation of human action, labeled in the text as interpretivism. This view posits a sharp separation between the natural and social sciences, to the effect that the methods of the latter cannot be applied to the former. I criticize this standpoint by reconstructing a case of educational research. As I argue, the case I analyze indicates that the arguments in support of interpretivism are contradicted by what social researchers can actually achieve. I conclude that the interpretivistic claims lack support and that the general separation claim seems problematic.

## 5.2 Essay II: *Qualitative Research Methods and Empirical Support*

In this paper I discuss the concept of empirical support in the context of qualitative methods (qualitative support). A conceptualization of empirical support that is specific for qualitative methods is proposed. This conceptualization is based on the analysis of a case of qualitative research. I provide arguments that this conceptualization identifies specific and non-trivial properties of qualitative support and that, at the same time, this conceptualization supports the claim that there is no methodological distinction between quantitative and qualitative methods concerning empirical support.

## 5.3 Essay III: *Scientific Representation and Science Learning*

In this article I examine three examples of philosophical theories of scientific representation with the aim of assessing which of them is a good candidate for a philosophical theory of scientific representation in science learning. The three candidate theories are Giere's intentional approach, Suárez's inferential approach and Lynch and Woolgar's sociological approach. In order to assess which theory is more promising, I compare the three candidate theories to two aspects of scientific representation in science learning that emerge from empirical research on science learning. I label these aspects as the intentional and normative character of scientific representation in science learning. As I argue, whereas the other

competing accounts of scientific representation can only capture one of the two aspects highlighted in this article, the inferential conception has the capacity to capture them both in a coherent way. Thus, I conclude that the inferential conception seems to be a fruitful philosophical theory of scientific representation in science learning.

## 5.4   Essay IV: *Actor Network, Ontic Structural Realism and the Ontological Status of Actants*

In this paper I discuss the ontological status of actants. Researchers have argued that actants are the basic constituent entities of networks in the framework of the actor-network theory (Latour 2007). I introduce two problems concerning actants that have been highlighted by Collin (2011). The first problem, according to Collin, is that actants cannot simultaneously be constituents and products of a network. The second problem, as Latour suggests, is that if actants are fundamentally propertyless, it is unclear how they combine into networks. I suggest that both of the problems that Collin ascribes to actants rest on the assumption of a form of object ontology, that is, the assumption that the ontological basis of reality consists of discrete individual entities. I argue that the solution to this problem consists in the assumption of an ontology of relations and, furthermore, that Ontic Structural Realism (Ladyman & Ross, 2007) is the kind of ontological theory of relations that can solve this problem. While my proposal may be regarded as an attempt to solve two problems, it is also an experiment in reconciliation between the analytic and constructivist philosophies of science.

# 6   Concluding Remarks

The four essays in this thesis arrive at different conclusions. In this section I try to summarize and roughly connect these conclusions.

The first set of conclusions concerns the methodology of educational research. These conclusions are drawn in the first two essays, which show a clear connection and draw almost the same conclusion. In very rough terms, the results of Essays I and II come to the conclusion that methodological separatism about interpretations and support is not sound, and that methodological naturalism is a more promising account of educational science.

The methods of educational research are not methodologically distinct from those of the other social and natural sciences. Of course, this is not to say that the methods of education are identical to those of the other sciences (including the natural ones). Instead, the essays indicate continuity between the different methods. The main point made in these essays is that methodological continuity can be observed even in the diversity of methods.

In Essay I, this claim is made in a more general way; the operation called understanding is not methodologically distinct from that of explaining. This means that I agree that understanding can in many cases be what social researchers try to achieve, but I reject the claim that understanding is incompatible with explaining. In the case of Pettersson and Scheja's article, the researchers tried to make sense of, or to *understand*, how students' personal considerations affected their conceptualization of mathematical terms. To do this, they provided an explanation of the students' actions. The students answered the researchers' questions in a different way than they did in the test, because they understood the test as belonging to a different instructional context (requiring certain differences). Hence, the researchers explained a difference in behavior with a theory about students' contextualization of the instructional settings. This supports naturalism, since understanding is not methodologically separated from explanation.

In Essay II, I make a more specific methodological claim: there are relevant methodological differences between qualitative and quantitative methods, but these are not sufficiently strong to claim methodological separation.

As I argued, methodological continuity consists in the fact that in both qualitative and quantitative methods observation reports are used, together with theories, to support hypotheses. In addition, Glymour's claim that theories "bootstrap" observations into hypotheses seems to identify a crucial aspect of support,

one that can be used to build a line of continuity between different methodological practices. This is also an instance of methodological naturalism, since it supports the idea that both natural and social sciences rest on testing.

However, the way in which observations support hypotheses through the theory differs in some important respects compared to quantitative methods. The inferences that are used to move from observation to hypothesis in the qualitative case are necessarily non-monotonic, whereas they can be monotonic in the quantitative sciences. Besides, there may be a difference concerning the role of differential structures in support between qualitative and quantitative methods. In Essay II, I argued that the structure of qualitative support is differential, meaning that observation can, at best, make a difference between two theoretically related and competing hypotheses. In contrast, Reiss (2015) has argued that similar differential structures are a feature of *warrant*, that is, of the way in which evidence entails some *change in degree of credence* about an hypothesis. A theory of support does not reject the idea that each supporting/contradicting instance of a hypothesis entails some increase/decrease in credence, but it does not specify it. It sees all increasing/decreasing instances as having the same weight.

These differences are useful for giving more substance to the otherwise vague claim of method pluralism. The diversity of methods consists not only of the fact that methods have different names and look different. Instead, what really differentiates methods is, for instance, that they use different inferences.

Hence, the essence of the methodological naturalism that I propose is that of a thesis of continuity; there are methodological features that work as lines of continuities across the different methods in education, connecting them with the methods of natural science. These lines of continuities are veiled by the rich diversity of methodological practices, but I have argued that they are nonetheless there.

With this general philosophical conclusion at hand, I would like to go back to the controversies discussed at the beginning of this chapter. The first controversy I considered is about the scientific status of educational research. As I discussed in the first two sections of this chapter, there are both internal and external forces in educational research that have claimed and still claim that educational research is not *scientific*. According to the thesis of the external forces, educational research does not satisfy the standard of quality and rigorousness of the natural sciences. According to the thesis of the internal forces, there are different standards and some of these can be radically different. Educational research has its own set of scientific standards, and there are no reasons to expect the different disciplines to have any common standards.

The value of the first two essays is to reveal and assess the claims behind this controversy. In the case of the external criticisms, the contention rests on the confusion between methods and methodology. If the claim is that educational research is not scientific because certain methods are not used (as in the case of the No Child Left Behind Act), then these critical voices should provide a sub-

stantial argument that the methods used in educational research are methodologically distinct from those used in other sciences. Both Essays I and II provide a case against this claim and for the opposite claim: educational science is not methodologically separated from the other sciences.[10] In the case of the internal criticism, the claim made is that the scientific rationality of the methods of educational research is different from the rationality of the methods of the natural sciences. The conclusions of Essays I and II form a case for rejecting this claim. Notwithstanding a plurality of practices there are some marks or symptoms of good science that are common for all sciences. These marks are realized in the practice of testing. Both Essays I and II provide indications that the methods of educational science exemplify this claim.

As soon as the claims behind this controversy are proven wrong, the controversy disappears. The external forces confuse methods and methodology, and the internal forces assume an idea of the natural sciences that is so strong that no science can live up to it. The essays identify these weaknesses and show that once these assumptions are disregarded, the problem of making sense of the continuity between educational science and other sciences in the light of the difference in methods is no longer a problem.

The second set of conclusions concerns the theoretical aspects of educational research. These are drawn in Essays III and IV and they are much more *minimal* and more loosely connected. In Essay III, I conclude that the philosophical standpoint of inferentialism about scientific representation is a useful tool to find the real *locus* of the conflict between two allegedly incompatible theoretical frameworks. This conflict is located in the different concepts of normativity of representation that the two theoretical frameworks seem to assume. Essay IV provides a realist interpretation of Actor-Network theory, claiming that it is consistent with the constructivist assumptions that are associated with this framework.

Both cases make a continuity claim. Essay III investigates where to find a possible point of connection between cognitive and social approaches to learning, whereas Essay IV argues that ontic structural realism can establish a meta-theoretical connection between realism and constructionism about learning.

These are *weak* or *partial* continuities, by which I mean that they only point out some possible or actual points of contact between competing theoretical frameworks. I call them partial or weak because they are by no means enough to

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[10] A clarification is required here. Some advocates of randomized control trials might interpret my words here as claiming that qualitative methods are just the same as RCT's, or that it does not matter what method we use. I do not claim this. Different methods do different jobs, and researchers choose their methods depending on the kind of job they need to get done. If we ask whether RCT is a better method than qualitative methods, I tend to think that the question is wrongly formulated. One method is good for some jobs but not absolutely. So qualitative methods could very well, in certain situations, be a better method than RCT. The comparison and possible integration between qualitative and quantitative methods is an issue that requires further discussion and exceeds the scope of this thesis.

claim that the different theoretical frameworks of educational research, or even only those that focus on learning, form a coherent whole.

If I reconnect this discussion with the controversies I introduced in the first section, the picture is not as clear as in the first two essays. There are not enough grounds to claim that the controversy between social and cognitive theories of learning disappears. I can claim to have found a point of continuity, but the two theories might still be incompatible in many other respects. Furthermore, in the case of the controversy between constructionism and realism, my essay only supports the claim that the two positions are not inconsistent. However, it does not address one major problem concerning Ontic Structural Realism: whether it is a sound interpretation of theories in the social sciences. Lyre (2013) has argued that Ontic Structural Realism is incompatible with the social sciences.

Nevertheless, Essays III and IV point out something relevant: possible lines of continuities in places where typically there is supposed to be a separation, that is, between the social and the cognitive approaches to and between the realist and constructionist ontologies of learning.

## 6.1   What to do, practically, with these conclusions (and the road ahead)

In Section 1, I made a programmatic statement about the relationship between philosophy of science and empirical educational research. Commenting on a criticism made by Phillips against the state of philosophical discussion of empirical research in education, I declared that the aim of this dissertation would not be to formulate prescriptions for empirical researchers. In contrast, the main aim of the whole dissertation has been to learn from the practice of empirical research to find solutions to and new perspectives for methodological and theoretical controversies that have a philosophical content.

Nevertheless, the question remains: What is to be done with the conclusions drawn in the essays and summarized in the previous section? Moreover, even when the aim of philosophy of science is to explicate the rationality of scientific practices, the philosophical conclusions have a normative component that is plausibly unavoidable. Philosophy looks at the ways in which science works, when it works. Therefore, it will always be possible to derive a conclusion such as: This is what was done in this practice that made it work.

Therefore, in this final section I will attempt to formulate some tentative suggestions for researchers that could be derived from the conclusions drawn in the essays.

Beside the main philosophical conclusions about the testability of action claims and about the possibility for causal explanation of actions, I believe that there is an interesting lesson to be learnt from Essay I concerning the difference

between philosophical reconstruction of scientific practices and researchers' philosophical commitments. The case analyzed in this essay showed that the theoretical framework used by the researchers contained interpretivist claims. What I did was to provide arguments that these claims did not represent the actual way in which the researchers in the case interpreted their data.

There is a possible lesson that can be drawn for this essay. Researchers' philosophical commitments might sometimes lack effect on concrete methods. Therefore, I can suggest that researchers should consider carefully what the real empirical import is of the philosophical claims that are sometimes included in theoretical frameworks. It is not unusual to read very broad philosophical statements that are used as frames for empirical social research. Educational research is not an exception. Essay I can be used to suggest that researchers should discuss explicitly what these statements concretely do for the empirical work.

The same can be said about Essay IV. Even though ANT might work in a partially realist framework, it is not unusual to see ANT-oriented researchers making anti-realist claims. Therefore, if these anti-realist claims are introduced on top of an empirical study, we are entitled to know what the consequence are of these claims.

Concerning Essay II, there are two possible suggestions. The first would be that teaching research methods are beset by too many oversimplified or downright false generalizations. One of these is the old idea that qualitative methods can only be used to generate hypotheses, whereas testing is the prerogative of quantitative methods. I think that the conclusions of Essay II indicate that we should stop teaching this inexact picture of qualitative methods.

The second suggestion is a practical one for qualitative researchers. Essay II indicates that a condition for using qualitative data for supporting (or contradicting) hypotheses is the transparency of the theory that is used as a framework. Presenting and discussing in detail the claims of the chosen theoretical framework will enable an external interpreter to map the inferences that go from data to theory and from theory to hypothesis, and to make an informed assessment of the validity of the argumentation presented in the study. Moreover, an explicit statement of the theoretical claims (called in Essay II bridge theories for their ability to bridge observations to hypotheses) provides the external interpreter with a clear picture of the justifications that are needed to assess the validity of the conclusions drawn in the study. For instance, if theory $T$ is used to connect observation $B$ with hypothesis $H$, and if $T$ is stated clearly, for instance as the claim that a subset of observation $B_1$ is the causal consequence of a subset of theoretical constructs $H_1$ included in the hypothesis $H$, then the external interpreter knows that the validity of the conclusion $H$ rests on the reliability of the method of observation used to derive $B_1$ and on the status of $T_1$. Now, if $T$ consists of clearly stated claims, such as the claim $T_1$ that $H_1$ causes $B_1$, the assessment of the validity of $H$ will transparently depend on the empirical status of $T_1$. This assessment will consist in checking whether there is evidence for $T_1$, for

instance in previous studies. To sum up, an explicit formulated theory makes conclusions more open to intersubjective assessment.

Apart from my suggestions for empirical educational researchers, I would like to conclude this chapter by briefly discussing future possible developments and some interesting issues that are left open in this dissertation. I will consider here some examples.

Essay II - This essay leaves a number of unresolved issues that could be used for future investigations. I have already mentioned the lack of a discussion on how qualitative methods can be used to *warrant* claims. This issue concerns the problem of comparing different supporting/contradicting evidential instances. For instance, given that I have a data set that supports a hypothesis and one that contradicts it, how should I regard the hypothesis? Are the sets evidentially equivalent? Answering these questions requires a thorough investigation of what qualitative researchers do in similar situations and an account that is able to make sense of what researchers do. This means that a *good* theory of qualitative warrant should be both descriptively accurate and normatively sound. The former requirement is assumed in order to avoid accounts that are normatively sound but simply prescribe qualitative researchers to modify their practices in a way that fits quantitative methods. The second requirement is assumed in order to avoid accepting what researchers do as justified *just because it is what they do*. An account of qualitative warrant should instead provide arguments that the practice is in some way justified.

Essay III – As discussed in Section 3.3.1, this essay leaves a number of questions unanswered. The main one is whether INF really can incorporate two kinds of normative content without generating a contradiction. More generally, Essay III opens the door for the investigation of learning as a trans-disciplinary concept. This trans-disciplinary character makes learning a perfect candidate for continuing the discussion about the sources of normativity in the social sciences. As the empirical studies of learning suggest, educational phenomena seem to allow different normative descriptions, such as belief-based and practice-based descriptions of the normativity of learning. What does this say about the debate between normativism and naturalism in the social sciences?

Essay IV – This essay proposes a way of interpreting ANT and suggests analogies between two apparently separate approaches regarding the ontology of scientific theories: constructivism and scientific realism. However, this essay only provides arguments for the claim that constructivism and scientific realism (at least a variety of it) are not inconsistent. This means that (assuming that the arguments provided are sound) the best that we can get is that we can interpret ANT within the framework of OSR, but not that we *should* do so. This leaves a big question regarding OSR unanswered: Are there arguments that an OSR interpretation of social theories is preferable to other ontological interpretations? Although OSR has been in the market of ontological theories for at least a decade, the discussion about the application of OSR to social science is still at best scarce (Ross 2008; Kincaid 2008; Lyre 2013).

Summarizing this section, even though the aim of the conclusions drawn in the essays is not to formulate methodological prescriptions, there are some simple suggestions that could be derived from them that might be useful for empirical educational researchers. Furthermore, this dissertation leaves a number of questions unanswered that might be used as starting points for further fruitful investigations.

# Svensk sammanfattning

Empirisk pedagogisk forskning kännetecknas av en rad livligt debatterade metodologiska och teoretiska kontroverser (se nedan). I denna sammanläggningsavhandling i pedagogik använder jag vetenskapsteorins metodologiska redskap i syfte att försöka bidra till denna debatt.

Avhandlingen består av ett inledande kapitel och fyra uppsatser (varav den första, tredje och fjärde är publicerade och den andra är inskickad till tidskrift). I det inledande kapitlet identifierar jag tre metodologiska och teoretiska kontroverser som berör pedagogiken som forskningsområde.

Den första kontroversen berör pedagogikens vetenskapliga status. Denna kontrovers handlar om huruvida pedagogiken är metodologiskt separerad från andra vetenskapliga praktiker och i synnerhet från naturvetenskapen. Å ena sidan finns argument för att en sådan separation inte finns (denna ståndpunkt kallas i avhandlingen för *metodologisk naturalism*). Enligt denna ståndpunkt vilar metoder i ämnet pedagogik som samhällsvetenskaplig disciplin på samma principer som rättfärdigar de metoder som används inom andra vetenskapliga discipliner. Å andra sidan finns de som istället har argumenterat för att separationen finns (denna ståndpunkt kallas i avhandlingen för *metodologisk separatism*). Bland separatisterna finns de som jag kallar för externa kritiker – forskare som ifrågasätter den vetenskapliga kvalitén hos pedagogikens forskningsmetoder. Dessa kritiker har föreslagit att forskningen i pedagogik bör använda sig av de metoder som används inom naturvetenskaplig och medicinsk forskning (ett exempel är randomiserade studier). Inom den metodologiska separatismen finns också interna kritiker, d.v.s. de som har argumenterat för att pedagogikens metodologiska separation inte bara är oundviklig, utan också önskvärd. Pedagogiska fenomen är, enligt dessa röster, sådana att naturvetenskapliga eller medicinvetenskapliga metoder överhuvudtaget inte är tillämpliga i pedagogiska sammanhang.

Den andra kontroversen berör motsättningen mellan *kognitiva* och *sociala* teorier om lärande. Lärande är ett centralt fenomen för pedagogisk forskning. Genom tiderna har forskarna använt olika teoretiska ramverk och metodologiska angreppssätt för att besvara frågor angående människors lärande. I avhandlingen diskuterar jag två teoretiska inriktningar som länge har betraktats som varandras motsats: *konstruktivistiska* respektive *sociokulturella* perspektiv. Dessa perspektiv skiljer sig åt när det gäller deras antaganden om lärandets natur. Konstruktivister betraktar lärande som ett kognitivt fenomen medan sociokulturella forskare hävdar att lärande är socialt och realiseras i mänskliga praktiker. Vissa har argumenterat för att de beskrivningar som sociokulturella teorier for-

mulerar bör reduceras till kognitiva beskrivningar. Andra har istället argumenterat för att kognitiva fenomen är resultat av sociala strukturer. Idag är reduktionismen inte så populär, men problemet kvarstår att vissa ontologiska antaganden, som ligger till grund för de två olika perspektiven om lärande, verkar vara oförenliga.

Den tredje kontroversen berör motsättningen mellan *realistiska* och *konstruktionistiska* teorier om lärande. Här diskuterar jag möjligheten att rekonstruera lärandet utifrån en realistisk ståndpunkt. Med andra ord är grundfrågan: vilken är den mest troliga ontologiska tolkningen av lärandefenomen, så som dessa fenomen beskrivs inom ramen för olika empiriska teorier om lärande? Debattens två extrema positioner har varit realism (empiriska undersökningar om lärande beskriver hur lärande faktiskt sker) och konstruktionism (lärandet finns egentligen inte, utan är endast konstruktioner som uppstår som resultat av mänskliga praktiker). Dessa två tolkningar (eller rekonstruktioner) har sedan debatten uppstod ansetts vara varandras motsatts.

I uppsatserna kritiserar jag antagandena bakom var och en av dessa kontroverser, och argumenterar för en alternativ rekonstruktion av dessa frågor. Närmare bestämt innehåller de fyra uppsatserna:

a) En kritik av separatism och ett argument för metodologisk naturalism i relation till orsaksförklaringar (uppsats I).

b) En kritik av separatism och ett argument för metodologisk naturalism i relation till begreppet "empiriskt stöd" (uppsats II).

c) Ett förslag på en lösning av problemet med den teoretiska motsättningen mellan kognitiva och sociokulturella teorier om lärande; denna lösning vilar på begreppet "vetenskaplig representation" (uppsats III).

d) Ett försvar av en realistisk tolkning av Actor-Network Theory i pedagogisk forskning, som innebär att konstruktionistiska och realistiska tolkningar av fenomenet lärande kan förenas inom ett och samma ontologiska ramverk (uppsats IV).

I det inledande kapitlet sammanfattar jag också de slutsatser som jag drar i uppsatserna och diskuterar hur dessa slutsatser kan ge en teoretisk och praktisk lösning av de tre ovan nämnda metodologiska och teoretiska kontroverserna.

Den första slutsatsen, som jag drar från uppsatserna I och II, är att metodologisk separatism i relation till kausala förklaringar och i relation till empiriskt stöd saknar grund, i såväl konceptuella argumentationer som i forskningspraktiken. De argumentationer för separatismen som har presenterats i litteraturen är bristfälliga och avspeglar inte vad pedagogiska forskare gör i praktiken. Istället argumenterar jag för att metodologisk naturalism verkar ge en mer trovärdig bild av hur den empiriska forskningen inom pedagogik faktiskt fungerar. Jag drar därför slutsatsen att pedagogiken inte är metodologiskt separerad från andra vetenskapliga praktiker.

Den andra slutsatsen berör relationen mellan sociokulturella och konstruktivistiska teorier om lärande. Utifrån de argumentationer som jag presenterar och diskuterar i uppsats III finns det grund för att hävda att representationsbegreppet utgör en möjlig kontinuitetspunkt för de ontologiska antaganden som ligger till grund för olika perspektiv om lärande. I enklare termer hävdar jag att oavsett vad man påstår angående lärandets natur (att det är ett kognitivt eller ett socialt fenomen) innebär lärande nödvändigtvis någon form av representation. I uppsatsen föreslår jag ett sätt att förstå representationsbegreppet som är förenligt med både det konstruktivistiska och det sociokulturella perspektivet. Jag drar slutsatsen att detta sätt att förstå representationsbegreppet utgör en kontinuitetspunkt mellan de två till synes inkompatibla perspektiven.

Den tredje slutsatsen berör den ontologiska tolkningen av lärandefenomen. Jag hävdar i den fjärde uppsatsen att konstruktionism och vetenskaplig realism kan vara förenliga, om man accepterar ett särskilt ontologiskt ramverk, kallat ontologisk strukturrealism, enligt vilket allt som finns är strukturer. Under antagandet av detta ramverk kan man se lärandet som både ett fenomen som uppstår ur våra vetenskapliga och empiriska praktiker (lärande som en produkt av vetenskapen) och samtidigt som en faktisk beskrivning av den empiriska verkligheten.

De här tre slutsatserna har något gemensamt: samtliga identifierar någon form av kontinuitet och argumenterar emot separationen. Den första slutsatsen identifierar kontinuitet mellan pedagogik och andra vetenskapliga discipliner, den andra och den tredje slutsatsen identifierar en intern kontinuitet inom själva pedagogiken. Detta förklarar avhandlingens titel: pedagogiken är ett fält av många kontinuiteter, som vid en första anblick kan vara svåra att se, och som kräver vetenskapsteoretiskt arbete för att bli upptäckta.

*Sammanfattning av uppsatserna.*
Uppsats I: Den här uppsatsen kritiserar en uppfattning om tolkningen av mänskligt handlande. Denna uppfattning, som i uppsatsen kallas för *interpretivism*, postulerar en skarp separation mellan naturvetenskap och samhällsvetenskap, som innebär att metoderna för den senare inte kan tillämpas på den förstnämnda. Jag kritiserar denna ståndpunkt genom att rekonstruera ett fall av pedagogisk forskning. Jag argumenterar för att man utifrån fallet kan hävda att argumenten till stöd för interpretivismen motsägs av vad samhällsvetenskapliga (och i synnerhet pedagogiska) forskare faktiskt kan uppnå. Jag drar slutsatsen att interpretivismens påståenden saknar stöd och att den allmänna tes om en metodologisk separation mellan samhälls- och naturvetenskapen förefaller som problematisk.

Uppsats II: I denna uppsats diskuterar jag begreppet *empiriskt stöd* i relation till kvalitativa metoder (kortfattat: kvalitativt stöd). Artikelns huvudsyfte är att föreslå en konceptualisering, eller en teori, för empiriskt stöd som är specifik för kvalitativa metoder. Den här teorin bygger på en analys av ett fall av kvalitativ forskning i musikdidaktik. Jag ger argument för att teorin lyckas identifiera och

fånga specifika och icke-triviala egenskaper hos kvalitativt stöd. Samtidigt argumenterar jag för att teorin stödjer påståendet att det inte finns någon metodologisk separation mellan kvantitativa och kvalitativa metoder rörande empiriskt stöd.

Uppsats III: I denna uppsats undersöker jag tre exempel på filosofiska teorier om *vetenskaplig representation*, i syfte att bedöma vilken av dessa som är en bra kandidat för en filosofisk teori om vetenskaplig representation inom lärande. Lärandeprocesser innebär intuitivt att individer använder konkreta artefakter som tecken på och för lärande. Dessa artefakter är representationer och artikeln diskuterar hur man bäst kan förstå representationsbegreppet i relation till lärandet.

De tre teorier som diskuteras i artikeln är Gieres *intentionalism* (som fokuserar på hur individer använder representationer för att uppfylla olika mål), Suarez *inferentialism* (som fokuserar på hur representationer används för att dra slutsatser) och Lynch och Woolgars *sociologiska teori om representationer* (som fokuserar på sociala normers konsekvenser för representationer). För att kunna bedöma vilken som är den lämpligaste teorin om representation inom lärande, jämför jag de tre teorierna med två aspekter av representation inom vetenskapligt lärande som uppstår från den empiriska forskningen om lärande. Jag kallar dessa aspekter den *intentionella* och den *normativa* karaktären hos representation inom vetenskapligt lärande. Jag hävdar att inferentialismen har kapacitet att fånga dem båda på ett enhetligt sätt. Således drar jag slutsatsen att inferentialismen verkar vara en lovande filosofisk teori om representation inom vetenskapligt lärande.

Uppsats IV: I denna uppsats diskuterar jag aktörsbegreppets ontologiska status. Begreppet "aktör" betraktas i denna artikel inom ramen för Actor Network Theory (ANT) och förstås som grundläggande beståndsdel i ett nätverk. Jag presenterar två problem som berör aktörsbegreppet som Collin har presenterat och diskuterat. Det första problemet rör aktörsbegreppets förklaringskraft. Enligt Collin kan aktörer inte förklara uppkomsten av ett nätverk och samtidigt vara produkter av samma nätverk. Det andra problemet är att om aktörer är egenskapslösa entiteter, vilket har föreslagits av Latour, så är det oklart hur de kan kombineras i olika nätverk.

Jag föreslår att båda problemen vilar på antagandet om en form av objektontologi, d.v.s. antagandet att verkligheten består i diskreta enheter som har inneboende egenskaper. Jag hävdar att dessa problem kan lösas genom antagandet av en relationell ontologi, enligt vilken verkligheten endast består av relationer. En sådan ontologi föreslås inom ramen för ontologisk strukturrealism (OSR). Ontologisk strukturrealism är en teori om vetenskapens ontologi enligt vilken vetenskapliga teorier representerar en verklighet som består av endast relationer, och inga diskreta enheter. Därför argumenterar jag för att OSR kan lösa båda de problem som Collin tillskriver ANT.

I artikeln söker jag stöd för mina påståenden genom att analysera exempel på tillämpning av ANT inom pedagogik, och i synnerhet inom området som kallas för networked learning (NL). NL är ett paraplybegrepp som används för att beskriva alla lärandesituationer där lärande uppstår i en relation mellan olika individer och digitala redskap. Jag hävdar att komplexiteten hos fenomenet NL ger oss en lämplig utsiktspunkt från vilken vi tydligt kan förstå många viktiga aspekter av både ANT och OSR.

Utifrån mina exempel argumenterar jag för att ANT och OSR visar ett antal tidigare okända beröringspunkter. Detta trots att de två teorierna vilar på olika och vid en första anblick inkompatibla antaganden. OSR är en form av vetenskaplig realism, som hävdar att vetenskapen representerar verkligheten så som den är (åtminstone till en viss mån). ANT kommer istället från en stark antirealistisk och konstruktionistisk tradition, enligt vilken de entiteter som vetenskapen använder för att beskriva världen inte representerar verkligheten så som den är utan istället är resultatet av sociala konstruktioner.

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