Preface

I wrote this book three years after The truth regimes of the past, which was a detailed enquiry into the epistemic and political foundations of the history and historiography of geography. One of the arguments developed there was in the direction of a radical rethinking of the ways in which we excavate the presuppositions that underwrite geographical thought and practice. It was, if you want, an invitation to being more critical about the ways in which we think we are critical. At that stage in my career I took much pleasure in criticising. Providing devastating critiques of the state of affairs in geography was my unacknowledged goal. French philosopher Michel Serres made the observation that creation is divine and destruction is devilish, thus reminding us of the late work of Sigmund Freud on the fight between Eros and Thanatos. After much personal reflection in the last three years, I have become acutely aware of the importance of building something, instead of merely criticising. This book is the trace of that shift in my thinking.

Those who know me know that I love theory and that I believe in the political power of theory. My educational and professional background (Romania, England, Norway, Canada) exposed me to an array of theoretical fashions, which forced me to become very careful about the fine details of the inner architecture of theoretical construction and about the infinite shades of grey various theoretical projects encompass. This care also enhanced my awareness about why exactly I endorse a certain position and not the
position closely related to it. The net result is that I distilled my own way of understanding the relation between epistemology, ontology, and politics, and the best name I found for labelling that way is pragmatic scepticism. Throughout the book, I stay away from the temptation to give a dictionary-like definition of this philosophy. I do not even like to think of it as a philosophy. Instead, I see pragmatic scepticism as a way of being and as a way of relating. A way of being human, i.e. enmeshed in language and limited by our senses; and a way of relating, i.e. crafted by the happy and sad encounters with theories, things, and life’s happenings.

The book is structured in three parts, which together give a sense of the potentials of pragmatic scepticism: as a way of thinking about the world, as a way of approaching theoretical dilemmas, as a way of mapping one’s inner contradictions. The first part of the book introduces pragmatic scepticism in relation to the general questions underwriting the philosophy of knowledge and the study of science. The second part is more specific in that it deploys the pragmatic sceptical attitude to the central metatheoretical questions of the discipline of geography. The last part of the book groups under the heading ‘philosophies of struggle’ two more applied essays on the political economy and the political epistemology of conflicts over knowledge in the globalised landscape of higher education in general and geography in particular.
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* The text of the final version of the book has its starting-point in several earlier sources: A Structural Theory of Geography
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I. Pragmatic Scepticism
&
Philosophies of Knowledge
Dragoş Şimandan
I. Pragmatic Scepticism & Philosophies of Knowledge

The closely related aims of the first part are: (a) to unsettle conventional modes of thinking the relation between epistemology, ontology, and politics, and (b) to introduce the basic tenets and metaphors of pragmatic scepticism.

For the purposes of this book, 'epistemology' and 'ontology' do not signify arcane definitions of particular branches of philosophy, but are conceived more broadly. Thus, 'ontology' stands for the 'world out-there', whereas 'epistemology' refers to the production of scientific knowledge and to the (im)possibility to know the 'world out-there'.

This first part unfolds in five chapters. The first chapter presents an overview of the major epistemic parishes in contemporary philosophy of knowledge and social science. That presentation is used to argue, in the second chapter, against the idea of apriori epistemic privilege, by taking the case of feminist standpoint theory. Whilst these first two chapters hint at the key tenets of pragmatic scepticism, their full development is presented in the remaining three chapters.

Thus, chapter three explains why we need to blur the distinction between inside and outside and to fully subscribe to a relational style of thinking. The support for this metatheoretical intervention is a concept central to all brands of geography: the concept of scale. Throughout the chapter we theorise anew the relation between one's unit of analysis and its context by means of a new definition of 'scale' as contexts collapsed within the unit of analysis. Chapter four
explains pragmatic scepticism as a simple epistemology that claims to fill the space between overly optimistic epistemologies (positivism, critical realism, Marxism) and overly pessimistic epistemologies (Rortyan neopragmatism, poststructuralism). It culminates by melting the contributions of the previous chapters into the production of the metaphor of ‘recursive cartographies’, which, it is argued, best captures the essence of pragmatic scepticism. The last chapter of the first part applies pragmatic scepticism to a novel analysis of the production of scientific knowledge, analysis that fully incorporates the mundane dimensions of this production. The more realistic view of science resulting from this chapter then opens the way to an analysis of the discipline of geography in the second part of the book.
1. Epistemic Parishes

We cannot, I think, imagine a moment at which the human race could settle back and say, “Well, now that we’ve finally arrived at the Truth we can relax” … On the contrary, we should relish the thought that the sciences as well as the arts will ALWAYS provide a spectacle of fierce competition between alternative theories, movements, and schools. (Richard Rorty, 1987, page 45)

In this chapter I propose that although classifications are necessarily guilty of ‘epistemic violence’ (Spivak, 1999) it might be useful to risk the reframing of the saga of the interpretation of science alongside three categories of approaches: firstly, ‘stubborn approaches’ which include perspectives on science either overly internalist (e.g. the Vienna Circle, Popper) or overly externalist (e.g. SSK), secondly, ‘accommodating approaches’ that try to find acceptable third-ways between internalism and externalism (e.g. critical realism, Kuhn) and thirdly, ‘smart approaches’ that repudiate all the vocabulary of classic epistemology and re-place the whole debate over science into novel discursive shapes (e.g. Heidegger, Latour). These perspectives are not ordered chronologically or qualitatively. Instead, they co-exist as epistemic parishes and animate the current debates while having repercussions in the political arena (e.g. ‘the Science Wars’). We defend the view that all theories and paradigms should be seen in the light of pragmatic scepticism, as configurations of epistemic gains and epistemic losses. In plain English, it is rare the case that a paradigm is better than one of its competitors in all respects. In the specific case of our argument regarding the three aforementioned approaches to science, we suggest that
there is a need for the co-existence of all these approaches, for keeping the conversation about the production of scientific knowledge alive and generative of fresh insight. To add even more challenge to this theoretical competition, we end our paper by reminding the readers that the whole business of explaining science is - from certain points of view - vicious and misleading. The lines of thought we enlist for this reminder are those of Latour, Foucault and Wittgenstein.

**Classifying the interpretation of science**

*‘Stubborn’ approaches*

The extremes of the complex spectrum of attitudes towards science are represented by ‘internalists’, who claim that although science is produced by humans, it cannot be explained in mere social terms, as it has something unique or special, a ‘mechanism’ of separation from trivial social logic and of creation of its own autonomous regime; and by ‘externalists’, who argue that since science is produced by humans it can be completely understood in social terms, with the tools of sociology and anthropology.

*Internalists* dictated the discourses on science in the first half of the 20th century, mainly through the philosophy of the Vienna Circle (Carnap, 1995, Schlick, Mach, Neurath, and partly, Ayer and Reichenbach) also called the standard model of science, as it furthered in sophisticated ways the scientific method established, among others, by Descartes in ‘Discourse on Method’ (1637 / 1983). His first rule was to start research by clearing one’s mind of prejudices and unwarranted assumptions; the second - divide the object of research in manageable units (1983, p. 100); the third to recombine the analytical findings into a coherent whole; and
the fourth to check all the process again, in order to detect omissions and false findings. The standard model acknowledged that science is produced by humans and its purpose was precisely to overcome this mundane shortcoming. The proposed solution was to reduce natural language to formal language (logic and mathematics), which was supposed to have a higher epistemic credibility, due to its being a closed, self-referential, axiomatic system. Once a finding was ‘reduced’, it entered the repository of ‘true statements’, the growth of which would finally lead to the knowledge of The Truth of the world.

The standard model was turned upside-down by Karl Popper (1934 / 1981) who advanced negative rationalism. The latter vision was grounded on the demonstration that the only valid logical mode is that of deduction, induction -on which the standard model was built- being less credible. In so doing, Popper replaced foundationalist epistemology with faillibilist epistemology and verificationism with falsificationism. Although unequivocally internalist (and see Popper, 1963, 1997, 1998) Popper’s view of science was that of an ‘open society’, in which all findings are provisional, open to possible refutation.

There have been other overwhelmingly internalist theories of science (e.g. Lakatos’s conception of ‘research programmes’, Lakatos, 1978; the Munchen School of Sneed, 1976 and Stegmuller; Toulmin’s ‘evolutionist’ theory, 1972; Newton-Smith’s ‘temperate rationalism’, 1994, etc.) but the hallmarks of internalist philosophies of science remain the standard model and Popper’s negative rationalism.

Externalists: the rather comfortable image of science as endeavour to represent as accurately as possible the world out-there (cf. Rorty 1979, 1987, 1995 - a representationalist regime of truth) started to be unsettled by the work of the
historian of physics Pierre Duhem (1907) taken later into account in W. V. Quine’s twenty - page devastating critique of the empiricism on which both verificationism and falsificationism relied (‘Two dogmas of empiricism’, Quine, 1951 / 1998). While Quine built a strong case for the thesis of the underdetermination of theories by observational data, seven years later, N. Hanson (Hanson, 1958) pushed the stake even further, suggesting that observational data are unavoidably theory - laden.

At a broader scale, late Wittgenstein and then structuralist and poststructuralist philosophers argued for the self- referentiality of language systems, for the lack of ‘natural’ correspondence between words and the world out - there; Foucault (1970, etc.) argued for the inextricable link between power and knowledge; Clifford Geertz (1973) proposed an innovative programme in anthropology; the work of Mannheim and later that of Merton challenged the hegemony of the traditional concern with the working of good science only, etc. But above all these theoretical shifts stands Thomas Kuhn’s ‘The Structure of Scientific Revolutions’ (1962) which prepared the ground for a major challenge to the philosophy of science altogether - that represented by the ‘Strong Programme’ (for an overview see Hess, 1997, ch. 4; Golinski, 1998, ch. 1; Couvalis, 1999, ch. 6). Agassi’s recent reflections (Agassi, 2002) on Kuhn’s autobiographical work (Kuhn, 2000) nicely capture Kuhn’s position within the canvas of the competing philosophies of science of Michael Polanyi and Karl Popper:

Michael Polanyi offered a traditionalist view that was further from positivism than that of Popper. He defended science and religion on a par – as traditions. Kuhn offered an austere version of Polanyi’s views, offering no theory of tradition and nothing at all on religion. The positivists could come to terms
At this point, the discussion becomes more complicated, because of the different meanings ascribed to terms such as externalism, relativism, constructivism, and philosophy of science (see the discussion in Hess, 1997, pp. 32 - 39). Under the heading ‘externalists’, we group all those epistemologists who consider that science can be explained solely and fully by social science. For these scholars the very distinction internalism - externalism is dubious, as it lets room for believing that there is some ‘internal’ logic of science, safely isolated from wider social processes. Nevertheless, we need to direct attention to the circumstance that the internalist - externalist divide is not coextensive with the distinction advanced by Pickering (Pickering, 1992) between studying science as knowledge and studying science as practice and culture. Most mainstream philosophers, but not all, are interested in science-as-knowledge only, even if they are not internalists. This state of affairs should be placed in the context of the disciplinary identity of philosophy, traditionally understood as a normative endeavour meant to guide humans according to the rules of rationality. The exceptions from this disciplinary tendency have been rather few (Polanyi, 1958; Kuhn 1962 / 1976, 1982, etc.).

Of sharp relevance for this moment in my argument is the work of those externalists with an educational background other than philosophy (sociologists, anthropologists, etc.). To begin with, they often argue that science could and need be studied in the same way as any social group. Thus, Geertz had no hesitations to compare scientific communities with tribes, idea well exploited later on, in the work of Becher and Trowler, 2001. Perhaps the most ‘stubborn’ positions
within externalism have been adopted by the Sociology of Scientific Knowledge School (SSK or ‘the strong programme’) which started with the work of Barry Barnes (Barnes, 1974) and David Bloor (Bloor, 1976 - the exemplar - ‘Knowledge and Social Imagery’) who have formed the Edinburgh School (including Mackenzie and Shapin, 1998); as well as with the work of Collins, main representative of the Bath School. While Bloor (1976) identifies four tenets for their approach - causality, impartiality, symmetry, and reflexivity - the paramount claim of the strong programme has been that it is the pursuit of various human interests that ultimately explains science, albeit those interests are hidden by rhetoric and the commonly accepted external image of science. The significant number of applied investigations and case studies undertaken within the SSK’s theoretical frameworks has revealed these hidden mundane interests, and thus disenchanted science, with all the political implications of this reality check.

Externalists have followed different methodological strategies: the Edinburgh School privileged the macrosocial approach; H. Collins preferred micro-social analyses (a third school ‘appeared’ at York, with Mulkay, 1991); Woolgar and Latour (1979) chose the tools of anthropology (the same was the choice of Knorr-Cetina, 1981); Garfinkel and Lynch developed an ethnomethodology of science1, whereas a more recent ‘fashion’ (criticised by Collins and Yearley, 1992a, 1992b, as an ‘epistemological chicken’ game) has been to exploit with innovative methods and styles the fourth tenet of SSK - that of reflexivity (and see Woolgar, 1992, Ashmore, 1989, Mulkay, 1991, Pinch and Pinch, etc.).

1 E.g. Lynch, 1992a, 1992b, argued that it is a departure from the parochial ‘sociologism’ of SSK, whilst Bloor, 1992, responded that what is good in ethnomethodology comes from SSK…
Nowadays it can be claimed that externalists represent a complex set of positions: they would agree we could study science in the same ways we would a primitive society, but they disagree over what exactly is the best way for researching it. More recently, the foci of research have also diversified, from the initial concern with laboratories - fabrication of substances, quarks, microbes, etc. - to increased attention paid to fieldwork and ‘softer’ sciences.

‘Accommodating’ approaches
Most approaches to science are situated between the pole represented by the internalist philosophy of science-as-knowledge and the pole represented by the social study of science-as-practice-and-culture. They consist of a range of third ways, which try to overcome the shortcomings of both extremes, whilst retaining their strengths. The ratio internalism - externalism varies in radically significant ways from Kuhn (Kuhn, 1962, 1982, 2000) to the Starnberg School\(^2\), critical realism (Bhaskar, 1978, Sayer, 1992), contextual feminism (Longino, 1990), feminist standpoint theory (Harding, 1987, 1991, 1998), the theory of situated knowledge (Haraway, 1991; but see a radicalised argument in Haraway, 2000), perspectival realism (Fricker, 2000), pragmatic realism (late Pickering, 1995), constructivist / modest realism (Galison, 1995, Nanda, 1997; see the discussion in Bassett, 1999), social epistemology (Fuller, 1988, Radder, Resnick), etc. Although most of these approaches propose some new conceptual tools (e.g. paradigm, standpoint, practical adequacy, situated knowledge), their main aim is to advance a solution using the existing overarching frameworks of the two

\(^2\) The Starnberg School was inspired by Heisenberg’s model of ‘closed’ theories and by philosopher von Weitszaker; the main proponent is G. Bohme; cf. Parvu, 1999.
aforementioned poles: content versus context of science; truth; objectivity; rationality; representation; progress; adequacy; interests; bias; subject-object dichotomy. Even though these terms are differently defined and explained, they are nevertheless the signifiers used in theory-building and argumentation.

Before making the next classificatory move, we find it appropriate to pause for a warning about the consequences of the massive success within Anglo-American social science of Donna Haraway’s theory of situated knowledge. Some invoke her name as the ultimate wisdom in recent epistemology, overlooking the theoretical compromises required if one subscribes to her views. Judith Butler’s seminal work (Butler, 1997) exposes the negative entanglements of the rhetoric of epistemic situatedness:

To Butler, it is no answer to propose that the subject is ‘situated’ in the relations it claims to know and to criticize. Insofar as theorists of the situated subject accord it a vantage point from which to ‘preside over the positions that have constituted [it]’, they resurrect the fantasy of autonomy. Butler counters that critical vantage is an impossible fantasy; for subjectivity is not a location but a ‘transfer point’ of attachments, dependencies, and losses without which no subject can emerge but that ‘no subject, in the course of its transformation, can ever afford fully to “see”. (Disch, 1999, page 551)

Butler’s remarks seem compelling to us, and so does the theoretical proposition through which she completes the constructive requirement expected of all robust social theory. Thus,

Butler counters the vantage-point metaphor with a figure of speech that is often used to characterize the inauguration of subjectivity, the ‘figure of turning, a turning back upon oneself’ or even a turning on oneself’…The significance of this
metaphor to Butler is that it exemplifies how reflexivity, the capacity that we take to be the condition of autonomous subjectivity, is of a piece with internalization, the process by which we subjugate ourselves to the authorities (persons, norms, principles) on whom we depend for recognition. We should understand this ‘turn’ not as an activity of the subject (for if turning inaugurates the subject then there is no subject to make the turn) but as a ‘trope’ by which we not only conceive of generation but perform it: we ‘turn’ a phrase that enables us to speak about the subject’s generation with due respect for its twofold relationship to power. (Disch, 1999, page 552)

‘Smart’ approaches

‘Smart’ approaches share the dissatisfaction with the established frameworks for thinking about the production of scientific knowledge, noting that the linguistic setting up of a problem creates much of the problem. The solution then, is to reject the frameworks discussed so far and to bring instead a novel vocabulary within which the former problem simply does not exist. Martin Heidegger and Bruno Latour are two of the most prominent figures who have taken this path.

Martin Heidegger subordinates science to the broader issue of practical human knowledge, which in turn is seen as a matter of performance and immersion in the lifeworld, and not as a question of representation and abstraction (aboveness - distance from out-thereness). For him, the epistemic agent is a Da-Sein (Da = there - ness; Sein = the domain of Being), and the object researched belongs to Being (Heelan, 1998). This view is clearly directed against Cartesian epistemology that posits a separation between object (the world out-there) and subject (the epistemic agent) with the consequent dramatising over the rules of the implied representationalist
regime of truth. In Heidegger’s philosophical system, this separation is taken to be the essence of the modern age:

The expressions “world picture of the modern age” and “modern world picture” …assume something that never could have been before, namely, a medieval world picture and an ancient world picture. The world picture does not change from an earlier medieval one into a modern one, but rather the fact that the world becomes picture at all is what distinguishes the essence of the modern age. (Heidegger, 1977, page 130)

Some of the reasons why Heidegger’s manoeuvre dislocates previous framings of scientific knowledge and why these dislocations would be socially welcome are suggested by Rosalyn Deutsche, who draws on feminist theory and psychoanalysis to propose that:

This [modern scientific] imagination stages the world –as –exhibition and, at the same time, is fabricated by the picture it creates. But it is also constituted by disavowing its dependence on the image. Adopting an objectifying epistemology that endows objects of study with an independent existence, the [scientific] imagination puts aside the discourse–object and subject–object relationship and thereby separates itself from the picture. The subject’s purported ability to see the world as a whole is an effect, then, of not seeing the continuity between itself and the visual field, or, put differently, of disclaiming its non-continuity with itself…the world-as-exhibition has been inhabited from its inception by a radical uncertainty, an instability produced by the image’s constitutive exclusions. These exclusions betray the incompleteness of every meaning and position, making the perception of a comprehensive space inseparable from a sense of what threatens that space. (Deutsche, 1995, page 171)

Heidegger maintains that we are immersed in our projects and in their doing we open up horizons and unavoidably
close off other horizons, which explains why systematic ignorance is always produced alongside systematic knowledge (idea taken further by Haraway, 1991; see also Spivak’s concept of ‘epistemic violence’, Spivak, 1999). The so-called ‘things’ and ‘facts’ are the result of our setting of boundaries (Heidegger, 1996). Our experience (Vorgriff) of the manifestation of those things is only later represented, elaborated in theories (Vorsichst) (cf. Heelan, 1998, page 280). ‘Truth’ is ultimately ‘our’ truth and becomes a matter of mystery, of revelation, particularly when openings of horizons of various projects interact or even ‘clash’ (Tanesini, 1999). Gayatri Chakravorty Spivak (1999), one of the exegetes of Derrida’s work, admits that the French theorist’s deconstruction system owes much to Heidegger’s reworking of the relation between epistemology and ontology:

...After the famous turn or Kehre of the thirties, Heidegger betrayed his insistence that, at the start of all investigative questioning was a prior question that could not be adequately answered...Derrida described a movement in his own work as well. It was a turn from ‘guarding the question’ - insisting on the priority of an unanswerable question, the question of difference – to a ‘call to the wholly other’ – that which must be differed – deferred so that we can posit ourselves, as it were. (Spivak, 1999, page 425)

Much less romantic than Heidegger, Bruno Latour (1987, 1993, 1999, 2000) and his collaborators (Calhoun, Law) propose an actor / actant – network theory which is an intended ‘amodern’ approach to science (drawing on the work of Serres, Deleuze, Foucault; cf. Latour, 1993). Latour’s challenging description of science is indebted especially to French philosopher Michel Serres, one of the earliest thinkers who addressed thoroughly the need to
produce new discursive frameworks for an effective overcoming of the internalism - externalism polarisation in 20th century epistemology:

What Serres offers …is a distinctive approach to the history of science. This is not an ‘internalist’ history, since it recognizes the significance and influence of events outside the scientific domain upon the development of a given science. But neither is it an ‘externalist’ history, such is the power accorded to the concepts and methods generated by a particular scientific practice, which…radiate throughout the entire social field. (Brown, 2002, page 9)

ANT proposes that there is no ‘society’ and no ‘nature’: they are produced by the modern project, built on processes of purification that are necessarily accompanied by hybridisation. What we have instead is a continuous reality made of objects, quasi-objects, and above all, of interactions. Those connections that are better articulated extend in space and time, developing into networks of heterogeneous associations. The power of a network stands in the good circulation and coordination among its components. Science is just one of the most successful networks, which works through effective loops and processes of mobilisation of the world, autonomisation, alliances, public representations, links and knots (Latour, 1999). Latour does not abandon the notions of reality and truth, but truth is not a matter of correspondence, but of ‘circulating reference’, of a curious chain of incessant interplay of things with words. The actants of science are not only scientists, but also the objects (some of them ‘immutable mobiles’) they manipulate (computers, desks, letters, etc.) or create (e.g. the ‘factish’; the laboratory - as a ‘centre of calculation’). Networks are themselves part of larger networks and their interaction with the ‘outside’ is so important that clear-cut boundaries cannot
be traced. Latour argues that dichotomies such as local -
 global, microsociology - macrosociology, culture - nature,
agent - process, and inside - outside have to be replaced with
a fresh and more realistic vocabulary. Furthermore, he
advocates close attention to the role of objects in the
production of science:

There is a social sociology but where is the physical
sociology? ...I propose rather that S[tudies of Science and
Technology] should be to sociology this other part which
keeps the discipline 'on its toes', which forces colleagues
immersed in the 'social' and the 'symbolic' to take seriously
the enormous difficulty of accounting for objects, which
oblige them to take up the radical hybridity of their
topics... (Latour, 2000, page 121)

Inherent imperfection as ground for a non-zero-sum
game

Any epistemic gain presupposes an epistemic loss
(Heidegger, Haraway, Spivak). The three types of
approaches presented above offer unique configurations of
gains and losses, without offering the prospect of
simplification, of not needing any more one of them. Such a
prospect would presuppose the existence of a 100% superior
approach (i.e. its gains-losses configuration would either
retain all the gains of a previous, inferior approach, whilst
reducing its number of losses; or increase the number of
gains, but preserving all the gains of the inferior approach
and keeping identical-or diminishing! - the number of losses
of the inferior approach; in all other cases, at least a small
- but perhaps crucial - part of the epistemic privilege of an
approach would be lost). The set of brief critiques that
follow supports this theoretical position.
We do not endorse the internalist philosophy of science, given that it has been compellingly criticised not only by representatives of various paradigms within social science, but also in its own terms, within the same framework - that of philosophy. Therefore, the convenient argument of incommensurability cannot be easily raised in its favour. Apart from theoretical histories, another issue we considered is that of plausibility: since science is made by humans, to claim that it does not have an overwhelming mundane dimension, and that all that really matters is its ‘inner’ logic (which posits it ‘above’ mere social research) is a too naïve, unconvincing position.

Although much less naïve, we do not completely endorse the SSK either, for it has the same problem – underdescription of science - but from the other end of the spectrum. The historical record of the successes of science, its dimensions and solidity, suggests that even if scientific communities might be compared and analysed in the same way as any ‘primitive’ society, they are however a special type of society. It is therefore: a) not sufficient, b) less interesting, and c) less fruitful to focus only on what science has in common with tribes. The focus on what makes it a special tribe is, we believe, more important, without saying that the other concern should be marginalized or abandoned. And we have no doubts that there is a connection between the answers to this question and the type of concern traditional philosophy of science has had. More to the point, the world of ideas scientists create co-produces in its turn scientists (e.g. the education of a scientist through ‘neural programming’ (Kuhn); the emergence of tacit knowledge (Polanyi); the ‘stamp’ of alternative gazes at the world: a geographer is obsessed by space, a psychologist by the human psyche, etc). A related point in this regard has recently been formulated by Barth (Barth, 2000) who was
interested in delineating knowledge from culture and thus in revisiting the Geertz-style (Geertz, 1973, 1976, 1980, 1983) anthropology of science:

...Knowledge always has three faces: a substantive corpus of assertions, a range of media of representations, and a social organization...Clifford Geertz...commented that my view of knowledge and its role in human life did not seem to distinguish it much from what anthropologists have been calling 'culture'. Indeed, it does focus on many of the same data and seeks to analyse many of the same phenomena. But in calling it knowledge rather than culture I think that we ethnographers will analyse it differently and find ourselves disaggregating our received category of culture in distinctive ways that hinge on what our ideas of 'knowledge' evoke. Knowledge provides people with materials for reflection and premises for action, whereas 'culture' too readily comes to embrace also those reflections and those actions. Furthermore, actions become knowledge to others only after the fact. (Barth, 2002, page 1)

These reservations notwithstanding, there is a problem with the SSK idea that scientists are driven by their interests, connected with broader social interests. First, one might speak of disinterested interest: sometimes a scientist might just be curious to find out something. Should we believe in pure curiosity, non-linked with broader social interests though? Spivak would answer 'no', as interests are irreducibly textual. Second, we are reluctant of those who establish a tenet (e.g. the claim that scientists are driven solely by interests) and then try to prove through empirical research that their idea is correct. In these cases, scientists often fall in the epistemic fallacy of arranging the facts to fit the model, instead of arranging the model to fit the facts (old-style Marxists see class conflict everywhere, etc.).
At a more general level, both of the ‘stubborn’ approaches seem ‘scientific’, established, coherent, but the admission of one presupposes the dismissal of the other. Given that appearance of trustworthiness, one might start to question whether the fact that both approaches seem plausible in their own terms is not due to the complexity of the object researched (here ‘science’), which allows plural interpretations. Two paths could be taken: first, to assume\(^3\) that the truth about science is somewhere between the stubborn positions, and that ‘stubbornness’ (read ‘narrowness’) is the effect of disciplinary parochialism (Sayer, 2000, Hofer, 2000), which opens only one horizon (the disciplinary horizon), to close all the others. Second, perhaps the first explanation is too sociological and the real explanation is ontological: reality, including the reality of science, is perhaps too complex to be fully apprehended by the minds of humans. Therefore, we should not hope to find The Absolute Truth, but only ‘human’ truths. From Heidegger to Rorty and postmodernists, the idea that the ‘cornered’ nature of any human knowledge cannot be escaped remains strongly endorsed.

With regard to accommodating approaches, we have four overall criticisms to make: first, they usually rely on the common sense belief that the truth is in the ‘middle’, between too extreme positions. However, it has frequently been the case in the history of science that the truth was counter-intuitive, non-common sense (e.g. the rotation of the Earth around the Sun despite the appearance of the moving Sun on the sky). Second, sometimes accommodating perspectives can be suspected to be mere rhetorical revamping of a ‘stubborn’ perspective, in order to make it

\(^3\) Assumption made by those schools of thought discussed under the heading ‘accommodating approaches’.
more acceptable (e.g. critical realism). Third, we should not forget that the two ‘stubborn’ approaches are mutually exclusive. We doubt the possibility of a perfect bridging of them into a coherent third way, and the partisans of ‘stubborn’ approaches share our doubt. Lastly, accommodating scientists strive only to change the ‘dance’ of the debate, whilst preserving its tune. In a sense, they are not ‘smart’ enough to reframe the debate by means of a radically new vocabulary. Because of this traditionalism, they might overlook other influences (e.g. they could try to accommodate the ‘world of ideas’ with the social dimension of science, ignoring the peculiar grammars of space, place, and scale, etc.).

As far as the ‘smart’ approaches (Heidegger, Latour) are concerned, two issues trouble us: first, they tend to be non-practical and unmanageable because of the too sophisticated and obscure conceptual framework. If we apply the well-established Ockham’s Razor criterion (simplicity) to the evaluation of theories, they fail when compared to the non-smart approaches. Second, they claim to solve older problems in epistemology, when in actuality these problems re-enter through the back door. For what Heidegger preaches is an original philosophy of knowledge, yet a philosophical approach, and what Latour preaches is an original sociology of science, yet a sociological approach. Their work transcends some limitations, but remains limited in its potential of transcendence...They just play skilfully with the tools they know best: those of philosophy (Heidegger), and those of sociology (Latour), but they remain captives within the horizons of research of philosophy and sociology.

I am a pragmatic. This means that I posit fruitfulness as the ultimate criterion in the evaluation of competing knowledge claims. It also means the enthusiastic subscription to the
crucial statement that we cannot know whether we can or we cannot know the truth. Engaged for a number of years in exploring epistemological and ontological controversies⁴, I cannot but conclude that we need the contributions of all these epistemic parishes (stubborn, accommodating, and smart) precisely because each of them is condemned to remain ‘imperfect’ (they offer the epistemic privilege of their uniqueness, but also the unavoidable epistemic frustration of not ‘seeing’ all what others can ‘see’ from their positions), and none of them is⁵ superior [enough] to completely solve the gains - losses configuration of another approach, and thus to replace it without any epistemic loss. We need ‘stubborn’ positions because radical philosophical standpoints help both as catalysts for hybridisation, and as referents in the endeavour of comparing in-between and ‘smart’ positions. We need accommodating approaches, even for the very fact that it is useful for scientists to believe that less cornered positions are attainable. This implies the ironic, ‘as-if’ subscription to commensurability and representational truth. Indeed, as a pragmatic sceptic, I deploy Rorty’s fruitfulness criterion, to argue, against Rorty, that representational truth can be useful sometimes, as long as its hegemony is undermined⁶.

Finally, we need ‘smart’ approaches for their potential to ‘fluidify solidified thinking’ (Malabou), to open new

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⁴ E.g. we suspect that philosophy of science has been the main responsible in creating modern science as a special ‘tribe’: in claiming to explain science, it has actually shaped it, through materialist semiotics.

⁵ So far; the future is open.

⁶ It is the belief that truth-as-representation is the only type of truth or the best type of truth that is unfruitful. Pragmatic Scepticism proposes that it is fruitful to have a healthy competition among different types of truth, thus reducing truth-as-representation to being just one other type of truth, with its attendant illusions and limitations.
horizons, to challenge other epistemic parishes in the direction of thinking in post-parochial ways.

* In this chapter we have proposed that even though classifications are necessarily guilty of ‘epistemic violence’ (Spivak, 1999), it might be useful to risk the didactic ordering of the schools of thought in epistemology and the philosophy of science alongside three categories of approaches: firstly, ‘stubborn approaches’, which include perspectives on science either overly internalist (e.g. the Vienna Circle, Popper) or overly externalist (e.g. SSK); secondly, ‘accommodating approaches’ that try to find acceptable third-ways between internalism and externalism (e.g. critical realism, Kuhn) and thirdly, ‘smart approaches’ that repudiate all the vocabulary of classic epistemology and re-place the whole debate over science into novel discursive shapes (e.g. Heidegger, Latour). To be sure, these perspectives were not ordered chronologically or qualitatively. They co-exist and animate the current debates, with repercussions in the political arena (e.g. ‘the Science Wars’).

We have defended the view that all theories and paradigms should be seen as configurations of epistemic gains and epistemic losses and that it is rare the case that a paradigm is better than one of its competitors in all respects. In the specific case of our argument regarding the three approaches to science, we suggested the need for the co-existence of all these imperfect positions, in order to keep the conversation about science alive and generative of fresh insight. To add more challenge to this theoretical competition, we will end this chapter by reminding that the whole business of explaining science is - from certain points of view - vicious and misleading. The first step to questioning the games of
epistemology consists in one of Bruno Latour’s observations:

The type of explanation possible for religion, art or popular culture no longer works in the case of hard science or technology. This does not mean…that science and technology escape social explanation, but that a deep redescription of what is a social explanation is in order. (Latour, 2000, page 107)

The intermediate step in this manoeuvre is Paul Veyne’s sharp dissection of the core thesis of Michel Foucault’s set of metatheoretical interventions:

Everything hinges on a paradox, one that is Foucault’s central and most original thesis. What is made, the object, is explained by what went into its making at each moment of history; we are wrong to imagine that the making, the practice, is explained on the basis of what is made…The whole difficulty arises from the illusion that allows us to ‘reify’ objectivizations as if they were natural objects. We mistake the end result for a goal; we take the place where a projectile happens to land as its intentionally chosen target. Instead of grasping the problem at its true center, which is the practice, we start from the periphery, which is the object, in such a way that successive practices resemble reactions to a single object, whether ‘material’ or rational, that is taken as the starting point, as a given…we end up fastening the two ends of the chain together with a bit of string called ideology. And, more seriously still, we take the points of impact of successive practices to be preexisting objects that these practices were aiming for: their targets. (Veyne, 1997, pages 160 – 161, my emphasis)

The ultimate step, indebted to Wittgenstein, implicitly invites us to deride the content of all that went into this book and to do away with the commonplaces of our minds, while
opening our eyes to the rough ground where the praxis of science escapes social explanation:

…Under the influence of idealism social analysis nearly always ‘goes too far back’ and so, in consequence, be the mode of explanation realism or social constructivism, misses or destroys precisely what it sets out to study…all these modes share the basic presupposition that cultural activity is ‘guided’ by or is the ‘realisation’ of rules which lie behind actual events and thereby determine conduct in situ: rules which when uncovered or unmasked would serve to explain the constitution of meaningful activity as meaningful. (Harrison, 2002, pages 490 – 491)
2. Privileged Standpoints

I see only shape-changers now.
(Marcus Doel, 1999)

The previous chapter outlined the fact that all epistemologies are imperfect and that there is no privileged standpoint or apriori circumstance that would make one epistemic parish necessarily superior to another. In what follows, I dwell in more detail on these statements, by taking the case of feminist standpoint theory, as synthesised in Sandra Harding’s (1991) contribution, and by providing a multi-layered critique of it. Two approaches are followed: in the first part, feminist standpoint theory is analysed as a discursive practice employing various strategies for intellectual struggle, whereas in the second part my main tools are logic and especially deconstruction. It will be argued that feminist standpoint theory, in itself a matter of power and knowledge, is less defendable and effective than the competing poststructuralist theories of gender formation.

One of the distinctive features in the evolution of geography in the last decades is the development of a powerful feminist discourse, influenced by and grounded on a series of feminist theories from philosophy, sociology, and cultural studies. Feminist standpoint theory has been particularly influential in the 1980s, but its proponents remain actively engaged in the current epistemological debates. Whilst acknowledging the need and value of a feminist geography, I attempt in this paper to suggest that feminist standpoint theory encompasses a series of shortcomings that makes it less valuable than usually
presumed by those who have grounded their feminist geographies on it.
I agree that an epistemic pluralism of partial perspectives - including those of women - is highly important in the production of knowledge. I applaud standpoint theory for outlining the value of women’s perspectives. However, I cannot agree with the claim that women’s perspectives are better than others, just in virtue of the specific social position of women. Standpoint theory has attempted to strategically situate itself between feminist empiricism and postmodernist feminism, but both of these perspectives share my disagreement with the strong claims of apriori gender privilege put forward by standpoint theorists. The following analysis reveals standpoint theorists’ discursive strategy, and, accordingly, the flaws of their arguments.

Understood as a discursive practice, standpoint theory has operated through a five-folded strategy, which comprises:
1) the argument of diversity;
2) the rhetorical metabolisation of fashionable attitudes;
3) the logic of appropriation;
4) plural self-identification;
5) The fifth strategy (included in this study to stand as a sample of a wholly different methodological approach, as the rest of this chapter is rather deconstructionist) is the exploitation of the interplay between two epistemological regimes: that of necessitarian truth and that of pragmatic truth. Standard epistemology portrays science as re-presentation of nature (read reality), truth being a matter of correspondence between nature and its re-presentation or mirroring. The purpose of epistemology is to analyse the accuracy of the ‘mirror’ (Rorty, 1979) and to advance methodologies able to improve it.

Standpoint theory emerged in a cultural context when this epistemology was still dominating, thus being compelled to
justify itself within the limits allowed by its ‘supervisor’: the standard epistemology of that time (see Hartsock, 1983). Harding’s (1991) work is a tribute to it, as its claim is that ‘starting off research from women’s lives will generate less partial and distorted accounts, not only of women’s lives, but also of men’s lives and of the whole social order.’ Leaving aside the compelling cultural context, standpoint theorists also had to impose their views as superior to the already existing encounter between feminism and epistemology, namely feminist empiricism, which has remained committed to the classic values of Truth (as correspondence), Reason, Objectivity (value-free science), endeavouring to reveal the male biases which distort science. Nelson, in ‘Who knows: from Quine to a Feminist Empiricism’, 1990, and Longino, in ‘Science as Social Knowledge: Values and Objectivity in Scientific Inquiry’, 1990, develop provocative pleadings for a non-naïve empiricism, acknowledging Hanson’s (1958) thesis of the theory-laden nature of observational data, as well as the need to move beyond the individual epistemic subject, opening the space for a collective epistemic subject, and thus allowing for the analysis of context and of its relations with the ‘constitutive’ features of science.

The regime of representational truth was not strong enough to ground standpoint theory. On one hand, mainstream epistemologists argued that objectivity is a prerequisite to science, and it can be achieved only through a quest for neutrality, for clear delimitation from the value-laden social world. In the light of this epistemic context, suffice to say that the claim that one social position—that of women—

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7 In the rest of this chapter I refer to this sentence as being ‘Harding’s statement’

favours science appears completely absurd: all social positions are bad for science to the extent that they are social and hence value-laden. The only good social position is that which Haraway (1996) ironically called ‘the view from nowhere’. On the other hand, postmodernists⁹, taking incommensurability seriously, have claimed, in Flax’s words, 1991, p. 37, that

There is no way to test whether one story is closer to the truth than another because there is no transcendental standpoint or mind unmeshed in its own story.

Between these two positions, the idea of an epistemically privileged social position could be sustained only with considerable difficulty. In this intellectual environment standpoint theorists have exploited (fifth strategy) a pragmatic epistemology, within the logic of which the admission of the truth of standpoint theory became quite easy. To put it simply, feminist standpoint theory is true because it is useful for women’s emancipation¹⁰. The all-pervading argument standpoint theorists¹¹ have lately raised against postmodernism is that it

…May eliminate not only the specificity of feminist theory but place in question the very emancipatory ideals of the women’s movements altogether. (Benhabib, S., 1995, p. 20)

As Di Stefano (quoted in Tanesini, p. 257) puts it,

To the extent that feminists politics is bound up with a specific constituency or subject, namely women, the postmodernist prohibition against subject-centred inquiry and theory undermines the legitimacy of a broad-based organised

¹⁰ The ethics of this intellectual policy is discussed by Pels, D., 1996.
¹¹ See also the debate between Hekman, 1997a-b, and Hartsock, Collins, Harding and Smith, all these in 1997.
movement dedicated to articulating and implementing the goals of such a constituency.

The problem with the pragmatic foundation of standpoint theory is that it relies on fourth controversial beliefs, which altogether make it as flaccid as the ‘strong’ epistemological foundation.

First, postmodernism does not prohibit reference to the subject, only attempts to re-conceptualise it. Derrida himself states (1984, p. 125):

I have never said that the subject should be dispensed with. Only that it should be deconstructed. To deconstruct the subject does not mean to deny its existence.

Second, postmodernism is not a-moral or a-political. It simply rejects the meta-narrative of emancipation and replaces it with an ethics and policy grounded on cultivating the idea of difference (in a transformational, not a simply affirmative strategy). It is obvious to me that an ethics of celebrating difference accommodates the emancipatory ideals of all those oppressed, including women (see also McDowell, 2000).

Third, the aforementioned pragmatic foundation relies on an obsolete quantitative logic of political fight: in order to beat the privileged, the argument goes, we must all be united and speak with a single, and therefore (sic), powerful voice. This reasoning is very doubtful. Guerrilla can be more effective than an open war, or, to take the case of electoral competition, in order to win, a group of parties can decide to compete on separate lists, rather than limiting the electoral offer by forming a coalition put forward on a single list. In other words, to the standpoint theorists’ plead for fighting the privileged by means of a single big group of the oppressed, postmodernists would oppose the strategy of a big number of small groups as being more effective. Again, I agree with the latter logic of political struggle.
Fourth, the idea of a classic fight against the privileged is also worrying and naïve, as it relies on an assumption of transparency: in real life, theoretical categories (the ‘bad’ side versus the ‘good’ side) are blurred, and sometimes the oppressors are those who claim to be oppressed. Postmodernism acknowledges these issues, perversely pointing to the perversity of real situations, to the need to let room for contingency in our accounts.

So far, the analysis was concerned with standpoint theory understood as the signified denoted by Harding’s (1991) statement, which was taken to be the signifier. The same strategy is common in interpreting laws: the interplay between ‘the words’ and ‘the spirit’ of a law. From now on the focus will be on the phrase itself, following a three-fold deconstructive strategy: changing the tune, reductio ad absurdum, and dancing to Harding’s tune.

Changing the tune

Instead of ‘dancing’ to the tune Harding has chosen for us, it is important to reveal its assumptions, to dismiss it as less productive than other ‘tunes’ and to suggest alternatives that overcome its short-comings. This strong reading will be exemplified by insisting on only three of the words Harding employs in her statement:

a) ‘starting’: following Derrida, it can be argued that ‘women’ is constructed as the weak term of a binary (men - women). One rule of logic is that in order to understand one term of a binary or dualism, it is compulsory to research the binary as a whole, to seize the ways in which one term is constructed as ‘other’, as difference to the other pole of the binary. All good methods of research will therefore arrive, sooner or later, to investigate ‘women’, even if the research
process starts from men’s lives. To make it clear: a1) ‘men’ and ‘women’ are the two poles of a binary; a2) a rule of logic is that the understanding of one ‘pole’ presupposes also the research of the ‘other’ pole; a3) given a1) and a2), it follows that in this case the starting point of research is highly irrelevant, all that matters being to operate with a good method, which, in order to be ‘good’, has to take into consideration a1) and a2). The same type of argument can be developed through both the rules of logic -as I have just done- and through the insights of deconstruction (the idea of slippages of meaning along and through the chains of signification);

b) ‘Women’: inspired by the inner beauty of Margaret Thatcher’s drastic rhetoric, radical postmodernists would say (and I cannot but agree) that there is no such thing as ‘women’. I agree, partly, with Sally Haslanger’s (2000) project of a peripatetic metaphysics: I believe in the existence of a single Reality, and of a single Truth about it. Moreover, I admit that there are laws, fundamental relations governing this reality. However, although my ontological beliefs might be labelled weak realism- for I reject some critical realist claims, such as the idea of a layered world- I endorse a faillibilist, sceptical and, ultimately, neo-pragmatic epistemology: all access to the world is mediated and distorted by our biological limits (e.g. the concept of ‘umwelt’) and by a series of devices (such as language). The consequence is that it is not possible to compare our view of the World with how the world really is. Therefore a strong foundational epistemology is simply not possible. This is not a tragedy! Instead, following the opportunistic bias of pragmatic scepticism, we can choose the epistemology which is the most fruitful in various contingent situations: sometimes truth – as - coherence, sometimes truth – as –
correspondence (i.e. truth-as-representation), sometimes truth - as – politically - convenient belief, etc.
So far as humans are concerned, we do not know for sure which is the most reasonable way for classifying them:
perhaps the major divide is men versus women, but perhaps it is humans with big nose versus humans with small nose,
and we have been educated through a citational, self-re-enforcing logic, convenient to a particular power-knowledge
system, not to see this major divide and to take the men - women divide as the most important (Butler, 1990, 1993).
Thus, employing the concept of ‘women’ in our discourse means dancing to the tune of the Master, re-enforcing his
categories and hence his power.
Irrespective of this observation and without going so far as Joyce Trebilcot did (her ‘dyke methods’ project is discussed
in Bubeck, 2000) I think ‘women’ is too big a category to cover the diverse realities behind it. Perhaps by obscuring
them, it enhances the injustice done to the most oppressed,
to those who are not even capable to speak for themselves
(Spivak, 1988). Standpoint theorists, despite their recent
revamped rhetoric, cannot avoid being accused of
essentialising women and universalising their experiences
and lives (Grosz, 1995) and this fundamental criticism does
not allow for accommodating strategies.
c) ‘less’ (partial and distorted): in the discussion of my
endorsement of a neo-pragmatic, ‘flaccid’ (Doel, 1999)
epistemology, it was noticed that humans cannot have
unmediated access to the world and that therefore, we cannot
know for sure: 1. what the Truth of the World is and,
accordingly, 2. which scientific discourse is -in absolute
terms- the best, or at least better than others. Harding’s
formulation clearly entails reference to a foundational
epistemology, to what Rorty (1979) considered the
mythology of ‘the mirror’. As it was outlined, such a view is
untenable in a postmodern context. In scientific practice we clearly need to compare discourses and we do compare them. The problem is that all commensurability is ‘precarious’, a mere ‘as-if’ game. This constitutive weakness follows from the key observation that the Truth is not directly accessible and cannot stand as reference for measurements of ‘accuracy’. Commensurability is therefore always contingent: we compare discourses by means of various ‘weak referents’ (i.e. various criteria for the evaluation of theories), such as coherence or fruitfulness, their use varying from one context to another. What I try to suggest is that, according to my pragmatic sceptical epistemology, the rejection of strong foundations does not entail that ‘anything goes!’(Feyerabend, 1975). We do not have strong objective foundations, but we do have weak foundations, on which we base our attempts to compare and rank competing discourses about reality. Because of this situation, we do not have to fear the alleged dark consequences of ‘corrosive relativism’ or the very predicament of scientific enquiry altogether.

We have showed that the privileging of the ‘starting point’ over the proper method (‘aware’ of the men-women binary), the use of the dangerously big category of ‘women’, and the assumption of a foundational, strong epistemology allowing for perfect commensurability, are the three big reasons which permit –individually and collectively- the dismissal of Harding’s statement for being flawed, passé, dangerous, and un-productive.

**Reductio ad absurdum**

Bubeck (2000) uses the classic tool of logic (syllogisms) for a marvellous demonstration of the idea that ‘the picture of knowledge production in standpoint theory is wrong’
(p.191). She pushes to the ultimate logical consequences the assumptions of standpoint theory, revealing that it falls into an epistemic impasse, generated mainly -but not only- by the fact that

...Knowledge without distortion and mystification seemed inaccessible to almost everybody, either in virtue of their position as oppressors of some kind, or, in the case of the most oppressed, in virtue of the fact that many different kinds of oppression were confounded into one social position and experience and were thus difficult to disentangle (p. 194).

She criticises standpoint theory for representing social relations and the production of knowledge in zero-sum / antagonistic terms, and pleads for a dialogical model of the production of knowledge, grounded on the existence of commonalities between apparently rival groups. Bubeck’s project reminds me of a series of tempting but too optimistic models of science production, including ideas from Rorty (1987, the definition of reason as being ‘sane’, ‘civilised’, open to dialogue), Fricker (2000, on perspectival realism), and even Harding (1998, on ‘robust reflexivity’). As a sceptic, I am inclined to pay attention to the dark side also, to what has been called the predicament of human communication12. The excessive optimism of Bubeck’s project notwithstanding, it is significant in that it proves that standpoint theory can be criticised not only through postmodernist approaches, but also with the old-fashioned means of formal reasoning.

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12 Thrift, 1996, p. 87, drawing on Habermas, identifies, for instance, five types of unknowing: unknown, not understood, hidden, undiscussed, and distorted. See also Clifford, 1988; instead of science as ‘open society’ I find more useful the metaphor of heterotopia, as discussed by Foucault, 1986, Gennochio, 1995 and Hetherington, 1997, etc.
Dancing to Harding’s Tune

Even if one closes their eyes to the major epistemological problems of Harding’s statement and accepts the rules of the game as set up by her, the dismissal of standpoint theory remains possible. Under scrutiny here is the list (Harding, 1991, p. 121-133) of the famous eight arguments raised to support standpoint theory, or more exactly, the claim that the feminist standpoint is less partial and distorted than that of men.

Three (1st, 2nd, 8th) of the arguments are totally irrelevant for the purpose of defending the aforementioned claim: they refer to the usefulness of a feminist perspective, and I agree with this position, but they do not defend the idea of a privileged epistemic status for women. Even in the case of the 2nd argument, it is clear that there are no scientific grounds for determining whether, generally speaking, the outsider’s perspective is better than that of the insider. They are both valuable, even though in different ways.

Arguments no. 3 and no. 4 can be rejected through each of the following five observations:

a) There is widespread agreement nowadays that knowledge is the main source of power and, conversely, that power allows the increased acquisition of quality knowledge through a positive feedback loop. If males have power in society, it follows that they also have knowledge, which allows them to control and master that society. It is one thing to say that they distribute distorted knowledge (they manipulate society for a better control), and another thing to say that their knowledge is distorted. Then, it follows that the oppressed should fight the oppressors and ask them to share their privileged access to proper, accurate knowledge, in virtue of which they have been so successful in maintaining their power over the ignorant and manipulated.
society. This reading, grounded in information theory (see Toffler’s ‘Powershift’, 1995) allows saying that the privileged standpoint is that of men, since good information is the key-resource for having and preserving power and patriarchy has been successful in preserving power.

b) The oppressed are not innocent beings, interested solely in equity. Frequently they hate the oppressor and want revenge. Instead of equality, they want to change places with the oppressor and punish him for being oppressor. It can be claimed that often the oppressors fear the oppressed, whilst the oppressed hate the oppressor, and hatred is at least as likely as fear to distort someone’s epistemic standpoint.

c) Both of Harding’s arguments rely on the assumption that the oppressors have a rational behaviour, always following their interests. Another assumption is that they know very well which their interests are. Studies in psychoanalysis, cognitive psychology, and social psychology proved that the assumption of rational behaviour is highly-misleading;

d) Men are assumed to have a coherent attitude in all fields, including science, where they also defend their interests. However, it is reasonable to believe that there are also male scientists who do disinterested research, who hold dear the illusion of truth-as-representation and really want to find out that brand of truth;

e) Social relations and the process of scientific production are understood, each of them, as antagonistic, which is, fortunately, a caricature of how things really are. The Manichean logic of black and white theorising is always denied by the multiple greys of everyday life.

The fifth argument can be attacked in four ways: first, it universalises women’s experiences; second, it unfairly portrays men as being in the ivory tower of public sphere, when in actuality all men also have an ‘everyday’ life and
most of them are not involved in ruling activities (McKee & O’Brien, 1983); third, according to the K.K. principle, you know only if you know that you know: since women are assumed to be centred on daily activities, it is unlikely that too many of them know that they know; fourth, women’s voices are represented through feminists in academia, and this category cannot claim for its support the fifth argument (their perspective is obviously not from daily life, but from the ‘ivory tower’ of the intellectual).

The sixth argument is flawed not only because it universalises women experiences, but also because it is based on biologist assumptions (women have a better unity of mind and body!). Moreover, many male categories -such as peasants or artists- can also be considered to mediate ideological dualisms (e.g. culture / nature).

Finally, the seventh argument can be attacked in three ways:

First, many women in academia do not endorse the feminist project. They work within the paradigm of the oppressor without clearly acknowledging it. Also, given the K.K. principle, it follows that they cannot be considered outsiders within.

Second, as outsiders within, women are better than men only in the discipline called ‘social studies of science’ (they work within academia). They do not have a privileged position for researching the whole social reality. For society as a whole, neither lay women, nor women in academia, are partial outsiders or outsiders within: it is the men - women binary that is central to the constitution of society, and not ‘men’ only (this is the problem with binaries). Women are

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13 The eighth argument was already analysed and rejected for its irrelevance to the problem of epistemic privilege.
14 Also think of what Kuhn called ‘neural programming’, M. Polyani ‘tacit knowledge’, and Althusser ‘interpellation’.
not at all outsiders, not even partial outsiders: they are in (and constitute) the very ‘centre’, together with men. A Russian hermaphrodite receiving British citizenship is an ‘outsider within’ British society, British women are not. Third, given the insights of the SSK, women scientists -one could argue- are driven by their interests just as everybody else (Pickering, 1992), which makes doubtful once more the claim of them having a privileged, more objective standpoint in virtue of their particular gender.

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In the first part of this critique, I took Harding’s statement as a signifier for feminist standpoint theory as a whole, which in turn, was analysed as a discursive practice developed over time through a five-fold strategy. The contextual reading from the first part was doubled by a ‘narrow’, highly specific reading in the second part, Harding’s statement being analysed at three different levels of innocence of reading (‘changing the tune’, ‘reductio ad absurdum’, ‘dancing to the tune’) the last of these being the most ‘innocent’. Both the contextual and the narrow readings allow me to conclude that:

1. It is not clear at all whether feminist standpoint theory has served the interests of all women, but it has been undoubtedly a very effective tool for serving the interests of women in academia. Meant to be (also) a theory about the intricacies of power and knowledge, standpoint theory is in itself a question of power and knowledge.

2. For both purely epistemological and ethical-political considerations, I evaluate standpoint theory to be less defendable and less effective than other theoretical and political programmes. Along the essay, I pleaded for an ontology of weak realism, a pragmatic sceptical epistemology and an ethics grounded on both the celebration
of the idea of difference (in a transformative sense), and the
strive to overcome the predicaments of human
communication. This view, I argued, acknowledges the
inherent imperfection of all discourses whilst allowing for
their assessment and ranking with the help of weak,
contingent foundations. It also acknowledges the fallacy of
invoking apriori grounds for epistemological superiority, in
the sense that all things in this world are derived from
relations, and hence no-thing can be said to be apriori in the
strong foundationalist sense of the concept.
3. Inside / Outside and the Textuality of Contexts

As long as we stick to things and words we can believe that we are speaking of what we see, and that we see what we are speaking of, and that the two are linked: in this way we remain on the level of an empirical exercise. But as soon as we open up words and things, as soon as we discover statements and visibilities, words and sight are raised to a higher exercise…

(Deleuze and Guattari 1988, page 65)

In the first chapter of this book I have presented an overview of the epistemic parishes in contemporary philosophy and social science, to argue, in the second chapter, against the idea of apriori epistemic privilege. In what follows, I explain one further dimension that is crucial to a pragmatic sceptical way of thinking, namely the need to blur the distinction between inside and outside and to fully subscribe to a relational style of thinking. My support for this metatheoretical intervention is a concept central to all brands of geography: the concept of scale.

I will develop an alternative understanding of scale as contexts collapsed within a given / chosen unit of analysis, by critically engaging with recent theories of scale in geography, especially with Neil Smith’s classification of scales, and then, by drawing on the work of poststructuralist thinkers like Derrida, and nonmodern thinkers like Bruno Latour.

Scale has been very much on the geographical agenda in the last three decades, although one should notice that this has been the case especially for Anglo-American geography
(Smith, N. 1992, Smith, P., 1998, Swyngedouw, 1996, Marston, 2000, Herod and Wright, 2002, Purcell, 2003, etc.). Other geographical traditions have not really paid much attention to its theorisation. Thus, in Romania, the issue has been tackled more frequently only in some ‘corners’ of the discipline almost exclusively in geomorphology, leaving aside the traditional, but very specific concern with scale in cartography.

In a recent attempt to look back to the development of the concept and to systematise for a broader geographical audience the saga of scale, Neil Smith (2000) distinguished between three parallel uses (figure 1):

- **Cartographic scale** refers to the area and degree of detail of different maps. A map at a larger scale allows a more detailed representation, but of a more limited area; conversely, a map at a small scale covers a broader area, but with the cost of a less detailed ‘re-presentation’. This matter of choice is as much a matter of power (one decides what is excluded / included) and ethics (the ethics of inclusion / exclusion / choosing);

- **Methodological scale** relates to the researcher’s strategy of dealing with her research area. Some strategies might be costly and time consuming, but allow a finer and more sensitive-to-difference enquiry (e.g. in-depth interviews, oral history), whereas others may be quite shallow, but convenient within a cost / benefit scheme of thinking (e.g. analysing the census, doing a questionnaire);

- **Geographical scale** is by far the most controversial understanding of scale. Neil Smith argues that it refers to how the world is organised, that it exists regardless of human conceptualisation. Nevertheless, he hesitates in fully endorsing a strong ontological claim for geographical scale, contending that, although not visible
Figure 1: Neil Smith’s classification of scales
and not tangible, it is both ‘real’ (operates in the geography / reality out there) and socially constructed (we think in terms of scale, we theorise about it, we assume it exists out there). He then gives examples about how scale is permeated by power, how it is involved in the social reproduction of the capitalist order, and how it serves both as a way of containment, and as a way of resistance (e.g. labour force protests that overcome the disadvantageous relation between the spatial fix of labour and the relative mobility of capital, by ‘jumping scales’ (sic) and imposing political attention at higher levels).

I agree with the general remarks concerning cartographic scales, but I found the other two quite problematic. To begin with, Neil Smith does not say a thing about the nonhuman, about the physical and biological processes that operate at various levels: scale pertains equally to the movements of the Earth (rotation, revolution, etc.), to the tectonic dynamic, to the area affected by rain, to the processes modelling landforms and hydrological regimes of flow within a given hydrographical basin, to the spaces devastated by a (non)human disease, etc. These all operate not at a lower level, within a different realm, as critical realists would maintain (a key tenet of critical realism is that of a layered ontology: the human realm is ‘above’, with its specific causal relations, mechanisms, and processes, whereas the physical and the biological are somewhere ‘below’, with their own causalities, mechanisms, and processes. All this awkward and anti-commonsensical worldview is premised upon problematic readings of otherwise useful concepts such as ‘emergence’ and ‘downward causation’; see Cilliers, 1998, Wagner, 1999, Manson, 2001), but within the ‘messy world’ that we all – humans, cyborgs, plants, animals, rivers, stones, etc.- perform and co-produce (Bennett, 2001, Barad,
As the last decade has witnessed fascinating debates about opening up human geography to the non-human (Whatmore, 1999, Murdoch, 1997, 1998, Thrift, 1999a), and making human geography overcome its uninspired retreat from physical geography (Massey, 1999), the aforementioned account of scale helps to re-enforce an outdated order of things within our discipline. And this leads to a second observation: Smith distinguishes several scales (the body, the household, the local community, the region, the nation-state, the global), and acknowledges that they interact, that they are not rigid, but fluid, in change. Nevertheless, he seems to *reify* the idea of scale, to make it somehow *superorganic*. For him, there is something such as ‘national scale’ that appears to be ‘structural’, more-than-human, ‘above’, a context not of our own choosing, as Marx would have put it. We should beware of sharp separations between ‘social structures’ and ‘human agencies’, or, in different but similar words, between broader social and political determinants and the practices of everyday life, supposedly ‘below’, enframed by the former in the ‘cage’ of modernity. It is much more fruitful to deliberately leave entangled the methodological-epistemological with the ontological. It is only in this way that we might actually put to work in our geographical practices the ‘progresses’ (cf. Bassett, 1999) carried out in the last fifty years in the study of knowledge production. Let me sum up my pragmatic sceptical view: whenever we think of the global, the local, the level of the body, etc., we never think just about ontology, about how things are. Thinking in itself, and scientific thinking particularly, is already a matter of epistemology and methodology. The epistemological is not a moment that joins a given ontological concern, but is already there in that ontology. Given our limited knowledge capabilities as humans, in order to know something we
necessarily close other horizons of knowledge. We can by no means raise claims of a separate ontology, out there, that is waiting for us to unravel, because the very existence of that ontology is a function of our knowledge possibilities. To come back to Smith’s classification, the fourth and final observation is that we would better include the ‘cartographic scale’ in the already messy re-configuration of scale attempted here. Let me elaborate. What is admirable and challenging about cartography is its ambivalence, its position as a messenger / intermediary between ontology (the world as a map that cartographers hope to mirror, knowing better than anyone else that the mirror will always be precarious and that what they mirror is an intriguing and indissoluble mixture of the world and of themselves), epistemology (cartography as a (sub)discipline, in search for reliable knowledge), and methodology (the map-making as craft and skill, as a matter of transmission through generations, as a locus for the production of novelty, as a way of ‘dwelling’, as an expression of ‘fabricated/manufactured’ knowledge, as a means to do power; Keates, 1996, Crampton, 2001, etc.).

The use of cartographic metaphors for all knowledge endeavours (mental maps, cognitive mapping, social cartography, maps of meaning/desire, etc.) has long been with us, and is a tribute to how space and place have always mattered in both our objects of enquiry, and our means for enquiry. Little wonder then, that postmodernist and poststructuralist thinking have reinforced this rhetorical register, and now even literary studies operate extensively with it.

So far, I have criticised Neil Smith’s three-fold classification of scales alongside four axes: the neglect of the nonhuman, the reification of the levels of scale production together with the reinforcement of the structure/agency dichotomy, the
misleading attempt to separate between the methodological-epistemological and the ontological, and the missed opportunity to think of cartography as a broader metaphor for capturing a synthetic sense of scale in geography.

This thesis proposes a fourth type of scale - operational scale - at the intersection of the aforementioned three (and intersecting them all). I understand (operational) scale as contexts collapsed within the unit of analysis. At least four things need to be said about it.

To begin with, I see competing theoretical projects as cost / benefit constellations that express the degree of fruitfulness of a certain opened epistemic horizon. In other words, the privilege of opening a horizon of knowledge is paid by foreclosing the possibility of immersion within other horizons. New theoretical projects never really succeed at bringing new benefits without any new cost. Indebted to Heidegger (1962), Tanesini (1999), and Haraway (1991, 2000), this pragmatic sceptical understanding of knowledge production as an opening of horizons at the price of closing other horizons leaves aside the never ending conflict between the overly optimistic epistemologies (Marxism, critical realism, positivism) that assume the ultimate transparency and out-there-ness of the world, and the overly pessimistic epistemologies (poststructuralism, solipsism, idealism, Rortyan pragmatism) that abandon the promise of science as ‘mirroring of the world’ (Rorty, 1979), and the subscription to truth-as-representation. Instead, the contention made here is that knowledge is possible to the extent that it systematically generates lack-of-knowledge (cf. Thrift, 1983). In simple words, as we are within a given horizon of knowledge (may it be quantitative geography, Marxist geography, or non-representational geography), labouring for its expansion, we cannot be at the same time in other horizons. Therefore, the truths we produce are
necessarily partial and path-dependent. Unlike overly optimistic epistemologies (for a sample see Harvey, 1999a), this account emphasises that the entry point does significantly matter, shaping all the truths and half-truths that animate a given horizon (a convenient but poor synonym for ‘horizon’ would be ‘school of thought’). Each and every horizon generates forms of epistemic neglect. We focus our energies in the direction pointed by the leading researchers within a given horizon, at the expense of other directions that might fruitfully collide with other horizons (cf. Becher, Trowler, 2001).

To be sure, researchers unavoidably get locked into a given horizon of knowledge production but the extent of these epistemic lock-ins (Simandan, 2003a) depends on whether research practices include or not several required therapeutic moments such as self-reflexivity (cf. Ashmore, 1989) and accounts of positionality (see Harding, 1998a-b, Rose, 1997, Sidaway, 2000).

With this crisp epistemology in mind, ‘operational scale’ is not proposed as a substitute for previous ‘bad’ conceptions of scale, but as a new way of thinking that is complementary to the older ones (it has its gains and losses). It is a way for negotiating the spaces between the analytical distinctions separating cartographic scales, methodological scales, and geographical scales.

Second, the adjective ‘operational’ emphasises three of its features:

a) It was thought from a practical perspective, as a helpful way to do substantive research in geography (the theorising arose as a ‘side effect’ when preparing an empirical study in Romania, see Simandan, 2001a-b). It is designed to be usable in what we research as geographers.
b) Scale is never purely ontological, ‘out there’; it is also a way for grasping the world. No matter how much we believe in a ‘reality’, part of the problem that will always harass that belief / conviction, is that it is (also) a belief / conviction. ‘Operational’, however, is not a synonym for 'methodological': ‘the methodological’ is a part of it, but not all of it. To put it simply, ‘operational’ is a way for acknowledging the impossibility of disentangling ontology from epistemology and methodology.

c) We would benefit from thinking scales as performed by and performing through actants, and not as merely 'produced'. Who is then performing scales and through whom does scale perform? The knowing side (ourselves as researchers who choose devices for scientific enquiry) or the ‘knowable’ side (the objects/slices of ontology researched)? The answer is: both. The metaphor of scale performance is better than that of scale production for just two reasons: first, it warns against the risk of reifying scale and of reinforcing the structure-agency dichotomous mode / mood of thinking, as scale is seen as a relation producing its actants. Second, it warns about the limitations inherent in separating epistemology / methodology from ontology. If we take scale as performed and performing, and if we ask who is performing scale and whom scale performs, we run into the very interplay I am trying to unravel: scale is performed by us, as 'researchers', (in the way we choose our unit of analysis, the way we conceptualise the role of its contexts, the way we narrow down to specifics, and the way we are ignorant about certain things) and by ‘the knowable side’ (this trivial distinction is used here to simplify the argument).
Thirdly, the definition of scale as contexts collapsed builds on the reconceptualisations of ‘con-text’ (figure 2 and 3) and of ‘difference’ developed in poststructuralist (Derrida, 1997, see Barnett, 1999) and a-modern (Latour, 1999) thinking. To start with Derrida, he argues that (cited in Barnett, 1999, page 288):

No meaning can be determined out of context, but no context permits saturation... deconstruction would be the effort to take this limitless context into account, to pay the sharpest and broadest attention possible to context, and thus to an incessant movement of recontextualisation.

Barnett (1999, page 289) summarises the specificity (and value) of deconstruction’s approach to context, by pointing to:

The need to rethink the characteristic spatialization of concepts that underwrites the construction of context as a possible empirical object of analysis or norm of interpretation or explanation. (Deconstruction) has done so by calling into question understandings of borders and limits, images of enclosure and representations of stable spatial patterns that are routinely taken for granted in discussions of context... the tendency to take ‘context’ for granted, both as an empirical object and as a theoretical theme, is related to a particular image of space that underwrites the possibility of making clear categorical distinctions between insides and outsides. What is distinctive about deconstruction is the way in which it directs attention towards a thinking of context without nostalgia for a lost presence, however formulated. It helps to call into question the authority of usual appeals to context, whether this is framed as the intention of a consciousness, the communicative horizon of intersubjectivity or as a determinant historical or social ground. Deconstruction’s concern for context is not, therefore, governed by an ethics of proper usage, rightful authority or necessary relations, which is closely tied to a particular spatial regime of conceptualisation. …deconstruction does not reduce everything to the status of a
Figure 2: Standard understanding of context as exterior to/ outside the unit of analysis
Figure 3: Scale as contexts collapsed into the unit of analysis
text. On the contrary, it multiplies and recasts context, and liberates an empirical and theoretical concern for contextualization from the normalising rules that usually govern explanation and interpretation.

In sum, what I take from Derrida is the openness and indeterminacy of contexts, the idea that they are always to be (de)/(re)constructed (knowledge production as creative practice), that they should be freed of the dangerous and narrow spatial imagination according to which contexts necessarily surround something central (a text, a place, a given unit of analysis), and are, therefore, to a large extent, distinct from what is being surrounded! On the contrary, the fundamental premise upon which the definition of scale (contexts collapsed within a given unit of analysis) is based, is that there is no clear-cut distinction between the inside and the outside, that the outside is constitutive of the inside, that, therefore, the inside is already a matter of difference: difference not only of A from B, but also of A from A. In this line also goes the work of Latour (1993, 1999) or Deleuze and Guattari (1983, 1988). Of paramount importance for my redefinition of scale, is the fact that the aforementioned understandings of contexts not only suggest the interplay between epistemology and ontology, but also allow for the prominent role of the political in the excavation of our epistemic commitments (Butler, 1997a, Bourdieu, 2000, Clegg, 2000, Haugaard, 1997, 2003). As Derrida noted (cited in Barnett, 1999, page 290):

> Our interpretations will not be readings of a hermeneutic or exegetic sort, but rather political interventions in the political rewriting of the text and its destination.

And his observation leads to the fourth note about scale, which considers the metaphor of collapse as encompassing
epistemological, ontological, and political dimensions. It bears epistemological-cum-political dimensions because we as researchers choose a unit of analysis and have to collapse its contexts within, if scale is to be taken seriously. To put it differently, our scientific practices are situated and do situate, they are neither innocent, nor purely political. It also bears ontological-cum-political dimensions because scale is performed by, and performs the actants themselves, including the political subjects in the world 'out-there'.

So far, it has been argued that there is not too much profit to be gained from distinguishing between cartographic scale, methodological scale, and geographical scale, nor is it of much help to focus on levels of regulation only (local, national, global) and thus ignore the polymorphous spaces between them (as noticeable in the use of the acrobatic metaphor of 'scale jumping'). The ontological, epistemological, and political are inextricably linked and a geography sensitive to this observation would have to work with a concept of scale that crosscuts naïve distinctions such as text - context, inside - outside, cultural - natural, epistemological - ontological, scientific - political, static - dynamic. While this alternative definition of scale has broader relevance for substantive research in human geography, in this book its uses will be narrowed to enlighten geography's relation with its contexts and the conditions of possibility for scientific knowledge. The understanding of scale as contexts collapsed within the unit of analysis, and the epistemology of gains and losses proposed in the previous pages will enable us in the following chapter to sum up our pragmatic sceptical conceptualisation of the bond between epistemology, ontology, and politics by means of the metaphor of 'recursive cartographies'.
4. Legacies, Rhythms, and Events

In the limits to which it is possible, or at least appears to be possible, translation practices the difference between signified and signifier. But if that difference is never pure, no more so is translation, and for the notion of translation we would have to substitute transformation; a regulated transformation of one language by another, of one text by another.
(Derrida, 1981, page 20)

Consider figure 4. At first glance, it is a mess or a futurist work of art. Yet, it provides a radical alternative to our routine conceptualisations of the world out-there and of our relation with it. A careful eye will detect that the figure is made of four elements: a black rectangle, a red fabric (light gray on next page), a yellow background (black on next page), and a disordered blue line (dark grey pattern on next page)\(^\text{15}\). Let us analyse them one by one.

The black rectangle in this case is a signifier for the discipline of geography as the unit of analysis of the second part of the book, but it might as well be the Ruhr Region, the Pamir Plateau, or the northwest of Madagascar. Its exact shape does not seem to be ‘natural’, and was deliberately chosen to alert the reader to the discursive construction of one's unit of analysis. One speaks of the discipline of geography, and is so concerned with the explanation of its production and reproduction that he/she overlooks how ‘epistemic violence’ (Spivak, 1999, see also Best, 1999) is committed in the arbitrary act of tracing the contours of any

\(^{15}\) The coloured version of recursive cartographies is on the cover of the book.
Figure 4: Recursive cartographies
particular 'entity' in the world out there (and see McEwan, 1998, and the critiques to Livingstone, 1992, in Transactions, 1995, 20/4, all concerned with how the boundaries of geography are manufactured).

Returning to our figure, one would note that the ‘stuff’ filling the black rectangle does not end at its borders but massively exceeds them. All the three elements composing the stuff (the red fabric, the yellow background, the disordered blue line) occur within and outside the rectangle, a fact which reiterates through a different path an earlier observation from the previous chapter: that we have to pay attention to the role of the contexts, to processes happening (apparently) outside our unit of analysis, to the operation of scale. But then, how could one classify those very processes (which together constitute the ‘ontological’, the world out-there)? The answer is painfully difficult, as one has to make educated choices on matters such as simplicity/complexity, elegance, explanatory power, commonsensicality, and so on (see Loux, 2002, on the criteria to consider in the analysis of metaphysical theory building, but also Foucault, 1970, 1994, on the political mechanics of classification). The most straightforward choice would be to re-state our received wisdom (cf. Latour, 1993, Haraway, 2000, Whatmore, 2002): the world is divided into ‘nature’ and ‘society’, with the further subdivision of the latter in the all too familiar (but see the Butler versus Fraser debate in Social Text, 1997) domains of the ‘cultural’, the ‘political’, the ‘economic’, etc. Yet recursive cartographies purport to be a radical alternative to this mainstream ‘Western’ model (as well as to that of hybridity, Whatmore, 2002) in an attempt to leave behind the habits of thought determined by this model. Some of these habits have recently begun to be exposed in the geographical literature (e.g. Thrift and Olds, 1996, Allen, 2003, Barnett, 2003a), but with limited success, as most of
them tend to criticise just this mainstream model, without offering the utopian-constructive moment (Benhabib, 1986) as a subsequent step in their critique.

The ontological model of recursive cartographies prioritises simplicity and elegance over complexity and explanatory power, and this choice enrolls it in the necessary trade-off of any horizon of knowledge (in order to know some things, other things cannot be known; i.e. any ‘paradigm’ is an unstable configuration of epistemic gains and epistemic losses). Put simply, the red fabric in the figure is a signifier for ‘rhythms’, the blue disordered line is a signifier for ‘events’, and the yellow background is a signifier for ‘legacies’. The world then, as well as our discipline (as part of that world), is the result of the interplay and mutual metamorphosis of three elements: events (the closure of the geography department at Salford, the scandal over the Iraq war at the AAG meeting in New Orleans, 2002, the downgrading of the geography department at Cambridge from 5* - the top category - to 5, the retirement of Peter Dicken, the publication of one’s book), rhythms (the Research Assessment Exercise cycle, the year-long timetable of the academic job market in North America, the teaching/exams/vacation cycle), and legacies (the involvement of British geographers in the colonial project, the controversial episode of the quantitative revolution, Richard Chorley’s theoretical contributions, etc). The types of concerns of metatheoretical work in geography dovetail then with this three fold ontology, although they require a complementary focus, as it will be argued in a different book. But ‘recursive cartographies’ can well be applied as a theoretical (and therapeutic) lens for any type of ‘substantive’ work in geography. Consider for example a random passage from the literature of economic geography (Hassink and Dong-Ho, 2003, pages 2-3):
The historical process of industrialisation in North America and Europe is marked by stories of *small accidents* leading to the establishment of one or two *persistent centres of production*. Thereafter *cumulative processes* can generate a geographical structure of production which may be stable for long periods of time (italics added).

I emphasised in the text three phrases. The first is ‘small accidents’ and in recursive cartographies this would be translated as ‘events’. Anything that disrupts the pre-existing order of things, reducing one system’s entropy, qualifies as event. The outstanding features of events are that they bring genuine novelty, they perturb the state of affairs, and they are irreducibly traumatic (on the latter contention, see also Derrida, 2001; note that ‘traumas’ can be sad – a death – or happy – a wedding), though in largely different extents. Political elections, a bankruptcy, a strike, a merger, an earthquake, an international workshop, a foreign investment, an innovation, are all events.

The second word emphasised is ‘persistent centres of production’, which in our three-fold ontology would be labelled as ‘rhythms’. Anything that regulates a place, bringing constancy, predictability, and structural identity to it, constitutes a rhythm. Four decades of communist rule in Romania, the day/night cycle, the production of chocolate in Birmingham, the four seasons of the temperate climate, the urban timetabling of the transportation network, the legal system of a country, the grammatical rules of a language, are all rhythms. They weave the fabric of a place, while being from time to time wounded by events that challenge their ontological hegemony in processes of place formation. The third word emphasised is ‘cumulative processes’, which in recursive cartographies would be classified under the heading ‘legacies’. Anything left in the world that is not either ‘event’ or ‘rhythm’ qualifies as ‘legacy’. Put simply, the legacy of a place is the coagulation of its past events and
past rhythms, with the critical observation that this does not mean that legacy is ‘dead’, lacking agency. On the contrary, it contributes to place formation extensively: our actions are often guided by lessons learned from past actions, the stereotypes that produce the ‘image’ of a place come from past knowledge (and that image significantly influences present decisions – e.g. to invest or not to invest in a region), a rhythm (e.g. the tourist industry / coal mining) might rest on regional legacies (architectural heritage / geological layers), etc.

This simple explanatory framework (the trinity of recursive cartographies, if you want) does not hinder further theoretical sophistication, and this will become apparent whilst explaining the name of the model. Why ‘recursive’, and why ‘cartographies’?

‘Recursive’ means ‘producing each other’ and is used here as a signifier for relational (but non-dialectical) thinking (see figure 5).

In relational thinking (Latour, 1993, 1999, 2002, Thrift, 2002a, Whatmore, 2002) it is the relation that produces the things which apparently create the relation. This approach destabilises conventional ontological wisdom and complicates the explanatory apparatus of geography. For the specific case of the mutual translatability of events, rhythms, and legacies, one would note, for example, that at its beginning each and every rhythm is first an event. However, not all events become rhythms (e.g. contrast the event of setting a textile plant, which soon after the start of production becomes a rhythm, with a strike, which after its end becomes legacy, inscribed in the documents of the time and in the culture of collective protest of the respective community, e.g. Sadler and Thompson, 2001).

‘Cartographies’, as already suggested when advancing the new understanding of scale, is the concept that best captures
Figure 5: Shifting from traditional thinking (above), to relational thinking (below)
the unbreakable bind between epistemology (we, as subjects of knowledge production) and ontology (the world apparently out-there, as object of knowledge production). To understand this, Derrida’s distinction between signifier (the utterance and letters composing the word ‘geography’), signified (the image that comes to our mind when we hear/read the signifier ‘geography’), and referent (the respective discipline ‘out there’, existing independently of one’s mind’s conceptualisation and discursive inscription of it) is useful (Derrida, 1997, 2002). Its usefulness comes from the fact that it alerts us to the fact that always when we think that we think about the world out-there (the referent), we are actually thinking about the signified, for thinking occurs through language, which inescapably compartmentalises the world out there into words/concepts – the vehicles of signification. Our ontologies, then, are always infected with the sins of our epistemologies.

The cartographic metaphor is useful, in that it simultaneously points to the fact that: a) when we read a map, we have mediated access to the world out there, but we should always be aware of the epistemic price paid in the act of mediation, b) cartography is a craft with its own rules, conventions, and tips, and knowledge of these rules might reduce the epistemic price of mediation (e.g. in building the model of ‘recursive cartographies’ I needed to be aware of the weaknesses of other models and of the criteria against which metaphysical research is evaluated, Loux, 2002), c) even within the frame of the same map/model, multiple interpretations of what we see are unavoidable. To give an example, a technological system can be read at the same time as ‘legacy’ (in the sense of Latour’s, 1993, 1999, definition of technology as ‘society made durable’), ‘rhythm’ (e.g. an electronic surveillance system that ‘regulates’ the collective behaviour in a place) or
‘quasiobject’ (a ‘rhythmic legacy’). This openness to interpretation requires then to speak of plural ‘cartographies’ and not of a single ‘cartography’.

The attitude of this book is to favour the utopian-constructive moment in our analysis of knowledge production in general and geography in particular, by providing a set of new definitions and new concepts that coalesce into the distinct philosophical perspective we have labelled pragmatic scepticism. We have already introduced most of the basic tenets that underwrite this perspective, whilst signalling that all of them (the understanding of scale as contexts collapsed within the unit of analysis, the introduction of a crisp epistemology of gains and losses marking one’s labouring within a horizon of knowledge, the theorisation of the inextricable link between epistemology, ontology, and politics with the help of ‘recursive cartographies’) are largely relevant for substantive research in geography’s various branches.

With this framework in mind, we will soon move to a consideration of the discipline of geography (second part of the book), but before this change of focus we need to explain at some length, in the next chapter, the last important tenet of pragmatic scepticism, namely that science belongs to the realm of the mundane, and not to some abstract ideal space with unique properties.
5. Incorporations of the Mundane

In what follows, I will make the case that it is more suitable to speak of 'scientific practices' in the plural and not of a 'science' in the singular. Whilst this detail seems trivial, it produces a more accurate vision of the world of science, a vision associated with the need to move away from thinking science-in-the-abstract to a consideration of the actual spaces of 'science' (see also Livingstone 2000 & see figures 6 and 7). Science-in-the-abstract has been the most frequent (not to say hegemonic) way of conceptualising science: science is portrayed as a body of ideas, theories and methods which generate a whole world in its own rights, an abstract space so thoroughly embedded in our representations that we actually forget that science is far more than this comfortable abstraction. Science-in-the-abstract exposes the fantasy of the admirable unequivocal progress of human knowledge, from total ignorance (the dawn of civilisation), to the ultimate truth (in a remote, but reachable day), by means of the rigorous protocols of scientific enquiry. As this goal is very difficult to achieve, the task of researching the world to find the ultimate truth was made more reasonable by separating various slices of the world and allotting them to a number of disciplines (economics researches the economy, botany researches plants, chemistry researches the properties and interaction of substances, etc.).

The theorisation of this analytical imperative was marvellously done by René Descartes in his 'Discours de la méthode' (1637), where he identified the four rules of proper science: the first rule was to assure yourself of not having unquestioned assumptions which might affect the scientific enquiry, then 'le second, de diviser chacune des difficultés
Figure 6: The spatial imaginary of science-in-the-abstract
Figure 7: The mundane spaces of ‘science’
No ultimate truth & situated knowledge

There is an ultimate truth, but we are condemned to 'discover' only half-truths

Who cares?
that I would examine in as many slices as possible and as many as it would be necessary for best solving them' (my translation from French)

17 'To make everywhere so comprehensive countings and so general revisions, so that I were assured of overlooking nothing' (my translation from French)
In our culture, the notions of "science", "rationality", "objectivity", and "truth" are bound up with one another. Science is thought of as offering "hard", "objective" truth: truth as correspondence to reality, the only sort of truth worthy of the name.

It becomes apparent that this fantasy of science as organised enterprise is functionalist: science is a 'factory' that has the purpose to find the ultimate truth, meanwhile producing a series of continuously improved representations of reality. Everything is organised, regulated, and controlled to make the progress of knowledge irrevocable. Whereas lay knowledge is value-laden, loose, uncertain, doubtful, subjective (a 'tricky mirror'), scientific knowledge is produced in such a way to ensure it is value-free, objective, and, consequently, universal. In Sandra Harding's words (1998a, page 3):

One should try to produce scientific information in which one can find no culturally distinctive interests or discursive resources of the societies that have produced the research. Of course, society and the institutions, cultures, and practices of the sciences should be understood to provide the necessary conditions for sciences to do their work, but they should not influence the results of research in any culturally distinctive way. Any and all social values and interests that might initially get into the results of scientific research should be firmly weeded out as soon as possible through subsequent critical vigilance.

The idea that only the context of science (the latter understood as science-in-the-abstract) is socially determined, and not its content, and that the dynamic of scientific knowledge unfolds in line with an internal 'logic' is usually depicted as the internalist approach to science. (Sandra Harding speaks of an 'internalist epistemology', 1998a). However, the fantasy of science-in-the-abstract partly applies to a good part of the opponents of internalist epistemologies, grouped under the label 'externalists'
(Thomas Kuhn, Paul Feyerabend, Hanson, the Sociology of Scientific Knowledge Schools, etc.). They claim that social factors determine not only the context, but also the content of science and it is the difference of views over the extent of this determination that stands as criterion when separating various schools and streams within the broader family of externalist epistemologies. The aforementioned tenet of external determination has entailed a shift\(^\text{18}\) from a philosophical approach to science to a rather sociological perspective, which has the suitable vocabulary to account for the various intricacies and intimations between society and abstract science. The main point of the Sociology of Scientific Knowledge (the schools of Edinburgh - mainly with David Bloor, 1976, 1992; Barry Barnes, 1974; and Steven Shapin, 1998; Bath - with Harry Collins, York - with Michael Mulkay, 1991 etc.) has been to explain scientific knowledge as a social product, notably by using the concept of 'interest' to unravel how consensus is obtained within a scientific culture, despite the fact that scientists develop the core body of ideas of a particular discipline in so many and hardly compatible directions. In Andrew Pickering's formulation (1992, page 4; see also Golinski, 1998), the SSK schools believe that there is a two-fold strategy going on in the making of science:

On the one hand, actors can be seen as tentatively seeking to extend culture in ways that might serve their interests rather than in ways that might not. And on the other hand, interests serve as standards against which the product of such extension of the net is one that serves the interest of the relevant scientific community best.

The shift from science-in-the-abstract to the spaces of 'science', entailed by pragmatic scepticism, has little to do

\(^{18}\) See chapter 1 for more details on contemporary epistemic parishes.
with the dispute between internalists and externalists in itself. Both are especially interested in how the abstract body of ideas called science is produced and transformed, or, in Pickering's words, in 'science-as-knowledge'. My focus is different, referring to what Pickering calls 'science-as-practice' (see figure 7). The first thing to notice in figure 7 is that science-in-the-abstract is part of the spaces of 'science': this signals that I acknowledge the need to analyse the inner logic (or the lack of ~) of the body of ideas which constitute abstract science, but also that I deny its alleged central role. So far, the internalists have argued that there is a dividing line between the content of science and its context, and that what really matters in epistemology is the first component, although a complete understanding of the history of science involves some greater attention to the social context (for a review see Simandan, 2002).

The spaces of 'science' do not imply a core-periphery model, as contexts are seen as already collapsed within the unit of analysis and the importance of 'external' or 'internal' factors in the dynamic of science changes over time and varies from one place/discipline to another. The empirical studies in the history of science of the Starnberg School of epistemology are useful to back this latter claim. The School was inspired by Werner Heisenberg's ideas about 'closed theories', whose basic argument is that the shift from one paradigm/theory to another does not entail the abandonment of the previous paradigm, but its transformation into a 'classic', together with the recession of its domain of validity. G. Böhme and his collaborators (R. Hohlfeld, T. Schäfer, W. Van den Daele, T. Spengler, W. Krohn) developed Heisenberg's insights, all being strongly influenced by German philosopher C. F. van Weizsächer. One advantage for studying with this framework the 'nature' of geography and its history is that it refers to particular disciplines (and not to science in general,
or to theories, or paradigms, or research programmes), and, as I have already signalled (Simandan, 2002), that it is very flexible, admitting a variation of the degree of influence of the externalist/internalist factors: in the first phase (*the pre-paradigmatic phase*) and the third (*the post-paradigmatic phase*) of the dynamic of a discipline, the externalist factors prevail, whereas in the second (*the paradigmatic phase*), the internalist factors are, by far, the most important. However, The Starnberg School advances just a soft attack on the core-periphery model of science: it admits that the influence of 'externalism' and 'internalism' varies over time and that, therefore, there is no trans-historical central focus in the study of science. Nevertheless, it still uses the unfortunate and misleading 'internalism-externalism' dichotomy (Sneed, 1976).

Bruno Latour's Actor-Network Theory, and in particular his 'Pandora's Hope: Essays on the Reality of Science Studies' (1999; see also Latour, 1987, 2000) delineate a strong attack against this dichotomy. Latour views science as a circulatory system that operates through, and by means of, a progressive accumulation of mediations. He insists on arguing that the distinction between content and context is a misleading and reductionist way of handling the science question. Sometimes we are tempted to merely change 'the dance' (i.e., here, the changing balance of the importance of content/context in the making of science), and forget that the most effective approach is to change the very 'tune' on which we dance (i.e., here, not operating with the content/context dichotomy).

A shift from thinking science-in-the-abstract to thinking the spaces of 'science' provides a more comprehensive vision of the operation of a scientific discipline, beyond the narrow philosophical mindset of internalist epistemologies and beyond the sociological biases of externalist epistemologies.
The shift to the spaces of 'science' gives centrality to the geographical dimensions of knowledge production and implies a broader rethinking of both science-as-knowledge and science-as-practice. Approaching science as a set of practices (Barth, 2002) is much more than a narrow sociological perspective insofar as the production of 'science-as-knowledge' is, in itself, a practice, or, in Foucault's words (Foucault, 1994), a discursive practice (and see Sawyer, 2002). Science-in-the-abstract has been for most of the 20th century the hegemonic representation of what science is, but, as outlined in figure 7, recent schools of thought (cf. Longino, 1990, Lynch, 1992a-b) have challenged the hegemony of this naturalised representation, with the result that in contemporary social science it remains only one among so many other possible representations of science.

Indeed, figure 7 emphasises how various scientists think differently of science. Some reject the idea of an ultimate truth, unmask 'the God's trick' (Donna Haraway, 1991) and plead for increasing awareness of the situatedness of our knowledge claims. Others admit that there is a truth out there, but consider the 'nature' of 'human nature' too poorly endowed for being capable, one day, of finding that ultimate truth. Yet others do not waste too much time on self-reflexivity, and concentrate instead on more mundane issues, like money-making or the climbing of the academic ladder. Some believe in the value of objectivity, some do not, and the intra-disciplinary and inter-disciplinary relations of power underwrite the politics of these beliefs.

Thus, historian Theodore M. Porter in his 'Trust in numbers. The pursuit of objectivity in science and public life', (1995), clearly outlined the political (read power/knowledge) dimension of this variation in the policing of knowledge (op.cit., page 230):
In only a few disciplines is the dynamic of research activity so self-contained that interactions within the community are mainly responsible for the forms of approved knowledge. And in such fields, fields dominated by a relatively secure community, much of what we normally associate with the scientific mentality - such as an insistence on objectivity, on the written word, on rigorous quantification - is to a surprising degree missing. Scientific knowledge is most likely to display conspicuously the trappings of science in fields with insecure borders, communities with persistent boundary problems... So, science is indeed made by communities, but communities that are often troubled, insecure, and poorly insulated from outside criticism. Some of the most distinctive and typical features of scientific discourse reflect this weakness of community. The enormous premium on objectivity in science is at least partly a response to the resultant pressures.

Together with the unravelling of the myth of objectivity, the recent problematisation of rationality renders even more dubious an exclusive concern with science-in-the-abstract. Although 'the idea of making individual rationality the foundation of objectivity in science dies hard' (Porter, op.cit., page 217), there are some promising attempts in this respect, starting with the early insights of Giambattista Vico (18th century) who pleaded for the idea that 'practical wisdom' was an outcome of language. Later, the work in ethnomethodology of Harold Garfinkel and Aaron Cicourel, who focused on the non-rational side of the 'rational', on reason as nothing more than a discursive practice, contributed to the undermining of the modern view of 'reason'. In the same vein, in 'Reason as Rhetoric. On the relations among epistemology, discourse, and practice' (1987), Richard Harvey Brown argued for reason as rhetorical social creativity, showing that this allows for heuristic advantages (page 185), for an openness to alternative types of rationality, as well as for maintaining a
relative validity of science within its own sphere. In his own words (op.cit., page 195):

The conception of reason as rhetorical, as social-linguistic creativity, abolishes absolute divisions between means and ends, between subjectivity and objectivity, between social planning and personal praxis. Instead abstract individual reasoning and concrete social order are both seen as emergent, from practical inter-subjective symbolic action, two moments in the same dialectic, each engendering the order.

From a neopragmatic perspective, Richard Rorty adds a further nail in the coffin of the modern understanding of rationality by proposing two alternative meanings of 'rationality': first, 'to be rational is to be methodical: that is, to have criteria for success laid down in advance' (1987, page 39), second, to be rational 'names a set of moral virtues: tolerance, respect for the opinions of those around one, willingness to listen, reliance on persuasion rather than force' (op.cit., page 40), being a synonym for 'civilised', 'reasonable' or 'sane'.

To complete the picture of the de-construction of science-in-the-abstract, we need to add to the reworking of objectivity and rationality, the set of attacks on the modern understanding of truth-as-representation. Thus, neopragmatists wish to reduce objectivity to solidarity and drop the idea that inquiry is destined to converge to a single point, 'that truth is "out there" waiting for human beings to arrive at it' (Rorty, 1987, page 4). As Rorty puts it, 'there is nothing to be said about truth save that each of us will commend as true those beliefs which he or she finds good to believe...' (op.cit. page 42). As for poststructuralist attacks, they proclaim the death of epistemology, in close relation with three other deaths (see also Flax, 1990): first, the death of the subject, now understood as a 'multiplicity without unity' (Doel, 1999), as decentred, as an effect of a textual wor(l)d (Butler, 1997a-b, Derrida, 1997, 2001), as related to
schizoanalysis (Deleuze and Guattari, 1983, 1988) rather than to psychoanalysis, as a 'fluidentity', as a curious mixture of 'mind' and 'body' (Pile and Thrift, 1995, Barad, 2003; for 'embodied science' see Lawrence and Shapin's Science Incarnate: Historical Embodiments of Natural Knowledge' 1998); second, the death of history, as all historical accounts are expressions of power/knowledge imposed on a reality impossible to objectively capture (Hacking, 2002a-b). Instead of histories, poststructuralists are rather interested in genealogies, following Foucault's early insights on the production of various regimes of truth, and on the power-knowledge woven into the fabric of scientific practices (Foucault, 1970); third, the death of metaphysics – a key-element in the 'cocktail' of Western thought: reality is inseparable from our depictions of it and the subject of knowledge is always already entangled with the object of knowledge (cf. Hacking, 2002a).

Although my plea for abandoning science-in-the-abstract in favour of the mundane spaces of 'science' draws on the rather recent reworking of the fundamental concepts of epistemology (internalism/externalism, content/context, truth, objectivity, rationality, history, subjectivity), it does not presuppose full agreement with all these reconceptualisations. Instead of an 'either/or' position, we prefer a 'both/and' approach that acknowledges the epistemic lock-ins haunting both orthodox and unconventional epistemologies. The 'both/and' approach results from our belief in pragmatic scepticism: we cannot know for sure whether we can or cannot know the world and, as a result of this uncertainty, we have to deploy contingent epistemic devices to our scientific practices, according to the fruitfulness for that specific context of that specific device (e.g. perhaps biologists find it more useful to believe in truth-as-representation, in spite of the smooth theoretical...
arguments that denounce this understanding of truth, etc.). To an extent, the epistemological wars that have agitated academic life in the 1980s and 1990s are due to the misunderstanding (and lack of engagement) between apparently conflicting positions, rather than to substantial differences. As Lawrence and Shapin (1998, page 4) put it:

> It has been, and continues to be, widely assumed that bringing knowledge (or, rather, conceptions of knowledge) "back to earth" can be motivated only by a desire to denigrate or can have the effect only of devaluing the knowledge concerned.

It is in this context of conflict that we will try to render more explicit the valuable insights offered by the shift from science-in-the-abstract to the spaces of 'science'.

First, Taylor's observations (1993, quoted in Thrift, 1996, pages 9-10) help to embed science-in-the-abstract in the spaces of 'science':

> In the mainstream epistemological view, what distinguishes the agent from the inanimate entities which can also effect their surroundings is the former’s capacity for inner representation, whether these are placed in the "mind" or in the brain understood as a computer. What we have which inanimate beings do not have - representations - is identified with representations and the operations we effect on them. To situate our understandings in practices is to see it as implicit in our activity, and science as going well beyond what we manage to frame representations of. We do frame representations: we explicitly formulate what our world is like, what we aim at, what we are doing. But much of our intelligent action, sensitive as it usually is to our situation and goals, is usually carried on unformulated. It flows from an understanding, which is largely inarticulate.

I invite my reader to compare again figure 6 with figure 7, this time bearing in mind Taylor's formulation (op.cit., page 10): 'Rather than representations being the primary focus of
understanding, they are islands in the sea of our unformulated practical grasp of the world'.
Second, and furthering Taylor's observation on the paramount role of inarticulate understanding, I will just underline the significance placed by Polanyi (1958) on *tacit knowledge* and by Thomas Kuhn (1962, 1982, 2000) on *neural programming* as a means of 'inflicting' the minds of the (new) members of a scientific community with a certain inarticulate know-how, involving, to a degree, a shared *weltanschauung*. Kuhn's work on normal science, scientific revolutions, paradigms, and incommensurability cannot be understood but by acknowledging the role played by the 'inarticulate', by the non-discursive in the dynamic of scientific communities.
Third, the mundane spaces of 'science' also rest on the pivotal contribution Heidegger had in the attack on the monopoly of representation in Western culture. His primary concern was with creating a whole new vocabulary for understanding and taking into account the essential pre-givenness of life and the everyday practices. He moved beyond the conceiving of truth as correspondence, in an era when this perspective was taken-for-granted. In his philosophy (Heidegger, 1962), truth is something which emerges out of our encounters with the world, these being 'situated', i.e., constituting a succession of 'projects' within which one's epistemic horizons are challenged and reworked when previously disconnected projects collide. Reality is possible through the pre-discursive daily experiencing of the world, through the production of partial truths.
Fourth, Nigel Thrift's non-representational theory (1996, 1999a, 2000a-d, 2002a) gives to practices a pre-eminence which seems rather bizarre when compared with the traditional 'theories' set up by geographers. The emphasis on practice and the shift - for the theorist - from being a
participant observer, to being an **observant participant** undermine traditional epistemology, insofar as it nurtures a celebration of the situatedness of the actant-scientist, and, accordingly, of his/her partial perspective, as well as a sort of political/ethical engagement, following 'naturally' from the fact that everybody is 'in' (and not above, beyond, or aside).

Fifth, we need to mention here the advantages Andrew Pickering (1992) detects in the shift from science-as-knowledge to science-as-practice, with the critical observation that he understands these two as being **complementary**, whereas my use of *science-in-the-abstract* and, respectively, the mundane *spaces of 'science'* is meant to clearly underline that the latter metaphor renders *obsolete* the former, as it metabolises it and thus becomes more comprehensive and more 'reasonable' for depicting disciplinary communities. Pickering insists on five advantages (1992, pages 6-7): a) Scientific practice is interesting in itself, as a possible subject of enquiry; b) What scientists do is as important as the knowledge they produce, particularly from the point of view of policies of science and of critical engagements with contemporary social realities; c) Studies of science-as-practice can offer insights/(feed) for the study of science-as-knowledge; d) Researching science as practice involves a move beyond the narrow disciplinary approaches, towards highly contextualised transdisciplinary accounts; and e) The conceptualisations challenged by studying science as practice involve a deeper engagement, as they expose core tenets of modern thought in general: distinctions such as subject/object or nature/society are good examples in this regard.

The **mundane spaces of 'science'** involve each and all of the elements mentioned in what follows. Their distinct
conceptualisation is for analytical clarity only and should not foreclose the broader attention on how these components relate and build up the assemblages of real 'science':

**Practice:** beyond the abstract representation of science, scientists actually do research, and this act of 'doing' remains undertheorised and highly problematic (Dewsbury and Naylor, 2002), not least because it is difficult to seize, it is hard to be 'objectively' depicted, it involves ethical considerations, and it determines the quality of the final scientific outcome. From a more political perspective, it helps to disenchant the 'magic' created around science-in-the-abstract and to link better theoretical accounts in epistemology with the lower 'level' of methods;

**Material culture:** figure 7 includes a book and a computer, to suggest the material side of science, the devices upon which it relies and which are produced, used, and improved, in the practice of science. Recent materialist approaches to science\(^{19}\), with Actor-Network Theory as the most radical example, agree in their belief that scientific artefacts substantially determine the content of the knowledge produced. For the unconvinced reader, a softer claim would be that they influence the final scientific products in a higher degree than mainstream epistemology would have had it (for the raising up of the strong claim).

The objects researched by scientists are increasingly included in the social studies of science and the metaphor of recursive cartographies as well as my portrayal of the mundane spaces of 'science' acknowledges this sensitivity to the interplay between epistemology, ontology, and politics. I would not go so far as to claim the ontological irrelevance or

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\(^{19}\) E.g. the empirical work in microsociology and anthropology of science with seminal contributions such as Latour's and Woolgar's 'Laboratory Life', 1979, Pickering's 'Constructing Quarks', 1986, or Latour's 'The Pasteurisation of France', 1988
unsuitability of dichotomies such as subject/object, organic/inorganic (but see the work of Serres, Latour, or Whatmore for stronger claims). Rather, there are the ethical and pragmatic considerations that make them problematic. One solution is to see the researched as active/actant (Haraway, 1991, page 124):

Situated knowledges require that the object of knowledge be pictured as an actor and agent, not a screen or a ground or a resource, never finally as slave to the master that closes off the dialectic in his unique agency and authorship of "objective" knowledge.

If things are put in this light, 'accounts of a "real" world do not, then, depend on a logic of "discovery", but on a power-charged social relation of "conversation"' (op.cit., page 125).

Another solution is to make room for the questioning of 'objects' (this is the role of the rectangle in the picture of recursive cartographies; an alternative idea is to focus on the performativity of objects, as in Latour's developments around the concept of 'immutable mobiles', which act as centripetal forces within otherwise loose networks). Haraway does this questioning by linking projects with their mapping, which is the very process of producing 'objects' (op.cit., page 127):

Boundaries are drawn by mapping practices; "objects" do not pre-exist as such. Objects are boundary projects. But boundaries shift from within; boundaries are very tricky. What boundaries provisionally contain remains generative, productive of meanings and bodies. Siting (sighting) boundaries is a risky practice.

**Scientific communities:** since Thomas Kuhn's revolution in epistemology (Kuhn, 1962, see also Fuller, 2000, Agassi, 2002), the place of the community of scientists in the explanation of science has reached centre
stage (Fuller, 1988). Indeed, all of Kuhn's key concepts (paradigm, neural programming, normal science, scientific revolution) theorise the social practices and social psychology such communities develop (cf. Brown and Lunt, 2002). Geographical scale is always crucial when considering the production and structuring of scientific communities, and subchapter 4.2 will further discuss the implications of this condition.

*The (in)dividual scientist* has received some attention in mainstream epistemology, but this attention was usually limited to his/her performativity within a scientific community. In figure 7, I explicitly individualised the scientists, drawing the details of their heads and their distinct modes of thinking, as I believe referring to them only as pieces within a greater mechanism is far too simplistic (and see Hofer, 2000). Important sides tend to be overlooked: *first*, the quality of a 'scientific' network depends considerably on the relations established among its (human) members, which, accordingly, are (also) a matter of personality match. Karen Horney (1998a-b) distinguished, for example, three basic types of personalities, and although her classification is too reductionist, it is useful to illustrate our point. She distinguished, first, the type 'moving toward people', then the type 'moving against people', and finally those 'moving away from people'. Unfortunately, the visceral dimension of science's actants remains insufficiently explored, in a time when sociological approaches to science are the norm.

Second, beyond temperament and personality, personal history is also crucial (Rose, 1996). Two issues are to be addressed: first, how it influences the theoretical positions of scientists. Oliver (2001), for example, has invited all scientists to cure themselves through psychoanalysis, in his case the beneficial result being 'a working to free up internal
boundaries-particularly between the emotional and the intellectual’ (op.cit., page 150). His point was that (op.cit., page 150):

Attempts to apply psychoanalytic intellectual insights without the emotional grounding of psychotherapeutic awareness seem to risk the reductionism of attempting to divorce theory from practice.

The second issue to be addressed is how scientists discursively produce an image of themselves in the practice of publishing, through the recent fashion of starting one's research by acknowledging parts of one's personal history which is supposed to explain how s/he arrived to do that research in the first place. The contemporary imperative of situating all knowledge by revealing the author’s position puts a premium on these confessions, assuming too easily (see Gilmore, 2001) both a transparent self-knowledge and an honest public account of it, whilst I suspect that the degree of revamping is in these cases too high to allow the innocent acceptance of such narratives.

Third, the individual actant is also a question of embodiment, gestures, and manners (see figure 7). Compare, for instance, the classic, Victorian behaviour of academic staff still found in some countries, with the imperatives of being casual/non-elitist which shape the current behaviour of the academic in the English-speaking world (though there are disciplinary differences here). Also, be aware of the standard image of various scientists (e.g. a geographer is the 'sporty' type, fond of fieldwork and not at ease with spending his/her time indoors).

Fourth, think of the unequal distribution of intellectual capabilities among scientists, and of how brilliant researchers (Bodman, 1991, 1992) relate to average practitioners. The way in which geographers have coped
with the sophisticated poststructuralist discourses since the late 1980s would be an interesting case study. 

*Fifth,* compare the various avenues through which scientists enter academia (Bourdieu, 1988). Some want to find answers to their intellectual dilemmas, some want a comfortable job, some want the prestige and exercise of power associated with it (it is nice to have students and assistants who have to ‘follow’ you), some want to make money, some arrive through nepotism, some do not have better things to do, etc.

*Sixth,* actants in the field of science simultaneously belong to various groups, among which the disciplinary affiliation (geographer, biologist, etc.) is only one (cf. George, 1990), not necessarily the most important. Some geographers believe in God, some do not; some are Marxists, some are liberals; some came from the working class, some have bourgeois roots; some are straight, some are gay, etc. Geographical texts and practices depend on these non-disciplinary affiliations to a much higher extent than usually acknowledged, and the critique of disciplines as obstacles to the progress of knowledge ignores the ways in which ideas and habits flow in society and within the community of scientists with too little respect for disciplinary boundaries.

*Geography:* last but not least, the mundane spaces of ‘science’ involve to a large extent geography. And it is to geography that I turn in the second part of the book.
II. Pragmatic Scepticism
&
Philosophies of Geography
II. Pragmatic Scepticism & Philosophies of Geography

All theories of geography can pragmatically be abstracted into a definition, making the process of comparing and criticising competing views much easier. There are risks associated with this strategy: some words within the definition may not be very suitable for expressing the author's view and thus his/her vision of geography may therefore be misunderstood. This is the case, for instance, with Hartshorne's famous 'areal differentiation' (Hartshorne, 1939, 1960; note, however, that Hartshorne reworked the details of his definition between 1939 and 1960).

In order to increase one's chances of persuading the readers that his/her theory is more valuable, the best rhetorical strategy is to introduce the arguments step by step, trying to obtain one's reader's partial agreements, and then to synthesise everything in a 'proper' definition only at the end of the paper. I will do the opposite: at the beginning, I will advance my 'bare' definition of geography, making a case for it in the subsequent three chapters. I do this partly as a tribute to Karl Popper's vision of the ideal behaviour of a scientist (Popper, 1934/1981, 1963, 1997, 1998), who should always dare and be willing to expose his/her ideas to thorough criticism; partly because it will help the readers (and potential critics): if they agree with my definition, there is no need for them to read the accompanying argument. Instead, they should skip it, going directly to the third part of the book. Furthermore, the arguments supporting my definition (a signifier for the underlying broader theory) will be built by considering the very specific concepts exposed in
the definition and their role in the overall theoretical assemblage.

Definition: Geography is a generic name for a set of various scientific practices, loosely held together, and thus identifiable, by a common and long-standing concern for the big themes of 'space' and 'Earth's complexity', as well as by the networks generated through its having a distinct position in the academic division of labour.
6. Deconstructions

In 1995, Thrift, Driver and Livingstone could assert that (page 2):

The study of science as a social construction has been pursued through a peculiarly spatial imaginary which always attaches sight to the site. The locales in which scientific knowledge is produced act not then as passive backdrops, but as vital links in the chain of production, validation and dissemination.

Certainly, there is a need to see beyond the grand spaces (continents, civilisations, nations), to analyse how sites such as libraries, cabinets, pubs, lecture halls, 'the field', etc., are part of the scientific networks, and indeed research in the anthropology and ethnography of science has focused precisely on these micro-scale spaces. Accordingly, the temporal scale of these analyses has also been restrained to no more than a few years. Roy Porter signalled the danger of having no larger coherent perspective because of too many narrow case studies, arguing that (quoted in Livingstone, 1992, page 1, whose comment was 'I could not agree more'):

Micro-studies are all very well but without an adequate sense of scale and perspective, the real interplay of forces-intellectual, social, and political-cannot be grasped; history becomes impoverished, and our grasp of the present is thereby impaired by default.

David Livingstone, probably the most influential scholar in the specialism usually labelled 'the history of geography', grounded his 1992 seminal work on this need for a meaningful big picture. This implies both a large span of time (his historical narrative starts in the 15th century) and a large spatial scale. Yet, I will argue that his project raises
some ethical issues ignored in the previous comments on his book (see, for instance those of F. Driver, D. Matless, G. Rose and C. Barnett in Tr. Inst. Br. Geogr., 20, 1995). In the paper 'The spaces of knowledge: contributions towards a historical geography of science', Livingstone reflects that (1995a, page 1) '…it is disquieting and, I think, ironic that historians of geography have taken so little account of the spatial in the histories they have produced'. Ironically, the remark applies to his own 'The Geographical Tradition'. To be sure, Livingstone concedes that his work (1992, page 30-31):

Is biased towards English-language geography, not because other traditions are unimportant, but precisely because they are too important to continue to be parodied in traditional textbook fashion. I simply do not know enough about these traditions to begin the task.

Nevertheless, the actual problem lies in that a book which is entitled 'The Geographical Tradition' and which has imposed itself as Peter Gould has anticipated as 'the core' (his emphasis) of all courses and seminars in the history and philosophy of our field (the phrase is quoted on the back cover of the book, contributing even more to a 'virtualist' dynamic; cf. Miller, 2000) is far from comprehensive. Livingstone added the subtitle 'Episodes in the history of a contested enterprise', but, notwithstanding the fact that it is not mentioned on the front cover, it can act as an excuse for temporal selectivity, rather than for spatial selectivity. To grasp this significant play of signifiers, note that Ron Johnston (1997) was cautious enough to entitle his book 'Geography and geographers. Anglo-American Human Geography since 1945', setting (on the front cover) the temporal and spatial limits of his narrative. Livingstone failed to do so and it is astonishing that he did not see the problem. Thus, he states (Livingstone, 1992, page 31): 'Of
course comprehensiveness is, I believe, quite irrelevant to
the task I set myself' and adds with some humour that he
waits for his reviewers to identify 'the gaps that I have both
purposely and unintentionally left unfilled'.

To summarise my position, there are considerable epistemic
and ethical problems arising from the insufficiently
acknowledged exclusionary content of a book established as
the key text in the history of geography, partly due to its
generous, but misleading title 'The Geographical Tradition'.
Furthermore, I find unfair and unproductive the logic of
comfortable geography which characterises the English-
speaking world: it systematically refuses to open itself
towards its 'others', the uncomfortable marginal geographical
schools (Japanese, Romanian, Hungarian, etc.). The strategy
of taking the easy ways by merely acknowledging the
limitations of one's narrative to the English-speaking world
has to be confronted and challenged and perhaps the
international geographical organisations should be actively
involved in this task.

So far, I have criticised the discursive gap opened between
the rhetorical plea for taking geography seriously in the
study of science and the way in which the issue is left aside
precisely by those who preach it. But one could add that,
even when geography is considered, there is a risk of
choosing the wrong space: instead of the real spaces of
geographical communities, some choose a metaphorical
space, which ultimately performs the fantasy of science-in-
the-abstract for the specific case of geography. For example,
Peter Haggett asserts of geography that (1993, page 223):

In some respects it resembles a city with districts of different
ages, [some built a] century ago and sometimes in need of
repair; and these are areas which were once fashionable but
are so no longer, while others are being rehabilitated; other
districts have expanded recently and rapidly; some are well
built, others rather gimcrack. If we use the city metaphor, then
geography has extended beyond its medieval walls to form a sprawling conurbation with other subjects.

It becomes apparent that Haggett refers to the abstract space of the various branches of geography (regional, physical, human, etc.) and that, although his metaphor is better than the standard vision (science-in-the-abstract), it still does not let mundane geography in. If one is to think of the various geographical schools which actually exist, then there is no need for metaphors: instead, it is crucial to see 'geography' as a set of various scientific practices scattered over (global and national) spaces: they do not communicate but in a superficial way with one other, and there is no contiguity between them, but a set of political, cultural, historical, spatial, and theoretical gaps. These gaps are a fundamental feature of the geography of geography and should make one very cautious of referring to geography as to a 'network' (in the singular). Seen in global perspective, the discipline is rather a set of networks that have superficial exchanges between them. Discontinuity prevails, but we rarely pay attention to this spatial condition given that hegemonic modern thought has interpolated us in the portrayal of science as supposedly neutral (to the contingencies of contexts, i.e. particular cultures in which science is practised) and, therefore, universal. Little wonder than, that good criteria for separating science from pseudo-science consider the extent to which scientists from different cultural settings are communicating in a shared/universally-accepted language. These normative stances privilege those sciences that are highly formalised (mathematics, physics, chemistry, logic, etc.), constituting a real network over the global space, and not just a set of scientific practices scattered over discontinuous spaces.

Geography, the social sciences, and the humanities belong to this second spatial 'model' of disciplinary formations and
this state of affairs contributes to their dismissal as not-yet-sciences /'soft' sciences. The alternative epistemologies blossoming in the wake of the Kuhnian Revolution rehabilitated this second group and celebrated the epistemic capital produced through scattered scientific practices. This added to their celebration of the political capital arising from their potential as sites of resistance to the totalising project of modernity, built on the idea of the universality of science and, accordingly, on the use of science for controlling and changing the world. Good examples in this regard are the epistemological projects of Donna Haraway (1991, 1997, 2000; her metaphor of 'situated knowledge' undoes the myth of the 'view from nowhere', of the 'God's trick' on which pre-Kuhnian epistemology has relied) and of Sandra Harding20 (1987, 1991, 1998a-b). Peter Haggett uses the city metaphor to shed light on the nature of geography, but, despite appearances, this metaphor obscures rather than clarifies what geography is. Thus, as mentioned before, the geography of geography, unlike a city, involves major gaps (and not contiguity), lack of shared knowledge (rather than proper communication and exchange: see, on this issue, Thrift, 1996, page 87, who, drawing on Habermas, identifies five types of 'unknowing': unknown, not understood, hidden, undiscussed, and distorted), and 'multiplicity without unity' (Doel, 1999), rather than some essentialised functional totality. For this latter remark, note that poststructuralist and post-Marxist sensibilities are compatible with the idea of totality to the

20 She argues for a 'robust reflexivity', built on the thesis of 'strong objectivity', i.e., of a rational attitude which consists in considering the role of cultural contexts as constitutive to science and potentially enriching it. Moving between postcolonialism and feminism, Harding speaks of 'borderland epistemologies' as tools for forcing Western culture to fairly engage its 'others' and to transform itself accordingly; see also Spivak, 1999, 2003.
extent that it is reworked and opened up. Thus, Laclau and Mouffe (quoted in Gibson-Graham, 1996, pages 221-222) explain that:

Our vision is to a large extent holistic, since it presupposes that any identity is differential…and that the systems of differences are articulated in totalities which are "historical blocs" of "hegemonic formations". But unlike classical sociological holism…we do not feel these configurations or social totalities to be self-regulating totalities, but precarious articulations that are always threatened by a "constitutive outside".

There is a striking resemblance between Laclau and Mouffe's vision and Alisdair MacIntyre's theory of tradition (developed in 'After Virtue', 1981, and 'Three Rival Versions of Moral Inquiry', 1990). He writes (quoted by Livingstone, 1992, page 347):

A living tradition is a historically extended, socially embodied argument, and an argument precisely in part about the goods which constitute that tradition. Traditions, when vital, embody continuities of conflict.

It is on this vision that Livingstone builds his work, but I find in Laclau and Mouffe a too conservative view of entities. Instead, the rhetoric -and ontology- of 'fluidentities' (Doel, 1999) has more potential to move beyond the inside/outside traditional divide (see also our theorisation of scale in chapter 1). I endorse Boundas' vision (1993, page 15), who, in comparing Deleuze's understanding of the 'outside' with Derrida's 'supplementarity', argues that:

The outside is not another site, but rather an out-of-site that erodes and dissolves all the other sites. Its logic, therefore, is like the logic of difference, provided that the latter is understood in its transcendental and not in its empirical dimension; instead of difference between x and y, we must now conceive the difference of x from itself (my emph.).
My attempt to suggest this difference of geography from itself had its starting point in the theorisation of the mundane spaces of 'science', with their fundamental insistence on the real geography of real geographies. Otherwise, one can hardly avoid lapsing into functionalist and essentialist views of geography. Take for example Haggett's use of the city metaphor. First, it casts an essentialist perspective over geography. To define the terms, essentialism is, according to Barnes, 2000a, page 230:

The philosophical idea that all phenomena and events are reducible to fundamental and inviolable properties, essences, that determine their character. A failure to refer to those essential properties is a failure to know those events or phenomena at all.

The attempts to define 'cities' constitute a classical example of essentialism in geography (cf. Beaujeau-Garnier, 1974) and the comparison of geography with a city unwittingly reproduces this epistemic sin. Note that Haggett dwells on an even more explicit complicity with essentialist theorising, for he portrays the history of geography as an expansion of the city over its medieval walls, to form a conurbation with other disciplines. In other words, he forges a core-periphery model that compellingly illustrates an essentialist view of geography: the expanded modern city owes its identity/essence to its medieval heart, starting from which derivative parts have been added.

Second, Haggett's definition embraces functionalism. Derek Gregory (2000, page 283) defines it as:

A perspective from which the world is seen as a set of differentiated and interdependent systems, whose collective actions and interactions are "instances of repeatable and predictable regularities in which form and function can be assumed to be related" (Bennett and Chorley, 1978) and which explains these form-function relations in terms of their role in maintaining the continuity or integrity of the system(s).
Functionalism is flawed both logically ('the unintended or unanticipated consequences of a form of social conduct cannot be used to explain its existence in the first place', op.cit., page 284) and substantively (it 'assumes a purpose - "needs" or "goals" - without a purposive agent', op.cit., page 284). Coming back to Haggett's metaphor, one can easily remember that much work in urban geography has been concerned with the functional zones of the city, with the way in which the urban system works. Haggett's functionalist portrayal of geography is even more noticeable when he comments on its 'districts' (subdisciplines): some are 'in need of repair' (my emph.), some are 'well built, others rather gimcrack'.

A final note on this mis-leading use of spatial/geographical metaphors refers to Haggett's conurbation as a (metaphorical) way in which geography has opened towards other disciplines. Fortunately, this is not the case, for there is no core of geography\(^{21}\) whatsoever: the interaction with other disciplines encompasses all of geography. There is no such thing as an untouched, pure 'heart' and hybrid, 'impure' forms around it that interact with neighbouring disciplines. The symbolic dimension of Haggett's discourse has far-reaching ethical implications. I will point here only to the constitutive ambivalence (cf. Pratt, 1999) of the depiction of hybridity: whilst the heart refers to 'purity', 'essence', 'identity', the hybrids around it, although more vital and dynamic, are, in a sense, inferior, lacking the prestige of purity and the legitimisation that history confers in Western culture (see also the Athena’s complex for the ways in which 'inferiority' is possibly a ground for 'superiority').

\(^{21}\) Two classic examples of geographical discourses that claim the existence of a core for our discipline are Wooldridge and East, 1951, and James and Jones, 1954.
The celebration of essentialist approaches implies an unfavourable and unfair treatment of hybridity, with all the epistemic and -above all- ethical issues poststructuralist and postcolonial theorists have endeavoured to signal (Bhabha, 1994, Young, 1990). Essentialism, and particularly essentialist definitions and metaphors of geography, engender power-knowledge relations and ethical debates. It is not difficult to observe, how, for example, the status of the practitioners of geography is affected by claiming that some branches are more 'geographical' (and, therefore, important and legitimate) than others. The periphery - to combine essentialism with functionalism - does the dirty, low-paid work, which is subsumed to the sophisticated work (usually synthesising or refining) done within the alleged heart of the discipline by its most brilliant practitioners.

Ethics aside, essentialist and functionalist thought in geography raises epistemological issues as well. Firstly, because some parts of an entity are taken to be more important, there is the risk of neglecting fruitful avenues of scientific enquiry for the very fact that they are labelled/stigmatised as non-essential and, on this ground, penalised in the disciplinary power games (funding, etc.). Secondly, in a broader perspective, even if we admit an essentialist ontology - and I do not see consistent arguments for irrevocably denying such a possibility- I find it very difficult nowadays to endorse epistemologies other than the sceptical ones. Indeed, even if there is a reality and a truth out there, we cannot know for sure whether our scientific findings are in a strong sense 'true', because, as J. Flax (1990, page 37) notices, 'there is no way to test whether one story is closer to the truth than another because there is no transcendental standpoint or mind unmeshed in its own story'. Putting things differently, even if geography is a totality with some essential features,
we cannot know for sure whether we identified them correctly. Confidently claiming that we did is both naïve and deontologically problematic, for such claims are usually grounds for decisions in the policy of science, which, in turn, have repercussions on all scientists/geographers.

Third, apart from ethical and epistemological considerations, in the case of defining geography one could charge essentialist and functionalist approaches on purely ontological grounds as well. Before trying to identify the supposed essences of geography, one should start with the basics, by simply asking 'What's in a name?', by approaching 'geography' as a mere word/signifier. Only afterwards is one justified in problematising which parts of those things which might be part of the category of 'geography' are more important than others in the shaping of its (presupposed) 'identity'. And if one does so and takes the geography of geography seriously, one can notice that the various schools of geography scattered over the Earth have few things in common. Furthermore, even if one concedes that a geographical school has a heart/core/essence (for instance in Romania geomorphology and regional geography play a more important role than in British geography), one has to admit that what is at the 'heart' of each school varies considerably. In the most generous scenario, one could speak of the essence of a certain geographical school, but not of an essence of geography in general, for our discipline is not a 'structured' entity/system totality, but a set of various scientific practices loosely connected and held together under the same label/signifier, in ways which have nothing to do with essentialist and functionalist accounts.

The aforementioned observations insisted on the present, on the geography of geography, but the same argument can be made for the history of geography. In Livingstone's words (1992, page 28):
The idea that there is some eternal metaphysical core to geography independent of historical circumstances will simply have to go. [...] For geography has meant different things to different people in different places and thus the "nature" of geography is always negotiated.

The reader will note, however, that one can dismiss Livingstone's argument on the ground that the different meanings different people have attached to geography do not imply that geography as such does not have an essence. *How it is perceived is an issue, what it actually is, is another issue.* Curiously, Livingstone problematises the history of geography, but forgets to properly problematise 'geography'. He says (1992, page 30): 'As I see it, geography is a tradition that, like a *species*, has undergone historical transformation' (my emphasis). Be that as it may, the fact that despite historical transformation, we may still speak of *that species* requires admitting an essence of that species (in virtue of the distinction between natural kinds versus objective types in traditional metaphysics: a species is a natural kind, with an 'essence'). Either geography is like a species, and then it can be conceded that it has an essence (a case in which Livingstone should have dared to explain to us what that essence is; a fact which would have allowed us to speak unproblematically of the category of geography, despite its historical partial transformation), or geography is not like a species and does not have an essence (a case in which Livingstone should have explained how we can speak of 'geography' and of it having a history without ultimately saying what is in that very name). There are epistemic and ontological escapes from essentialism, but there is no escape from the need to define the tools of our discourses. In the doing of science we are inherently confronted with, and limited by, our own capabilities (we do not have, for instance, unmediated access to reality). It is reasonable then to try to avoid further limitations, by agreeing to
define/explain what we are doing as scientists. Livingstone avoids the risk of being criticised for not offering a good enough definition of geography, but he cannot avoid the risk of being criticised for not confronting that risk.

We have argued so far that the set of various scientific practices labelled or self-identified as 'geography' are, from an ontological perspective, unlikely to have a core/heart/essence, given: first their peculiar geography (they are scattered over space; gaps prevail over connections between 'schools'); second, their highly problematic history, complicated with historiographical controversies ('Is the history of geography possible?'); third, the compelling distinction that has to be drawn between the theories of what geography is/should be and the reality of geography's discursive and non-discursive practices, exceeding these rhetorical levels. For example, in the early 1990s Yi-Fu Tuan asserted that 'geography is the study of Earth as the home of people' (Tuan, 1990) but in the same year what were American geographers actually studying? If most of them indeed studied the Earth as the home of people, is this a ground (a quantitative one) for saying that the essence of American geography was, in the early 1990s, what Tuan argued?

In explicit definitions of geography, what is 'essence' appears rather obvious to the reader. But to what extent and on what grounds can we speak of essences when we move beyond explicit definitions and focus on the practices and non-metatheoretical works of geographers labouring within a given 'school' (the concept of school is in itself problematic, as it casts the idea of homogeneity within a scientific community only because it was shaped within a given culture. If we admit that any given school is in itself a convenient/totalising label for the variety of scientific practices beyond, then finding essences, even at this more
manageable scale of analysis, becomes abusive)? To further our enquiry, let us assume that most Hungarian geographers identify themselves as regional geographers. Is this enough to say that the essence of Hungarian geography is regional geography?

The point is that those geographers can claim doing regional geography only in virtue of subscribing to a set of beliefs about what regional geography is. But their set of beliefs on what regional geography might be are likely to be (slightly) different from one geographer to another. The problem of finding essences is translated then to this lower level, and so on. The conclusion I have attempted to argue for is that all non-metatheoretical 'geographical' work easily escapes from being subject to the identification of its alleged essences. Yet, there is a remedy to this theoretical slippage: instead of looking for essences (which are important only when analysing metatheoretical work), one would better try to figure out the processes of 'neural programming' (Th. Kuhn) whereby the 'tacit knowledge' (Michael Polanyi) of a scientific community is produced and re-produced. However, as I shall argue in what follows, there are interesting processes going on between the claimed essences of geography (present in metatheoretical papers) and the tacit knowledges and everyday dynamics of scientific communities.

My plea for taking real geography seriously when defining geography partly developed as a critique of Haggett's misleading use of a geographical metaphor. Subsequently, this allowed for a broader critique of how essentialism and functionalism are handled in remote or recent works in

22 I.e. I use ‘metatheoretical’ and/or ‘paratextual’ to include all geographical work that is self-reflective; everything that is not metatheoretical forms ‘substantive geography’ or ‘textual’ geography.
metatheoretical geography (I include here the history and historiography of the discipline, also).

A question remains though: is an essentialist approach in defining geography necessarily a bad thing? And if not, in which ways could it be exploited, so that its negative effects are minimised? The argument will unfold by means of an analysis of the ways in which a definition can be produced and exploited, and, by means of a discussion of the recent metatheoretical debate whose protagonists have been David Livingstone, Clive Barnett, Felix Driver, David Mattless, Gillian Rose (all in 1995), Ron Johnston (1997) and Marcus Doel (1999).

Any definition synthesises a theory about the object defined. In the definition I have provided for geography at the very beginning of this second part of the book, every word and all the association of words were carefully chosen so that the definition could act as a serious signifier for a broader theory of geography. In the pages of this chapter, I have unpacked that definition, revealing systematically the theoretical approaches on which it was built (i.e. the underlying theory that structures that definition). My propositions heavily rely on the theorisation of the shift from science-in-the-abstract to the mundane spaces of 'science', which, although not explicitly mentioned in the definition, lies at its very heart. A definition of a discipline implies a theory about it, even though that theory is not what we usually expect a theory to be.

Following a similar logic, all histories (stories) of a discipline imply a theory of that discipline, since the writing of the story of something is the construction of something. At stake here is that the reader realises that the main categories of any metatheoretical endeavour (e.g. a definition, a theory, a (hi)story) encompass a broader theory, and hence can be subject to translations and comparisons in
an easier way than initially assumed. The significant amount of metatheoretical work in geography is not at all the sign of the low-quality of the discipline or of a disease (e.g. Livingstone speaks of a definitional neurosis among geographers, but forgets to analyse the theoretical and practical stakes implied in the production of definitions/metatheories). Quite the contrary, it is a sign of health and openness, especially if one considers the fact that one of the main features of contemporary science-broadly speaking-is the unprecedented emphasis put on reflexivity, on the doing of science and on the ways in which this doing shapes, and is shaped by, more general social relations.

Among the major epistemic sins that haunt existing theorisations of geography, we have discussed in this subchapter the problem of essentialism and that of functionalism. But note that in metatheoretical work on geography it is often the case that essentialism goes hand in hand with normativity.

When one defines geography very narrowly (take for example Robert Dickinson, 1969, who considered that geography is 'fundamentally the regional or chorological science of the surface of the Earth'), (s)he ascribes to it a certain 'essence' (what is proper geography), everything else being non-geographical or less-geographical than that which constitutes the presumed essence. In the history and historiography of geography normativity translates itself into presentism, in that it extols a certain corrective 'should': the story of the tradition is told in such a way so as to convince/manipulate the audience that present geography should be mainly X, since its past is retrospectively constructed to have been mainly X as well (e.g. in Hartshorne's case, X was regional geography). It is not difficult to understand how normativity, functionalism,
essentialism, and presentism are put at work together for the manufacture of partisan arguments. What seems important in this context is to state clearly that whilst presentism is always 'bad', normativity and essentialism are not always to be blamed. In the aftermath of foundationalist epistemologies, scientists are not only concerned with answering how reality is (the regime of representational truth), but also with controversies over how the world should be (see the normative dimensions of various strands of Anglo-American critical geography, Castree, 2000) or over the more useful ways to conceive reality (a 'pragmatic' regime of truth). By advancing normative and essentialist definitions, one can serve these alternative endeavours of post-Kuhnian science. Gayatri Chakravorty Spivak (1990, page 12) brilliantly made the point: 'You pick up the universal that will give you the power to fight against the other side, and what you are throwing away by doing that is your theoretical purity'. In the case of geography, under certain conditions, essentialist-normative definitions can help the emergence of new research programs, of new geographical schools, which in turn contribute to the diversification and sophistication of the discipline. Consider, for example, Fred Schaefer's (1996/1953) definition of geography, which set the agenda for spatial science: geography is (to read 'should be') 'the science concerned with the formulation of the laws governing the spatial distribution of certain features on the surface of the Earth'. I mentioned, however, that there are some conditions to be respected: essentialist-normative definitions of geography are useful tools for stimulating the diversification of the discipline, but they are acceptable under the condition that their normativity is explicitly stated. When one presents his/her view of what geography should be in a paper called 'a
theory of geography' or 'a manifesto' (consider David Harvey's 1984 'On the history and present conditions of geography: an historical materialist manifesto', Nigel Thrift's 1983 'On the determination of social action in space and time', or Peter Kropotkin's 1885 'What geography ought be') one can judge the pertinence and vigour of their project and, accordingly, endorse or reject it. What I believe is less acceptable is the hidden use of normative approaches, such as in the manipulation, through too biased accounts, of the history of the discipline.

On the other hand, one should pay attention to the fact that the very distinction of normative definitions from descriptive definitions is problematic, as it helps to hide the construction of any description, the fact that the accuracy of competing descriptions is hard -if not impossible (see Rorty, 1979)- to be determined in absolute, definitive terms. Therefore, if one deepens his/her reflexive analysis, (s)he will reckon in any merely 'descriptive' definition a soft kind of normativity.

The definition of geography theorised in this chapter tries to be as descriptive as possible; nevertheless, for its proper contextualisation it is important to add to the unpacking of the 'normative vs. descriptive' dichotomy, an analysis of the distinction between too weak and too strong definitions. To the extent that the definition provided occupies the middle ground between these two poles, it makes sense to consider why the extremes are less fruitful options. For too weak definitions, we will analyse David Livingstone's approach; for too strong definitions, we will consider the definition proposed by Marcus Doel.

In Doel's words (1999, pages 103-104):

The name itself relays an irreducible perturbation and solicitation: geography, from the Greek "geo-graphia", means earth writing or earth drawing. Many would have us believe that geography is merely representational, that it presents again (it copies and reproduces) in innumerable media what is
always already present to itself "out there" in the so-called "real world". [...] the more perverse amongst us might argue that if geography really were representational, then the status of its works would be undecidable since one could not be sure in the braiding of … writing-earth-writing-earth-writing … which came first: this earth or this writing…a reiteration entails a transformation, a redistribution of energy and effects…As a reinscription and reiteration, geography is an exemplary transformer (my emph.),

Add to this his attempts to (op.cit., page 104) 'slacken the hold of Platonism in geography, of that dutiful reproduction that disavows its own participation in transforming the texture of things' (my emph.). Despite the nice wording and the flattering depiction of geography as a virtualist discipline (see Miller, 2000), Doel's account cannot but be criticised when confronted with the reality of a rather dusty and ignored discipline. Geography has always been a Bovaric discipline: according to its metatheorists, it should have been supposed to play the role of the queenly science, bridging, synthesising, and refining the findings of 'normal disciplines' and serving as key resource in policy-making, in the transformation of reality\textsuperscript{23}.

In reply to Doel's wishful thinking (too strong) definition of geography, I would make the observation that the weakness of the discipline should not be attributed -as it has often been the case -only to its failure to achieve the high standards of scientificity established by the epistemological hegemony of positivism, but also to its failure in being a 'really virtualist'

\textsuperscript{23} And see the whole celebration of the rational, scientific organisation and management of space pervading the sub-discipline called territorial planning- the very proof of geography's relevance; on similar grounds, some have ventured to say that geography would be the most fashionable discipline of the 21st century- the claim is critically discussed in Simandan, 2000a-c.
discipline, (as compared with economics, for example), in contributing to the transformation of reality according to its all-too-optimistic rhetoric.

For the case of too weak definitions, David Livingstone's approach makes necessary a more detailed critique. The main problem lies in Livingstone's definition of geography in the same way that almost all disciplines could be defined (succinctly and metaphorically): as a tradition, extended conversation, experiment, species evolving over time. He does not clearly state, however, what makes geography a distinct discipline, probably fearing that this inevitably involves essentialism.

Second, he oversimplifies when depicting geography as an argument 'precisely about what constitutes the tradition' (1995b, page 420). Instead, I believe it is useful to admit, with caution, the analytical distinction between paratextual geography (a synonym for this is metatheoretical geography) and substantive geography, the first referring to the whole debate about what geography is and what geography should be, while the second points to the very 'real work' done in geography by its 'common' practitioners (geomorphologists, medical geographers, regionalists, etc.). It is too often the case of writing histories of paratextual geography without acknowledging clearly enough (the way Ron Johnston does, for instance, in the preface to his Geography and geographers ... , 1997) that beside this history, there is another history, that of substantive geography, commonly analysed at sub-disciplinary level (the history of climatology, of economic geography, etc.). Otherwise, the tradition of geography in the narrow way Livingstone defines it would not be but a sterile philosophical debate.

Third, although Livingstone admits that the (sub)traditions of geography can be easily 'constructed' in all sorts of
artificial, doubtful ways, he does not pay too much attention to his own style of construction, which can be criticised on several grounds. To be sure, he identifies ten traditions, briefly described under the headings: 'to the ends of the earth' (exploration), 'magical geography', 'a paper world' (cartography), 'a clockwork universe' (theological geography), 'an instrument of imperialism', 'the regional recitation', 'the go-between' (environmental geography; i.e. culture/nature relations), 'space science', 'figuring people out' (Marxist + humanistic geography), and 'everything in its place' ('structuration' geography).

To begin with, this 'cutting' is more exclusionary than it has been assumed (e.g. see the apologetic quotations from the back cover of the book): a) Livingstone's geography starts quite late -only in the 15th century- although this is better when compared to Stoddart's even harsher choice of the starting point (18th century); b) Livingstone's geography is mainly the geography of the English-speaking world, spiced with the not-too-recent history of the French and German geographies; almost nothing is said beyond the horizon of these three traditions; c) Livingstone's geography is male-biased. Gillian Rose (1995, page 415) deprecates his strategy 'for the two women he does mention as contributing to half a millennia of western geographies who then end up as no different at all in his account. The question of gender [is] entirely erased'. She is right then in pointing out (page 414) that 'the construction of geographical traditions might better be described as the construction of geography's paternal lines of descent'; d) curiously and outrageously, Livingstone fails to mention an eleventh tradition-that of physical geography!

It is obvious that this could not be reduced to either the tradition of exploration, or to that of culture/nature relations, given that, until recently, most work in physical geography
was very 'natural', disregarding the role of humans in 'changing the face of the earth'. Contrast this erasure with William Pattison's (1990) identification of four geographical traditions: that of spatial science (founding father-Ptolemy), regional geography (founding father-Strabo), culture/nature relations (founding father-Hippocrates), and physical geography (founding father-Aristotle). Whether his choice of founding fathers seems mis-placed nowadays, the idea of a distinctive tradition of physical geography should have concerned Livingstone more than seems to have been the case.

Add to this the fact that Livingstone's delineation of the ten traditions ignores the basic rule of classic Aristotelian logic according to which the categories of a classification should be separated through the constant application of the same criterion. It may be the case that classic logic is obsolete and reinforces certain problematic patterns of thinking (such as the purity of identities, which are not 'allowed' to overlap, the principle of non-contradiction, etc.), as unravelled by much poststructuralist writing (see Derrida, 1997, 2001), but Livingstone is far from being a committed poststructuralist (see his first chapter in 'The geographical tradition' and Livingstone, 2000). In the light of these observations, one would believe that the degree of arbitrariness in his cutting of traditions has been higher than it has been assumed to date (curiously, most critics of Livingstone were concerned with his choice of the 'tradition' model, instead of other possible choices such as the 'genealogical' model). Let us group the ten traditions he identified in the following way (see figure 8).

One can easily notice that, instead of a single criterion, Livingstone actually uses four: A) the attitude to transcendence (some geographers believe in God, some in magic, some do not believe at all, some separate their
Figure 8: Unpacking Livingstone's typology of the traditions of geography
religious beliefs from scientific practice, some mix them, etc.); B) the attitude towards social-political life (some privilege human agency and methodological individualism, some privilege the role of structures, some fight for emancipation, others plead for taking advantage from collaborating with the government and supporting national economic growth, others adopt in-between positions, etc.); C) the assumptions about what constitutes the essence of geography (some say that geography is about space, acting as a counterpart to history's concern for time; others consider that the core of the discipline is the study of regions, or of culture/nature relations at a more general level-geography as 'cross-roads science'; some might say that there is no essence whatsoever, etc.); and D) the beliefs over the most suitable practices to be performed in geography (some say that a geographer is above all an empirical scientist, who spends most of his/her time in the field, grasping the geographical reality; others value the traditional geographical concern for mapping, others consider that these practices go hand in hand and co-constitute the originality of geography, etc.).

Bearing in mind the simultaneous and non-consistent use of four criteria for the crafting of a single ten-fold classification, two observations are important: a) a geographer, according to Livingstone's implied logic, cannot but be simultaneously attached to more than one tradition: any regional geographer has some attitude to transcendence (s)he believes in magic, religion, etc.), to society (s)he might be Marxist, 'imperialist', etc.), and values differently traditional geographical practices (some are 'cabinet' geographers, using frequently maps, others are field-oriented, etc.). This unclear situation makes one believe that the lack of a single criterion in delimiting traditions raises more problems that it solves, and obscures what geography
is about (in the most conservative sense, which refers to science-in-the-abstract); b) one could notice that what Livingstone credits as geography under the third criterion (C) is precisely the set of traditions usually accepted by other historians of the discipline (see Haggett's 1990 'The Geographer's Art'). As it was shown, a big problem remains however: that of physical geography, overlooked and not separately considered by Livingstone.
7. Reconstructions

Between too strong definitions (such as Doel's wishful thinking virtualist geography), and too weak definitions (such as Livingstone's focus on what is a discipline in general, at the expense of not saying what makes geography a distinctive discipline), my pragmatic sceptical definition hopes to be more realistic and 'courageous', as it tries to identify geography's distinctiveness without falling into essentialism. Its conceptual and logical building blocks, partly already revealed, are, in summary, the following (as a reminder, the definition is: 'Geography is a generic name for a set of various scientific practices, loosely held together, and thus identifiable, by a common and long-standing concern for the big themes of "space" and "Earth's complexity", as well as by the networks generated from its distinct position in the academic division of labour'):

1.) **Generic name**: 'generic' points to the fact that, although there is something common to all geographers, there is also a large amount of diversity beyond the label 'geography'. 'Name' is a fruitful way for acknowledging the value of the linguistic turn in the humanities and social sciences - including epistemology and the philosophy of the social sciences, and for alerting the reader to the abusiveness of using the name 'geography' when one actually makes reference to a small part only of what counts as geography;

2.) **Scientific practices** is an expression used for signalling the conceptual shift that underpins the whole definition: that from science-in-the-abstract to the mundane spaces of 'science'. It allows a move beyond the traditional antagonistic approaches to science: on one side epistemology and philosophy of science (concerned with
science-in-the-abstract), on the other side the social studies of science (interested in the cultures and practices of scientific communities). This move 'beyond' is achieved by ignoring altogether the traditional dichotomies between: a) the content and the context of science (in connection with the theorisation of scale as contexts collapsed within the unit of analysis), and b) between science-as-knowledge and science-as-practice. Its metatheoretical relevance resides not only in the fact that, through this manoeuvre, geography is 'honoured' (the concept of space being key in this epistemological project: the mundane spaces of 'science'), but also in the fact that, in turn, this geographical epistemology allows for a better, more comprehensive definition and understanding of geography;

3.) *Set of various* is used to suggest the diversity of geographical schools, and, at a subtler level, to point to the fact that the spatial and conceptual divide between these schools scattered over global space is at least as important and worrying as the gulf between human geography and physical geography. The fact that overwhelming attention is paid only to this latter divide - and see for example Massey, 1999, and Thrift, 2002 - is largely due to the pervasive presence of the imaginary of science-in-the-abstract among geographers: the focus then remains on abstract disciplinary sub-divides and not on the more mundane oceans of ignorance that isolate the various geographical practices scattered over the Earth;

4.) *Loosely held together and thus identifiable* wants to suggest the image of poorly connected geographies and the idea that their identification is a very difficult task. This difficulty arises, *partly* because what holds them together is *not essential*, and is secondary to the cultures, practices, and protocols of enquiry that separates them ('geography' is, therefore, a *loose holding*), and *partly* because the very facts
which hold them together are fuzzy, blurred, vague, unclear, drawing boundaries which overlap, interfere, disappear, are contested and subject to rhetorics of legitimisation, change, sometimes appear as multi-layered, and are seen and traced differently by both those within and those outside it (where the very distinction outside/within is dynamic, contested and troubling). Important in this image is to understand that any metatheoretical work in the mundane spaces of 'science' is uncomfortable, situated, and (in a positive sense) precarious. Instead of identities, the mundane spaces of 'science' involve fluidentities all over.

Be that as it may, given the epistemological sins (essentialism, functionalism, normativity) discussed in the previous subchapter, one has to make a special mention of the claim that what holds together these various geographies is not essential. The stakes involved are considerable: how can one say what geography is, without endorsing an essentialist view? The strategy I developed comprises several approaches, which together allow the conception of geography in a non-essentialist, yet not too weak definition. These approaches refer to:

(i) The concept of mundane spaces of 'science', in which all categories are fluidentities and, therefore, all boundaries are dynamic, unstable, 'fluid', complex, fuzzy. It follows then, that, within this mindset, a definition appears necessarily as an approximation of an unstable 'entity'. Yet, in the geographical epistemology envisaged here this provisional-approximate status is a sign of theoretical strength, and not a failure to capture exactly and uncontroversially the reality 'out-there' (strength precisely because that reality is dynamic, unstable, 'fluid', complex, fuzzy);
(ii) In my definition, ‘geography’ is first and foremost a mere generic name, and not a distinct, unproblematic realm. Further down the chain of signifiers, that generic name stands for what is behind: a set of various scientific practices scattered over space: the fact that I use the plural for what constitutes ‘real’ geography (as a discipline) subverts all temptations to essentialist accounts. Furthermore, the fact that I explicitly specify that the key feature of these scientific practices is that they are various helps to unsettle the comfortable idea of 'finding' (i.e. imposing) essences. The image suggested by the expression 'various scientific practices' is, I hope, that of rather disconnected geographical schools which cover with various 'densities' (in Great Britain there are around 80 geography departments, in Mexico the discipline does not have but a peripheral academic status, etc.) the surface of the earth (and see Dunbar, 2001). This image vigorously helps to interrupt the view of geography as 'totality', with clear 'insides', 'outsides' and rules of internal functioning and organisation. Whereas at a global level the idea of geography as totality is untenable, it can hold, to an extent, at more specific scales of analysis (such as national, regional, departmental), provided that 'totality' is conceptually reworked to avoid its traditional rigidities (see, for example, the reworking advanced by Laclau and Mouffe, who speak of the outside as 'constitutive' to a 'totality' and underline the precarious articulations which allow its identification).

(iii) I emphasise that what I say geography is refers to a necessarily weak epistemic position: the key word

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24 I express my views here, as an academic interested in the study of science. From other standpoints (‘lay people’), geography is just a body of information. On the need to connect the academic and popular understandings of geography see Bonnett, 2003. On popular geographical imaginations, see Schulten, 2001.
in the definition is 'identifiable'. The focus is therefore on an observational 'situatedness' (cf. Haraway, 1991), which allows at its highest potential only a difficult, controversial work of identification, by means of features which can be noticed (but also overlooked). This opens various possibilities for interpretation. Perhaps those features are fundamental, perhaps they are not, but seem fundamental, given their excessive epistemic visibility (which, in turn, might be the outcome of the efficiency of historical processes of re-inscription and successive iterations). Or perhaps the very distinction fundamental/nonfundamental features of 'reality' is untenable. This latter contention could arise, firstly, by adopting a sceptical epistemology (we cannot know for sure what is fundamental and what is not and, therefore, it is better to avoid these terms altogether) and, secondly, by endorsing a more sophisticated ontological perspective (conceptualising fluidentities as actants within a single all-encompassing ontological realm: the question of what is 'fundamental' to an identity is replaced then with the question of what is distinctive for a role or another played by that fluidentity in the web of ontological co-production).

Given these, one can further the strategy for building a non-essentialist definition by drawing on work in epistemology and the philosophy of science, and particularly on Sally Haslanger's (2000) project of a peripatetic metaphysics. She distinguishes, following Aristotle, between natural kinds and objective types (however, the distinction has previously circulated in traditional metaphysics, under various names). Compare and contrast a group of ten tigers, with a group of ten red objects (a cup, an umbrella, a telephone, a pen, a hat, a door, a condom, a book, a car, and a piece of cake). The first is a much stronger grouping than the second: the similarity/sameness of the ten tigers is striking and, although there are differences between
them (each has its 'personality' and distinct genetic history) it is obvious that from our point of view (humans) what is common to all of them (the 'essence' of tigerness) prevails over 'minor' differences between them. In the second case, we have to deal with objective types: the criterion for grouping all those very different things is the fact that all are red: this commonality is helpful in the work of cognitively organising the messiness of reality (see Descartes, 1637). Nevertheless, the same commonality allows a degree of arbitrariness and abusiveness through the overstatement of the distinctiveness of that group. The fact that all those objects are red is useful for grouping them and identifying them, but is not of too much use in characterising those objects. Their redness is a side feature, and a red umbrella has more in common with a blue umbrella than with a red pen.

To be sure, this epistemological device is used in my definition of geography for a number of related purposes: firstly, to acknowledge the big difference among scientific practices labelled 'geography' (as a counterstrategy to the overemphasis on commonalties in almost all previous definitions of geography); secondly, to avoid an essentialist definition, with all its negative consequences (which refer to ethics, epistemology, the history of the discipline, etc.); thirdly, to prevent - fearing essentialism- the lapse into a too weak definition, as a solution to this fear (describing, for example, only what makes geography a discipline, without mentioning its difference from other disciplines and its difference from itself); fourth, to draw attention to the epistemic situatedness of my views (I do not say anything of 'geography', except that it is a name for various scientific practices; in turn these scientific practices are not characterised in a strong ontological sense - i.e. what they
are, but in a modest perspectival sense - i.e. how they might be identifiable).

(iv) A further approach to the overall strategy of defining refers to the double way of identifying geography: on the one hand, there is a 'common and long-standing concern for the big themes of "space" and "Earth's complexity", on the other hand there are 'the networks generated by its distinct position in the academic division of labour'. At first glance, these seem to constitute an assuring way of avoiding criticisms from both epistemologists and scholars concerned with social studies of science. The identification by the two big themes belongs, one would say, to working within science-as-knowledge, while the identification by disciplinary networks would belong to working within science-as-practice (I use here Pickering's already criticised distinction; 1992). This criticism does not hold, however, since the concept of mundane spaces of 'science' presupposes the dismissal of this very distinction. In the particular case of geography, one can illustrate quite clearly the undoing of this dichotomy. A useful tool in this regard is to conceptualise in a double regime the vague, free-floating term of 'big theme'.

Its first meaning refers to its linguistic-rhetorical dimension. 'Big theme' would be then a cluster of concepts, concept at its turn theorised in Agnew, Livingstone, Rogers, eds., (1996). However, as I shall explain, it is useful to simplify this first meaning of connotations related to 'concepts' and 'discourses', as they would introduce further levels of ambiguity (like those in Agnew-Livingstone-Rogers, (1996): if we take 'space' or 'region' as concepts, then there is room for too much theorising - misleading at this stage, I believe - of what is the concept of 'space' or 'region' and what constitutes a spatial discourse or a regional discourse within the rhetoric of geography). Instead, the first
meaning I give to 'big theme' is simply that of set of signifiers. What is signified by each signifier, and the relation signifier-signified-referent is secondary in importance (the only significant thing is that the set of signifiers as a whole has a certain correspondence to the set of 'signified' referents; what exactly each signifier denotes is irrelevant for the debate) to another relation, that between the signifiers in themselves (from within the same big theme, but also between different themes).

The second meaning of 'big theme' refers to its role as legitimating referent. This meaning accounts for the historical becoming of geography differently. All histories imply a considerable amount of artificial 'cuttings' and the author of these lines acknowledges this by underlining that his model works as a hypothesis, as a way in which we might think of the evolution of the discipline. The starting point of this model is the definition provided, which outlines that geography is an historically constituted, evolving set of scientific practices. The opening towards an exploration of its becoming is a useful exercise, since it 'tests' the potential of the definition, the extent to which the reduction of all histories and all definitions of geography to an underlying theory of it is really achievable, and the ways in which this definition could stand as a central theoretical device within a refigured paratextual geography.

This model of historical unfolding, unlike some of the influential perspectives on geography's history, which have insisted on the richness and diversity of the discipline (see, for example, Livingstone, 1992, Johnston 1997, Peet, 1998) outlines the commonalties of various geographical practices and what has distinguished them from the disciplinary neighbourhood. Note that the metaphor underpinning the aforementioned influential accounts has usually been that of spectres/lineages which have evolved distinctly, although
interacting with the others, the final result being a sort of a fabric the strength of which would stand in the great number of threads (Marxism, humanistic geography, quantitative geography, etc) and in their combination within the geographical tradition.

Unlike them, my model is more interested in the colour of that fabric, than in the different worldviews, philosophies, and practices which constitute the criteria for delineating traditions/lineages/paradigms. I believe their model is misleading in that it supports the norm of intellectual purity (with some allowed hybrids), when in reality what we have in the mundane games of geography is nothing but 'promiscuous theoretical impurity'. Moreover, these models outline the influence of particular 'worldviews' (structuration theory, phenomenology, Marxism) on the discourse of geography, whilst I am more interested in the influence of the wording (signifiers) of geography on the different worldviews.

At a more general level, in attempting to explain how words create, and are created by, worlds, my model on the becoming of geography displaces the entrenched dichotomy between epistemology (ideas as produced by humans) and social studies of science (humans as producers of ideas) through the conceptualisation of a mundane space of 'science' (which also renders obsolete the distinction content of science/social context).

This latter ambition makes it necessary to conceive of the two 'big themes' of geography as being simultaneously both sets of signifiers and legitimating referents. The practices of geography, seen in their historical unfolding, have been done by invoking the two big themes as grounds for a distinct discipline concerned with 'space' and 'Earth's complexity'. In turn, these practices served to reinforce and legitimise the very referents by giving them 'substance'/content' and by
creating a *history* (which is a queenly way for acquiring respectability and legitimacy). In all these interactions and co-productions, the search for legitimacy plays a central role and its analysis is fundamental for the understanding of any disciplinary history (note that the *discursive power* of a discipline depends on the success of this search for legitimacy).

How was the concept of 'big theme' chosen as the most satisfactory means for defining a discipline, and how come that for geography it is *necessary and sufficient* to delineate only two big themes? The choice of 'big theme' resulted from a two-fold consideration: *first,* it matches the general politics of the definition provided, namely that of deliberately promoting a sense of vagueness, lack of precision/fixed boundaries, lack of certainty. Needless to say, this politics of unfixity helps to better capture the significance of a set of scientific practices (called 'geography') which are themselves lacking exact boundaries (temporal, spatial, conceptual, etc.). *Second,* the term was coined to encompass two related dimensions: 1) as a set of signifiers (which is not the same as a 'cluster of concepts') and 2) as legitimating referents. The interplay between these two dimensions translates the dynamic between the *less-than-discursive* of a scientific practice (signifiers as not-yet-discourses) and the *more-than-discursive* of a scientific practice (matters of power, status, resources).

As for the 'finding' of only two big themes, at stake has been the problem of commonality across rather different geographical communities. Everything which is 'covered' by *at least* one big theme belongs to geography, as long as it also fulfils the *second* criterion set in the definition (geography can also be identified by the networks generated by its having a distinct position in the academic division of labour).
This way of defining captures together, I believe, (almost/more than?) all of 'geography': a) human geography, physical geography, in-between geographies, b) 'established' Western geographical schools and remote/marginal schools, c) geography in the past and geography in the present; d) 'paratextual geography' (which is the discourses of what geography is/should be) and 'substantive geography' (which refers to the 'real' geographical work done by geomorphologists, economic geographers, GIS specialists, etc.). In speaking in terms of these two themes, the attention focuses on 'identifying' the external boundaries of geography, its difference from other disciplines, and not on capturing its internal variations. Again, the search was for commonality (the 'red colour' which makes geography an 'objective (distinct) type'). Given these model specifications, it is easy then to understand why a third possible theme - that of the region - was not delineated as a distinct theme, but melted within the other two (see figure 9). Indeed, all the signifiers/concepts associated with 'region' (e.g. place, locality, locale) belong simultaneously to the theme of space and to that of Earth's complexity. 'Region' has meant in geography: a) a certain space which b) has some unique features produced through the complex interaction of its geographical elements (rivers, climate, human practices, geomorphic landscape, etc.).

Unlike the case of the 'region', one cannot reduce the big themes of 'space' and 'Earth's complexity' (culture/nature interactions) to one another. Although Richard Peet in his 'Modern Geographical Thought' (1998) has thoroughly criticised this position (he argues that geography has always been about culture/nature relations, 'space' being constitutive, and therefore, inseparable from that theme), I appreciate (and so do most theorists of the discipline- Hartshorne, Haggett, Livingstone, Johnston, Claval,
External boundaries of geography

Internal boundaries between 'big themes'

All boundaries are relative & fuzzy (not only epistemologically - i.e. difficult to be seen, but also ontologically - i.e. disciplines are 'fluidentities').

Figure 9: The two big themes of geography
Mihăilescu, etc.) that the argument for space as a distinct theme of geography is consistent. Indeed, Peet wrongly assumes that from the fact that any discussion of culture/nature relations inevitably has to consider space, it necessarily follows that, at the level of signifiers, concepts, and discourses, the theoretical working of the two themes would also be inevitably melted into one another. But there is a distinct spatial rhetoric in geography. Compare, for example, the English-speaking world's geography in the first half of the 20th century -dominated by regional geography and environmental geography, with the human geography of the late 1960s and early 1970s, remarkable for the then new rhetoric of space-locational analysis, spatial behaviour, distance, etc.

A further problem in the selection of only two themes, is the challenge related to the alleged fundamental difference between space and place, which, if true, would render unfair the bringing together of space and place within a singular spatial theme. Illustrative of this position is Michael Curry (1996, page 99) who warns about the dangers of confounding space with place:

> What makes it difficult to see the importance of places is the propensity to confuse place and space. It may appear something of a shock for me to say that the two are in fundamental respects different, yet it is both true and important.

However, despite this urge, the fact that place is, ultimately, a special type of (particularised) space, is sufficient ground for not separating a distinct theme of 'place' (to support this claim, see also Thrift's, 2003, typology of space and Thrift's older remark, 1999a, that 'place is a means for producing space'). 'Place', in the same way as 'region', belongs to both themes, in that it is a type of space, yet a complex, 'unique' kind of space (cf. Tuan, 1977, Casey, 1997).
Shifting attention to the other big theme, we preferred the name 'Earth's complexity' to the much more frequently used 'Culture/Nature relations', 'Humans' relations with their environments', 'Earth as the home of people', in order to avoid the danger of excluding from the definition much of what is done in physical geography. Indeed, whilst it is true that, after the second world war, physical geography has increasingly considered 'humans' among the factors which cause changes to the Earth, this does not account for:

a) The fact that physical geography is not about culture/nature relations, but about relations between various geographical elements, among which 'culture' is just one such simple element. The epistemic stake of this detail unravels easier in figure 10;

b) The fact that in the past, when the study of the human impact on the Earth was not so socially rewarded as it is nowadays (see the recent rhetoric of environmental concern that legitimises the funding agendas of organisations such as the NERC in the UK), physical geography tended to disregard the role of humans, focusing almost exclusively on the non-human (cf. Gregory K J, 2000). This tendency was also cognitively secured by the pervasive worldview of a layered reality, in which humans occupy a distinct level, between the level of transcendence, and the level of the 'less-than-humans' (with a further separation of the biological from the mineral, the latter being pushed down, to the lowest level). Suffice to say here that its roots are back in Ancient Greece, and not, as Latour would have it (1993), in the dawns of 'modernity'.

The naming of the second big theme as 'Earth's complexity' captures both a global vision of geography as the study of 'culture/nature' relations, and the legitimating idea of physical geography: even if the Earth had been without
humans, it would still have been very complex when compared to other planets, because of the unique mixture of life (plants, animals), air (the specific chemical composition and physical stratification of the atmosphere), water, soil, landforms. In order to delineate the distinctiveness of physical geography from more analytical sciences, the usual argument is that physical geographers (K. J. Gregory, 2000) undertake the relational study of all these elements, the ways in which they determine each other, etc.

In sum, 'Earth's complexity' seems the most suitable term for capturing the diversity of practices grounded on the epistemological perspectives suggested in figure 10.

But how can one 'find' that geography is about these two themes? There is an abusiveness in putting the problem in these terms, for my model is meant to be a good account/approximation of what geography has been, and my epistemological scepticism does not allow me to speak in terms of 'finding the truth'.

The answer, then, is manifold: a) we considered the considerable amount of previous work in the history and theory of geography in different geographical traditions (a general review is in Simandan, 2002); b) we analysed definitions provided for geography in various periods and locations; c) we identified those perspectives on the discipline which have been highly influential, not only at the level of paratextual geography, but also for the doing of substantive geography (for instance, Ratzel and Hettner in Germany, Vidal de la Blache and Claval in France, Sauer and Hartshorne in the United States, Mehedinti and Mihăilescu in Romania, etc.); d) we considered accounts (studies, observations) of what non-geographers (scientists, common people, etc.) and geographers who work in substantive geography (as opposed to paratextual
Figure 10: Alternative analytical strategies in physical and environmental geographies
Legend figure 10:

A-environmental geography;
B-general physical geography;
C-a branch of physical geography (hydrology);

A + C = autecological perspectives;
B = synecological (endostructural) perspective.
geography) believe geography is (historians say that history is about time, geography about space; geologists say that geology studies the inside of the Earth, whilst geography studies its surface, or, more sophisticatedly, that geology starts with the present Earth to explain the past earths, whilst geography uses the knowledge of past earths to explain the present Earth; general school pupils define geography as the study of countries, etc.; other people say that geography is about mountains, rivers, clouds - i.e. mainly physical geography, etc.); e) I reflected upon my own experience of trying to find out what is common to various geographical texts (e.g.-an article in geomorphology, with a paper in cultural geography) and practices (e.g. in Romania, between various geographic schools; in England, etc.); and f) we analysed some classifications of sciences which mention geography (for it has to be said that most classifications of sciences done by non-geographers overlook geography). The guiding rule has been that my definition should be sufficiently general that it can include all the very specific/biased definitions of geography. As an illustration, I will mention only three definitions (cf. Simandan, 2002):

− Robert E. Dickinson (1969): 'Geography is fundamentally the *regional* or chorological science of the surface of the Earth';
− Immanuel Kant, (1780): Geography is 'a synoptic discipline synthesising findings of other sciences through the concept of *Raum* (area or space)';
− Harlan Barrows, (1923): Geography is 'the study of human ecology, adjustment of man to natural surroundings';

One can easily notice that the first definition is covered by the two themes simultaneously, the second is reducible to the theme of 'space', and the third is reducible to the theme of 'Earth's complexity'. The inclusionary attitude of my
definition is due, partly, to the fact that it aims to be 'descriptive' and not 'normative' (though this distinction is problematic), and partly, to the fact that it does not rely on essentialist assumptions (therefore, the focus is not on the identification of a core of the discipline, but on the tracing of inclusionary boundaries to geography as a 'whole').

To be sure, the two big themes are open lists of signifiers: their unique high frequency in the texts and conversations of geography grounds the identification of those texts and conversations as 'geographical'. In table 1, we exemplify the sets of signifiers performing the two themes.

In what follows, we will present a series of insightful metaphors (cf. Buttner, 1993) that suggest how these two big themes might have worked over time to shape the culture and practice of geography.

1. The two big themes have worked as 'lips speaking together' (Luce Irigaray), articulating the geographical discourse. Both themes have a history as long as that of geography, being advocated by the geographers of Ancient Greece (Erathostene and Ptolemy within the spatial theme, Strabo within the complexity theme, etc.). However, their relation is not as much of harmony as it is of tension and competition for playing the role of 'the core' of geography.

On the one hand, the 'space' theme has usually been associated with a certain degree of abstraction and sophistication: from the Ancient art of mapping and the development of the spatial science of the 1960s, to the theorisation of space and space production in contemporary critical human geography. The 'complexity' theme has fuelled a more empirical geography, grounded on fieldwork, on how the real elements of the world out-there (rivers, mountains, humans) interact. David Stoddart's (1987) urge to do 'some real geography' was a reaction to the relative neglect of this theme, starting with the years of the
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Table 1: The two big themes as sets of signifiers
Quantitative Revolution. On the other hand, the tension between a theoretically sophisticated 'space' tradition and a theoretically naïve 'complexity' tradition is not general in space and time. In Romania, for instance, geography is clearly dominated by the 'complexity' theme, and, whereas 'space' is indeed invoked as a geographical issue, its actual discursive use is quite naïve: instead of the theoretical sophistication from the English-speaking world's geography, there space is usually understood as a 'container' in which things are located. Furthermore, the 'complexity' theme has become lately quite sophisticated: compare, for example, environmental geography nowadays (with work drawing inspiration from Haraway, Latour, Ingold, Escobar), with that of the 1960s (natural modelling); also compare the shift in physical geography from empirical approaches to sophisticated mathematical approaches (such as numerical modelling).

In short, geographers have unevenly exploited the two themes in various times and locations, according to their potential to legitimate the practice of our discipline. In the 19th century and the beginning of the 20th, the idea of a crossroads science meant that holding culture and nature together in a single explanatory narrative was acceptable. In the 1950s, given the changed political and scientific context, many geographers felt a crisis of legitimacy in their discipline (Johnston, 2002a-b) and found it more fruitful to explore the legitimating potential of the 'space' theme. Although differently prioritised, the two themes have substantiated the content of geography all along its history. None of the two themes has completely replaced the other: they have 'spoken together' ever since the word 'geography' was coined and the first theorisations came into being in the geographies of the Ancient Times.
2. In focusing on the two themes as sets of signifiers, Derrida's theorisation (1981, 1997) of *différance, trace, inscriptions, iterations, slippages of meaning* and *chains of signification* is also insightful. Geographical artefacts have to bear the inscription with geographical signifiers if it is for them to belong to the sphere of geography and this rule becomes even stronger as time goes by, as the history of its application increases (path dependency; see also our observations regarding the ontological agency of legacies within *recursive cartographies*). Thus, the production of geographies is a matter of re-inscriptions and iterations of what has passed to be 'geography' (a sociological text has good chances of being published in a geographical journal, as long as it pretends to be geographical and *covers* this claim with the abundant use of 'space' and 'complexity' signifiers). The slippages of meaning within the same set of signifiers and, accordingly, the chains of signification generated, help the understanding of how these signifiers have been mobilised into a *discourse*. In a humorous way, the signifiers of the two themes appear to be both *dry* (they seem to tell nothing, but remain useful in avoiding a traditional account of geography's history; an account based on violent separations between alleged worldviews or paradigms) and *juicy* (the meaning mobilised in the geographical discourses is produced through the intercourse of signifiers; these acts of mobilisation upgrade 'mere' signifiers to the rank of geographical concepts).

3. Slippages of meaning link in various degrees the two big themes. To suggest the nature of these connections, I use Marcus Doel's notion of *fluidentity*. The two big themes interfere considerably, such as in the already analysed cases of the 'region' or 'place'. However, 'fluidentity' refers not only to a non-pointillist ontology and logic, but also to the historicity of the two themes (thus dovetailing with Derrida's
differance), to their shape-changing over time (some terms, like landschaft, fall into oblivion, others, such as Gregory's 'geographical imaginations', Thrift's 'spatial formations', or Whatmore's 'hybrid geographies' enrich the themes with new 'tools');

4. One should also think of these two themes as Baudrillard does in his 1996's 'The System of Objects', where he distinguishes between traditional colours and functional colours. The value of a colour does not depend on itself, its intrinsic value, but is continuously constructed as relative to the other colours, as difference from them: 'colours obey no principle but that of their own interaction' (op.cit., page 34; this reminds of Saussure's, 1972, view of language as a system of differences without positive terms).

This parallel is useful for grasping the significance of the two big themes as legitimating referents. Indeed, disciplinary legitimacy is not acquired through a primordial ontological allocation, as many still believe (i.e. geography matters because place and space matters!-the idea underpinning this naiveté is that when disciplines came into being, they were allotted a certain portion of reality: history-the past, geography-the present Earth, medicine-the human body, etc.; then, the argument would go, because that portion of reality is important, the discipline studying it is also important…), but through a continual fight with theoretical and non-theoretical resources, for acceptance, prestige, academic territory, 'scientificity', etc.

Geography is not valuable as such, its (non-)value is systematically produced in relational terms, as difference to…. and as 'the difference geography makes'. In turn, this process of legitimisation (co-produced with the building of disciplinary 'self-esteem' in ways which blur the inside/outside divide) develops even between the various geographies differently located in the mundane spaces of
'science'. To give an example, in Romanian geography, a paper is much more appreciated if it has extensive references from the big, prestigious geographical traditions (French, English, German). The latter act as shrines of theoretical validation for this minor geographical school which -given the reduced circulation of the local language- systematically runs the risks of parochialism. Another example would be from within the English school: a recent call for papers for a session in Population Geography urged the participants to explore the ways in which population geography could become more theoretically-informed, in order to reduce the gap separating this 'dusty' specialism from the (at the moment) prestigious cultural geography.

5. An alternative metaphor refers to Deleuze and Guattari's *folds* (1983, 1988), since, in their vision: a) the smallest dimension is not the point ('closed'), but the fold ('open'), and b) folds are always full. In this light, the two themes would appear as series of folds, as openings which generate the new;

6. The metaphor of folds, as well as that of 'juicy' signifiers (signifiers as 'sources') can be connected with two other metaphors, which marvellously suggest the *noncontradiction of sameness and difference*, the way in which the 'new' constitutes its 'old' and vice-versa. The first of these metaphors is Jean François Lyotard's depiction of the process of *cancerization*, as *difference producing repetition*: an initial cell reproduces itself to conquer a whole body (Doel, 1999). In the case of geography, the two big themes have been reproduced constantly at the discursive level by the practitioners of the discipline, who justify themselves as geographers through the invocation (iteration) of the signifiers of the two themes. In a sense, the practice of geography changes over time (e.g. nowadays we conceptualise space differently than fifty years ago), yet, it
remains the same, since the change is forged with the old tools (e.g. we still 'waste time' with theorising what space is - this legacy of wasted time builds into a history of conceptualisation that fleshes out the signifiers of the 'spatial' theme into meaningful disciplinary discourses).

This idea is re-captured in the second metaphor, that of difference-producing repetition (Doel, 1999): in using the same set of signifiers (of 'space' and 'complexity'), geographers are constantly 'establishing' their discipline in a certain academic niche, defended both discursively and institutionally. 'Space', 'region', 'environment', 'place', 'landscape' have been for a very long period key concepts in geography. We cannot do without them. We use them nowadays in different ways than hitherto, but we still use them (re-inscription and iteration). In so doing, the scientific practices called 'geography' are keeping their connection with the past and generate a tradition of research, with all the legitimacy conferred by having a history. Nevertheless, the meaning ascribed to different signifiers changes, and by iterating a signifier we produce new concepts or completely new approaches to older concepts. The repetition becomes, then, a producer of difference. To push the connection even further, it can be argued that the basic condition for the production of difference is a certain repetition (i.e. to make new scientific developments within geography, one has to be sure that geography is maintained/reproduced through the invocation of its big themes, which act as discursive legitimators).

Seeing things from the other end of the chain, it becomes apparent that the condition for repetition to occur is the production of difference (i.e. geography has to prove that it is useful and relevant, that it generates progress (cf. Bassett, 1999), otherwise, the risk to be dismantled comes true- thus, most accounts of the Quantitative Revolution insist on the
perceived lack of substantial progress within the discipline at the time and on the imperative to create something different, so as to safeguard the repetition of geography (Johnston, 1997, 2002a-b).

To sum up, my model put to work two interplays, to produce a nonessentialist and 'nonmetaphysical' (cf. Livingstone, 1992, who said that the idea of a metaphysical core of geography has to go) account of what geography is and of how it has come into being: the first interplay has been between the two dimensions of 'big theme' (a-set of signifiers; b-legitimating referents), the second interplay has been between the two opposing perspectives on the relation difference/repetition (a-difference producing repetition; b-difference-producing repetition);

7. Whereas Clifford Geertz (1973, 1976, 1980, 1983) compared scientists with the members of a tribe, I will go further and compare them with animals. Those who are outraged should remember Sarah Whatmore’s ‘Dissecting the autonomous self: hybrid cartographies for a relational ethics’ (1997) where she advocates the extension of ethical considerability to the nonhuman realm. My comparison is simply about the ways in which animals defend their territory—for example by rubbing their bodies against trees and eliminating certain substances. For it is in a similar way that I see the process of marking one’s texts and discourses with the proper signifiers. What is fascinating in both cases (scientists and animals) is that from almost similar initial conditions (undifferentiated territories), by using some apparently superficial inscriptions (smells-substances/signifiers), striking differences are subsequently created (the territory of a community; the ‘place’ of a discipline). At this point, my outrageous comparison

25 For a detailed analysis of human territoriality see also Sack, 1986.
encounters a more general and academically sanctioned model, namely materialist semiotics (cf. Haraway, 2000);

8. Michel Serres' materialist semiotics refers to the processes through which ideas/words exceed the abstract space in which they were initially conceived and actually shape the world 'out-there', creating the very reality that they apparently just describe. Let us now distinguish, for heuristic purposes, three basic types of geographers: a) romantic geographers, interested in the fate of their discipline, working always within its boundaries and cultivating the narrative of geography as -at least potentially- queenly science; b) web-master geographers, who do not necessarily love exclusively their discipline, but are interested in the reproduction and enhancement of its network (given the fact that this is the source of their powerful privileged positions); and c) careless geographers - which include those who have studied geography but now have left it, and those who still practice it but identify themselves as ‘social scientists’/ ‘hard scientists’ rather than ‘geographers’ (even if they work in geography departments). The romantic narrative of a besieged discipline, threatened by recent challenges (interdisciplinarity, disciplinary imperialism, imperatives of 'public relevance', funding issues) does not impress this last category too much.

The perpetuation of geography's institutional network (and of its scientific practices) essentially depends on the 'centripetal' work of the first two categories, who coerce most of those in the third category to obey the rules of the game of geography, if they want to enjoy the advantages associated with being a geographer (institutional prestige, academic life, income, etc.). Although with different voices, they all have to speak the same language - that of the 'two lips speaking together'- 'space' and 'Earth's complexity', which mark the geographical territory. However, since the
two big themes connote particularly the discursive dimension of a scientific practice, in what follows, I am interested in the materialist semiotic processes through which paratextual geography insinuates itself in the corpus of substantive geography, in the transmogrifications between 'discourses about geography' and 'the actual doing of geography' and in the ways in which signifiers work not only to delimit geography from its others, but also to create difference/theoretical progress within geography, to create the so-called geographical traditions / schools of thought.

The insistence on this double work of differentiation (a) difference from geography's others, and b) difference of geography from itself and within itself) helps, I hope, to outline that a discipline operates not so much at the discursive level, or at the practical/nondiscursive level, as it does between them. This 'go-between' illustrates the relational thinking of recursive cartographies, as it presupposes a re-focusing of our attention from 'entities' to the flows which produce different 'entities' (e.g. the discursive, the non-discursive -both fluidentities) and to the regulators of these flows (the politics of academe). If this is so, than the distinction science-as-knowledge/science-as-practice is not of too much need; indeed, that disciplinary go-between becomes much more visible in the mundane spaces of 'science'.

To ground the previous observations, one could start by noting that a powerful idea or research program has considerable chances to cause a destabilisation and reshaping of the network of a discipline (in the vocabulary of recursive cartographies, we have an event that traumatises the fabric of disciplinary legacies and rhythms). Ideas might come 'true', and in the case of geography, this refers to the relation between a research manifesto (encompassing frequently a normative definition of geography) and the
practice of geography, including here the education required to become a geographer. The key web-masters of the discipline, who have the power to make their dreams come true, act as regulators of this kind of relation. To avoid misunderstandings, their access to top institutional positions results partly from the brilliance of their research programs and their list of scientific achievements and constitutes a sign of recognition of their excellence and a normal application of the punishment/reward mechanism within a social network. Thus, one might think of Simion Mehedinti and Vintilă Mihăilescu in Romania; Paul Vidal de la Blanche, Emmanuel de Martonne, Paul Claval, Roger Brunet in France; Karl Ritter, Friedrich Ratzel, Alfred Hettner in Germany; Herbertson, Halford Mackinder, Peter Haggett, Richard Chorley in Great Britain; William Morris Davis, Carl Sauer, Richard Hartshorne in the United States, etc26.

This institutional position does not necessarily refer to the whole network of a national school, although in the past there were precisely these key positions which helped the shaping of a 'national school' according to the webmaster's philosophy of geography. Nowadays, the diversification and segmentation of the discipline have transformed these institutional positions, containing their spheres of influence to limited segments within a discipline. Let us say that professor X, an established figure in his/her field, arrives to be the head of a geography department. From this position, (s)he has the power to shape a 'school' around his research program, through certain mechanisms (I have in mind a Romanian example; in the English speaking world, I came across cases where the powers of a departmental head were much more confined):

26 David Harvey is yet another example, although he cannot be associated with only one country.
a) The number of applicants for research studentships is far greater than the final number of those admitted. The patronage of a head of the department in favour of those students who will work with him/her, developing his/her research program is influential in the final selection of those who are awarded scholarships, whilst applicants supported by regular staff tend to be disadvantaged. Accordingly, through research, seminar presentations, papers, pub chat, those research students tend to spread the program of their master, to make his/her ideas permeate the geographical conversation;

b) The selection of academic staff is another means for grouping people with similar worldviews and styles of research; if the head is a Marxist, (s)he will tend to favour Marxist lecturers, if (s)he is postmodernist, preference is likely to be higher for postmodernists, etc.;

c) The making of the undergraduate and graduate curricula allows the leader to eliminate or diminish the training in fields which are not of interest to him/her, and to boost the courses which are 'relevant' for him/her. To give an example, the replacement, in the winter of 2000, of the head of a department in Romania, was soon felt in the curricula: the new head is a regional geographer, and in the summer of 2000, months after his election, he introduced an MSc in Regional Geography. In addition, because of financial constraints, the number of staff was kept constant with the exception of a new position in the research group of …regional geography. The shaping of the curricula is an important mechanism through which a normative vision of geography transmogrifies the 'body' and practice of the discipline.

Note that all accounts of geography (definition, history, purpose, methodology, core-periphery relations, good 'geographical' things / bad things) discussed in the education
of future geographers tend to be biased: although the opposite point of view to the one endorsed by the professor might be generously mentioned, its mentioning is not 'supported' rhetorically and affectively in the ways it would be supported by those who actually authored that opposite point of view. The subtle manipulation through the apparently neutral: 'I've told you both points of view; it is up to you to decide which you prefer, but, as I said, I would go for the first...' has long-term effects, for it traces the specific difference of the regimes in which geographers are produced. No wonder then, that the courses which are meant to offer the big picture of the discipline ('History of Geographical Thought', 'The Nature of Geography', 'General Geography', etc.), given their effectiveness in constructing the desirable educational product ('the geographer', or rather 'the ideal geographer'-all curricula are underpinned by a philosophy of what that ideal should be), are frequently taught not by young staff, specialised in those metatheoretical issues, but by the webmasters themselves. The connections are clear: those well appreciated in the geographical community tend to achieve strategic positions within the network, which in turn are used as means for increasing their influence, through one of the most effective sources of power: the dissemination of their ideas and normative positions so that they enter into the tacit knowledge which holds together a certain school of geography (e.g., it is frequent both in France and Romania for the geography textbooks to be written or at least supervised by the most prestigious geographers of those national schools: in this way, their ideas arrive to shape the understanding of geography at the national level and not only among geographers, but also among all those who study geography in the general school);
d) Another means for regulating the functioning and development of a network is by controlling the main academic journals, since beyond 'technical' work, editorial boards also impose a strategy/philosophy of what is desirable to publish and what is not. If one considers only the present situation of geography's networks, (s)he might not realise how effective the mechanisms described had been in the past, particularly in those cases when the network was built on a 'virgin' land. As a case study, I will pick the example of Romania. Until 1900, there were no professional geographers there, although since 1875 a Royal Geographical Society had been functioning (its members being geologists, economists, businesspersons, etc.). The society funded a student from the University of Bucharest, who previously studied philosophy and law, to go into Western Europe to be trained to become a specialist in geography. This student, named Simion Mehedinti, after studying in Paris with Vidal de la Blache, and in Germany with Ferdinand Von Riehtoffen (in Berlin) and Friedrich Ratzel (in Leipzig), returned to Romania where he was offered, in 1900, at the University of Bucharest, the first chair in geography in the country. From that position, he controlled the shaping of the Romanian geographical tradition for half a century, his influence being so decisive that one might say even nowadays that Romanian geography is Mehedinti's geography (for example, he endorsed the view of geography as a 'hard' science, and even now physical geography is at the core of the discipline in Romania, whilst human geography is seen all to frequently as a dubious 'sociologised' geography). The next chair was founded in 1904, at the University of Iasi, but the professor there was appointed with the approval of Mehedinti; the third chair was established in 1919, at the University of Cluj-Napoca,
again with the supervision of Mehedinti. It is worth noting that Mehedinti had scant concern for substantive geography, almost all his work being on paratextual and 'general' geography. Until the publication of Vintilă Mihăilescu's, 'Theoretical Geography' (1968), Mehedinti enjoyed the privilege of being the only geographer in Romania with a powerful philosophy of the discipline (1900-1968).

Although this example is marginal and certainly represents the exception for the history of geography, I believe it offers an accurate perspective on how a discursive body (Mehedinti' ideas) enters 'reality' and produces geographers and geographical practices and identities. It also grounds my methodological approach of identifying the big themes of 'space' and 'complexity' (partly) through comparing the various definitions of geography advanced by the key figures in the history of the discipline. This strategy is not elitist or irrelevant, since these key figures actually put their views 'to work', in the shaping of the various scientific practices labelled 'geography.'
8. Conjectures

In the definition proposed at the beginning of the second part of this book, two criteria underwrote the identification of geography: firstly, its common and long-standing concern for the big themes of 'space' and 'Earth's complexity'; secondly, the networks generated through its having a distinct position in the academic division of labour. We have already explained the materialist semiotics which works in the creation and reproduction of geography and this explanation makes it clear now that the 'mundane spaces of "science"' do not presuppose a privileging of scientists (social studies of science) over ideas (epistemology), or vice-versa, but instead maps them as 'recursive', i.e. they are the outcomes of the relation that they seem to generate, thus rendering not-too-relevant the discursive/nondiscursive divide.

It becomes clear then, that the setting of the two criteria for the 'identification' of the discipline is not just a fair trade-off between internalism and externalism, but an epistemic tool meant to ensure the inclusionary character of this definition. In order for something to be called 'geography', it is both necessary and sufficient that simply one of the two criteria is fulfilled. To be sure, a text that debates issues of 'space', is not defined as geography if its author explicitly states that it is, for example, sociology. The rule is that all 'things' which are inscribed with the signifiers of 'space' and/or 'complexity' belong in this (inclusionary) geography, if they clearly do not 'claim' to belong to another discipline. Put it simply, one would label as 'geographical' those 'things' that either are 'self-identifying' themselves as 'geography', or do not claim
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any disciplinary belonging, but are inscribed with one of the two big themes. This way of seeing the setting of boundaries justifies the understandable, yet problematic, tendency of historians of the discipline to include some figures, like those of the great explorers (Christopher Columbus, Magellan) in the story of geography.

From the perspective of the second criterion (the academic networks geographical practices generate), there is also a certain independence from the first criterion: in other words, any 'thing' which self-identifies as 'geography', although it is not inscribed with either of the two big themes, belongs however to 'geography'. The boundary - tracing rule would be: everything which claims to be 'geography' is geography, everything which claims to belong to a discipline other than geography is NOT geography, everything which does not claim a disciplinary belonging, but is inscribed with at least one of the big themes, might be invoked to be 'geography'.

This last criterion is particularly useful, given recent disciplinary history. One thinks of the trend within social sciences to explore their commonalties; a trend which, together with the low prestige associated with the label 'geography', explains why many human geographers write papers which are meant to be relevant for the whole body of social sciences (and therefore avoid the word 'geography' within the title and the text, to signal the 'universality' of the paper across the whole spectrum of the social sciences). Sometimes, a too frequent use of geographical signifiers condemn one's work to circulate only within this disciplinary community, although, as a 'reward' for the concern for the discipline, its author might obtain easier influential positions within its network.

Even if one ignores the criterion of the institutional networks for distinguishing disciplines from one another, the two big themes of geography clearly do the task, as they underline
with decent precision (i.e. disciplinary overlapping is normal; it is more and more common for a topic to be 'researched' by competing disciplines) the specific difference of geography from its others, particularly from the perceived disciplinary neighbourhood. It needs underlining here that there is no such thing as neighbour disciplines and remote disciplines. The very word 'neighbour' reproduces the fantasy of science-in-the-abstract (see figure 6).

All disciplines are potential neighbours and the dynamic of any discipline makes a compelling case for the idea that apparently 'remote disciplines' can become close neighbours. However, in what follows, we use the idea of disciplinary neighbours as a heuristic device that helps clarifying how and why the two big themes of geography are capable of delineating the domain of this discipline in a way that is both inclusionary/generous and fairly precise. Let us then consider the paratextual discourse of some of the disciplines that seem 'natural' neighbours of geography.

**Geology**, for example, has traditionally meant the study of Earth's past, having as a starting point the present. The big theme 'space' has never been central to the discipline, and 'complexity' might be limited only to 'physical' complexity, omitting the central part of it -culture/nature relations. In short, geology does not have the 'social' and 'spatial' dimensions geography has.

**Sociology** lacks completely the 'physical science' components of geography. It has never placed 'space' at the core of its discourse and 'complexity' is limited to the complexity of social relations (even in some work in environmental sociology).

**Ecology** has traditionally been limited to the 'nonhuman', geography being sometimes defined as its complement, as 'human ecology' (e.g. Harlan Barrows, 1923). However, nowadays ecology also studies humans in their
'surroundings', but what is striking is that humans are approached biologically, as superior animals (a naturalist conception of society); a fact that delineates an important difference to geography (which is increasingly 'infested' with the virus of social theory). Also, in ecology, 'space' is not a central topic of study and theorisation, and the space that interests ecologists is much more limited (mostly, the surface of the Earth) than that of geography (which, through climatology and geomorphology, has quite large dimensions).

Philosophy has always been, paradoxically, both narrower and broader than geography. Narrower in that it has always taken a distance from the 'real', 'substantive' things which constitute the world, retreating into pure theory. Thus, it has often been the case that its work of theorisation has been done with no direct reference to 'reality', but with concern for the proper rules of theorisation (how one statement can be deduced from other statements with truth-value, etc. In other words, the abstract system built by this 'discipline' tends to be closed, self-sufficient, 'pure', like in the case of mathematics). Philosophy studies an 'abstract' complexity, geography a 'substantive' complexity; similarly, geometry studies 'abstract' spaces, whilst geography is concerned with the 'real' spaces. From another perspective, philosophy is broader, in that the concern is not with the earth, but with 'all possible worlds', philosophy's statements being general and its search being for primary principles.

Environmental Science and the Earth Sciences are pragmatically oriented academic hybrids and a result of the recent dynamic of the politics of academe: e.g. dissolving small departments into big hybrids to create economies of scale, to advertise the implementation of trendy interdisciplinarity, or to eliminate an underperformer in the RAE. They group traditional disciplines, or, more exactly,
most of their substantive side (e.g. 'substantive' geography without the traditional paratextual geography). They cannot replace without losses traditional disciplines (and see our crisp epistemology of epistemic gains and losses developed in the introductory chapter), since the paratextual dimension of a discipline is the main thing responsible for the progress of that discipline, and of science broadly speaking. (consider, for instance its role in creating 'tacit knowledge' during a period of 'normal science', and its crucial significance during a 'scientific revolution').

Figure 11 tries to forge a heuristic device that enlightens us about the ways in which geography's two big themes both separate it from, and relate it to, other scientific disciplines. Meanwhile, it blurs the distinction science-as-knowledge / science-as-practice, since the apparent science-in-the-abstract portrayed in the figure is brutally interrupted and subverted from within by placing in the drawing some scientific practices (Romanian, English) labelled 'geography'; a fact which makes the figure representative for the mundane space of 'science', theorised before. It is worth noting that:

a) Geography is not a 'fortress' with solid boundaries. On the contrary, the boundaries are open and permeable;
b) Geography has been typically portrayed as either the 'science of space', or as a 'crossroads' science between the social sciences on one side, and the hard sciences on the other side. Figure 11 suggests that both portrayals need to be qualified: geography is indeed a crossroads science, but the 'roads' which cross each other are not 1) the social sciences and 2) the hard sciences, but 1) the axis of hard sciences-social sciences, and 2) the axis of empirical sciences-abstract sciences. The last axis has been of considerable importance in the recent history of the discipline: the big problem of
boundaries of 'traditional' disciplines
boundaries of academic hybrids
boundaries of various networks of scientific practice ('national schools')

R - Romanian school of geography; E - English-speaking world's geography

Figure 11: The disciplinary neighbourhood of geography
geography seemed to be its naïveté (excessive empiricism), hence the corrective *theoretical-abstract* strive which characterised (in the case of English-speaking world geography) the second half of the 20th century. Whilst in the late 1950s and the 1960s this effort comprised an engagement with geometry, mathematics, and statistics; starting with the 1970s the preferred path became philosophical sophistication. The leaving behind of the Quantitative Revolution has usually been depicted as a rupture/threshold, suggesting only discontinuity, when actually there has been *continuity* as well (the endeavour to make geography an abstract, sophisticated discipline).

The second portrayal of geography, as the 'science of space', is too broad, in that it overlooks the existence of another science of space - geometry. Geography is interested in the empirical space only, even if this interest may eventually lead to the need to elaborate some abstract mathematical modelling.

c) The neighbours of geography are not only sociology (for human geography) and geology (for physical geography). Firstly, as hitherto suggested, one has to acknowledge the *triviality* of all these approaches which identify 'neighbours' (the actual number of disciplines with which geographies have interfered is impressive). Secondly, one also has to concede the usefulness of this type of concern (the importance of simplification; the analysis of the *specific difference, if any*, between geography and the most identity-troubling disciplines; this, in turn, is fundamental for the discursive fight for a distinct place in the academic division of labour). With all simplifications here acknowledged, in my graphic model, the very close neighbours of geography are not only geology and sociology, but also history, geometry, philosophy (see the history of Naturphilosophie) and ecology (it is interesting that the main way of separating
it from geography is not so much the 'object of study', as it is
the level of abstraction and the philosophical assumptions
which underpin that 'study').
d) Hybrids like 'Earth Sciences' or 'Environmental Science'
are analysed and represented as distinct from 'traditional'
disciplines. They are nothing more than a pragmatic,
convenient way of organising academic specialities. Their
lack of disciplinary 'dust'/paratext is precisely what makes
them epistemically less-fruitful on a long-term perspective;
e) The figure portrays geography as an open space which is
constituted by various scientific practices: instead of the
*over-mentioned* physical geography/human geography
divide, the key divides which continuously produce the
differential space of the discipline are those between
(national but not only) schools of geography, seen as sets of
disconnected networks. The representation of the R
(Romanian geographic school) and E (English-speaking
world's geography) reflects the author's choice for outlining
two *contrasting* visions and practices of geography. We did
the distinct positioning by taking into consideration the tacit
knowledges and the mainstream accounts of the two
paratextual geographies. In the case of Romania, there is a
clear perception of the discipline as *empirical* and the
discourses themselves tend to be poorly theoretically
informed. The classifications of sciences and the self-
identifications place geography rather among physical
sciences (until 1990, the most important three geographical
schools-those of Bucharest, Cluj-Napoca, and Iasi,
functioned together with geology, within the same
departments). In the English-speaking world (and France),
geography figures mostly as a social science, and the last
decades were characterised by a general
abstraction/sophistication of the discipline. The figure tries
to suggest that trivial concerns for geography's loss of
disciplinary identity in the context of its shift toward social science would better be superseded with a geographical perspective on 'geography'.

f) The theoretical conflicts between geography and its 'neighbours' on issues related to the disciplinary belonging of some contested tradition (geomorphology claimed by geology, environmental issues claimed by ecology, the analysis of the social past claimed by history, etc.) suggest more tension than is actually the case: only from a holistic perspective can one trace the distinct place of a discipline, paying close attention to the variations from one region to another, from one historical period to another, from one tacit knowledge (and módus operandi) to another. The figure offers this holistic image and the contexts which create a 'distinct place' for geography. What I think is striking in this model is that the disputed boundaries between geography and each of its 'neighbours' are not endangering its existence and identity, but, on the contrary, are constitutive of, and fundamental to, the shaping of geography's identity. Two intertwined issues come to mind:

− Firstly, these contested boundaries are signs of a live discipline, of an ongoing theoretical (and non ~) argument of how far geography can extend its academic territory. The reality of a dynamic body of knowledge production owes much to its external contestation. A 'safe', uncontested geography would be a dead body in a coffin, whose silent rest would be threatened only by the worms who excavate the holes of organic decay. The image of a vivid discipline is fundamental then to geography's identity;

− Secondly, the contested boundaries create difference, or, more exactly, contribute to the production of 'geographies' as a differential space. Difference is, then, another key feature of geography's identity. But contested boundaries produce difference even at the substantive level,
of concrete research, in that they act as heterotopias, as spaces of theoretical encounters between two disciplines, fuelling processes of cross-fertilisation and hosting dynamics of hybridisation, which, in turn, act as challengers for the neighbour disciplines. It appears then more suitable to speak not of disciplinary boundaries, but of disciplinary boundaring (i.e. continuous boundary tracing) since what actually happens is an ongoing process of 'scientific' negotiation. The vision of 'boundary-tracing', with which the concept of mundane spaces of 'science' operates, dovetails with the ontology of fluidentities and with the undoing of the inside/outside dichotomy in chapter 3.

If one makes the necessary mental connection so as to see that: a) the (global) space of geography is constituted through a loose 'almost-networking' of rather disconnected geographical traditions, and b) there are no boundaries, but 'boundaring', one can notice that boundaring is not something which happens at the periphery of geography, for 'geography' has no periphery (it has a periphery only in 1) the spatial imaginary created by essentialist and functionalist definitions of geography, which expose a core-periphery model, and 2) in the science-in-the-abstract worldview)! Boundaring happens everywhere within the space of geography: first, there is a boundaring of some geographies from other geographies; second, each geography separately negotiates its boundaring (in historically and spatially specific sites) with the other disciplines.

In the context of advancing a definition of geography, we ventured into some speculations about the historical dynamic of disciplinary formations, yet, we have not discussed the basic problem of the very origin of geography. How was geography 'born'? In her explanation of the production of contemporary gender regimes, Judith Butler (1990, 1993, 1997a-b, 2000) argued that finding the exact origin does not
matter. What is important are the social processes which fuelled and regulated the imposition of those regimes. The lack of concern for 'origins', endorsed in some poststructuralist and postmodernist writings seems to me deeply problematic and sterile. Two issues arise:

a) Firstly, all non-postmodernist discourses emphasise the importance of the starting-point in the explanation of the development of an 'entity'. Avoiding a consideration of this matter might seem as the taking of an easy way, as a deliberate overlooking of an important issue. We conceived this chapter as a 'low-context culture', which means that its understanding does not essentially depend on one's having a solid background on topics such as epistemology, philosophy of science, social studies of science, social theory, ontology, history of ideas, history of geography, etc. As such, it acknowledges the commonsense concern for discussing the problem of origin and does not dismiss it as irrelevant. This aside, transdisciplinary approaches such as general system theory and, more recently, complexity theory (Simandan, 1997, Thrift, 1999b) pay considerable attention to the starting point, given that, for example, two 'systems' which originate in almost identical (but not identical) locations might however have trajectories and ending-points far remote from one another. 'Insignificant' differences in the starting-point can produce fundamental differences for the future dynamics of two systems;

b) Secondly, but of equal importance, our analysis will take aim at the heart of the controversy over geography's origins in order to offer an interpretative alternative to some of the other accounts, which tend to ground the value and legitimacy of geography in some trans-historical metaphysical allotment and associated epistemic patronage over an important realm in itself. The argument,
conspicuously spurious, is that discipline X is valuable and important because its 'object of study' (i.e. realm) is important. In other words, instead of correctly assessing the value of a discipline by considering the quality and relevance (lato sensu) of its theorising and findings about the 'allotted' realm, they take the realm itself and demonstrate that it matters, and, therefore, they argue that the discipline studying it also matters. The case is all-too-frequent in geography, including here works such as Edward Soja's 'Postmodern geography. The reassertion of space in critical social theory' (1989) or Doreen Massey and John Allen's 'Geography Matters!' (1984). Interestingly though, whereas this type of rhetoric is at work almost always when the importance of geography is grounded on the importance of the big-theme of 'space', in the case of the other big theme-'complexity', some arguments refer not so much to the ontological idea of a complex world, but at the epistemological meaning of complexity: the knowledge production is so divided among disciplines, that geography is needed in order to bring together the findings of 'normal' disciplines and to offer 'the big picture'.

In order to subvert and displace the spurious argument 'valuable realm, therefore, valuable discipline', my explanatory hypothesis on the origin of geography brings together three ideas summed up by three signifiers: 'uncontested referent', 'sceptical epistemology', 'mundane account'.

1. Uncontested referent: in the history of humanity almost all ideas, theories, and bodies of knowledge are, in various degrees, contested and, therefore, their invocation makes necessary a more or less systematic demonstration of their validity or usefulness. However, there are some (very few) issues which are, so far, undisputed. One of them is the analytical imperative of human knowledge: one cannot know
everything 'at once' and, therefore, one way or another, one has to divide the reality out there and to study it through a piecemeal approach. The very existence of language, 'of more than one word' might be interpreted as a way of dwelling with this analytical imperative as well. From a historical perspective, the systematic theoretical work that drew the consequences of this imperative and organised a coherent framework for (human) knowledge production took first place in Ancient Greece, or, more generously and exactly, in the Mediterranean Basin. Some argued (William Pattison, 1990) that, in the case of geography, the physical geography tradition started with Aristotle, the spatial tradition with Ptolemy, the regional tradition with Strabo and the complexity (culture/nature relations) tradition with Hippocrates. Be that as it may, how people defined geography at that time and what realm ('Earth', 'space', 'culture/nature relations', 'regions', etc.) was taken to be its object of study are issues that do not have too much relevance for defending geography's case today. At this point, our pragmatic sceptical epistemology enters the stage.

2. Pragmatic sceptical epistemology: the beginning of this book divided theorists into two main groups, according to their degree of optimism regarding the possibility of knowledge. One group says that humans can get to know the truth 'out-there' (including here the crucial fact that once that truth known, humans can also know that they know the truth). The other group says that our biological and/or cultural condition 'prevent' us from knowing the 'absolute' truth of reality, or that there is no truth out-there, 'truth' being a mere human construction. The very fact that there is such a debate around these issues might lead one (it certainly leads me) to a cautious, in-between position, called here pragmatic scepticism: we cannot know for sure whether we can or cannot know the
truth out-there. This is helpful in that it destabilises all strong legitimating strategies grounded in the pretence to have unproblematic access to ontologies. To get down to specifics, one cannot say that 'geography matters because space matters' since perhaps space is an erroneous, misleading concept, ontologically credited because it seems plausible, commonsensical (the work of Albert Einstein on the non commonsensical relation between time and space or that of Giordano Bruno might be a good reminder of the dangers linked with assessing epistemic claims against the mere backcloth of common sense).

The analytical imperative explains why we 'cut' the world into categories - space, society, past, nature, substances, animals, etc.- and allot them to distinct disciplines meant to ensure a systematic study of their ontoepistemic 'garden'. What is not sure at all is whether these cuttings were abusive or not. Perhaps the truth of the reality out-there is that 'space' and 'time' are distinct realms, and therefore the cutting was correct; but perhaps they are not correctly cut: we just cannot know for sure. Given these epistemic frustrations, saying that 'geography is important, because space is important' (an example) is not at all the solid, legitimating argument people assume it to be.

The perceived importance of a discipline is not a matter of either historical precedence or of metaphysical monopoly (e.g. over 'space'), but a non-linear function of the politics of its contemporary dynamics (intra-disciplinary politics, interdisciplinary politics, politics of research assessment and funding, and politics with a big P, such as the limitation of the freedom of speech in American academia after September the 11th, 2001).

It encompasses the quantity, the perceived quality, and perceived usefulness of its findings in the present academic and political economic context (cf. Castree, 2000, Johnston,
In turn, the judgement on 'quality' and 'usefulness' is assessed on shifting grounds, in relation with competing protocols of enquiry, changing policies of science, intradisciplinary idiosyncrasies. From a temporal point of view and within the mindset of our recursive cartographies, the act of disciplinary legitimisation does not precede a comfortable subsequent working within a discipline ('we have legitimacy and prestige, therefore we can work without worrying too much'), but is continuous and unfolds as a rhythm that mediates between that intra-disciplinary working itself, and the broader politics of academe. In sum, disciplinary legitimacy is never a happy comfortable legacy, but an anxious and for-ever-insecure rhythm through which an identity ('geography') constructs itself as it tries to cope with its Other.

3. To destabilise even further the legitimating narratives of origins, the third mentioned element – the mundane account - is much needed. It makes room for performativity, embodiment, and happening. What is fundamental for the starting-point of geography is, I believe, the very invention of the concept 'geography', due to a scholar called Erathosten, who lived between 275(?) and 194 BC. The very name 'geography', although denoting a whole series of various scientific practices, has been particularly important in their becoming. Its coining might be paralleled with the grain of sand, so 'insignificant', yet so necessary for the genesis of a pearl. We do not pay sufficient attention in our accounts to these happenings, which involve performance and embodiment, and which are so important for explaining all the 'big things'. Analyses of intellectual 'contexts' and intricacies and of social backgrounds are useful but never sufficient in the building of a historical explanation.
This three-fold approach to the matter of geography's origin has the advantage that: a) it invokes the problem of origin in order to subvert the attempts of using it for (spurious) legitimisations, and b) it supports the overall non-essentialist, non-normative, and non-heroic view developed here about the dynamics of 'geography'. At a more general level, the three-fold approach is an argument for the idea that starting-points do matter a lot. In the big scheme of paratextual geography, this approach helps then, to subvert some 'mythological' elaborations that frequently underpin judgements on the past and future of the discipline. Among them, one could mention the myth of the *Golden Age*, as well as its 'others': the myth of *decadence* (geography is loosing its past distinctiveness and prestige, is on the wrong path -see, for example, Richard Hartshorne's chapter 'Deviations from the Course of Historical Development', in his 1939 'The Nature of Geography', or David Stoddart's, 1987, 'To claim the high ground: geography for the end of the century'), the myth of *progress*, the myth of *unity* (unity is power!), the myth of the besieged 'city' (in the chains of signification of Haggett's urban metaphor), the myth of the *Saviour*, the myth of the *revolution*, etc.

We are a long way now from the starting question of the second part of the book 'What is geography from a pragmatic sceptical perspective?' The answer involved work within social theory, epistemology and the philosophy of science (e.g. the concept of the mundane spaces of 'science', of boundaring, of legitimating referents, of big theme, etc.); critical engagement with past answers to the question; broadening of the discussion to the history of the discipline, which made necessary the critique of established models and the proposal of a new model of historical becoming centred on the relation within and between the two big themes; and some elaborations of ontological issues (on fluidentities,
boundarings, starting-points, discursive / nondiscursive interplay, settings of distinct realms, actants, flows, plural belongings, etc.) which have enriched in solid ways the broader framework outlined in the first part.

* Yet, I am not satisfied with applying the philosophy of pragmatic scepticism for merely explaining disciplinary formations. In the final part of this book, I try to open a further horizon of research, by analysing how pragmatic scepticism sheds a distinct light on several inconspicuous struggles.
III. Pragmatic Scepticism

&

Philosophies of Struggle
III. Pragmatic Scepticism & Philosophies of Struggle

The first chapter of the third part of this book elaborates on the political economy of struggles over knowledge in a globalised world. Inspired by the pragmatic sceptical celebration of the multiplicity of epistemic positions, I believe that one of the ways for overcoming some of the biases of traditional research on the relationship between higher education and the labour market (dominated by economists and sociologists) is the adoption of a geographical perspective. Paying attention to the difference that space makes, to regional inequalities, to the agents of place formation, to globalisation, or to the overall image of a complex issue is likely to bring about a better, more fruitful account of the intricacies between the globalised struggle for knowledge in higher education and the globalised labour market.

Taking seriously my commitment to the ideals of pragmatic scepticism, in the last chapter I approach the question of struggle not from the political economy perspective of chapter nine, but from a political epistemology perspective. However, in order to maintain the coherence of the whole book, and to ensure the logical flow of its third part, I keep the same focus on the question of knowledge and the same background of globalisation, but I return to the narrower domain of geographical knowledge.

Some of the recent struggles over geography have been prompted by the ideal to heal the painful divide between physical geography and human geography. In the last chapter, I engage this struggle by explaining the differences between my approach to this struggle and the approach
advanced by Doreen Massey, one of the central figures involved in this political-epistemic debate. More specifically, I will provide an immanent critique of the way she theorises geography, physics, and the relation between these two disciplines. Both chapters support the same aim: to suggest some ways in which a pragmatic sceptical attitude renders apparent inconspicuous struggles in the globalised academic landscape.
9. Struggles for Knowledge

This chapter discusses the impact of globalisation and socio-economic struggles on the multifarious relationship between higher education and the economy, with a special emphasis on the labour market. After a brief account of the methodological controversies related to research in this area, there are outlined, in turn, recent changes concerning the labour market in general, the affordability of higher education for students and the student market, the academic labour market and the academic work environment, and the management of higher education. In the concluding section, there are presented some suggestions regarding both future research in this area and the shaping of more adequate strategies for coping with the managerialist turn in higher education.

It has been recently suggested (Alasuutari, 2000) that globalisation is not as much interesting for understanding the changes in the contemporary dynamics of our world, as it is for making sense of the policies and fashions of the academic discourse. Whereas this is a very sided position, it has to be noted that indeed the word ‘globalisation’ is one of the most frequently deployed in the sphere of the social sciences. Its definitions are quite diverse, but nevertheless converge in the core set of ideas referring to the increased importance of the social-economic actors other than the nation-states (e.g. multinational corporations, international networks, religious groups, regional associations, etc.), of the international impact on the shaping of national socio-economic policies, of the emergence of a consistent series of challenges (e.g. global environmental change, famine, poverty, demographic growth, the management of the
‘global commons’, regulation of trade and labour force) which cannot be effectively met but at the international level, of the phenomena of Americanisation, spread of English as the new lingua franca, etc. From the very enumeration of these facets, one can hint at the close influence of globalisation on the re-configuration of the relationship between higher education and the world of work. The fact that both terms of this relation are themselves increasingly ‘globalised’ makes the debate even more complicated. Therefore, in the economy of this chapter we are particularly keen to capture those elements which shed light on a structural feature underpinning higher education, the labour market, and globalisation altogether, notably the increasing social and economic inequality and the struggle over resources this inequality generates. The understanding of its dimension, its impact and its ethical implications constitutes a prerequisite for designing emancipatory policies for both higher education and the labour market. It is also needed for facing more effectively the reality of uneven regional development, as we shall try to suggest in pointing to the case of the restructuring of the Romanian tertiary education system.

**Methodological problems of research linking higher education and the labour market**

Traditionally, this area of research has been dominated by economists and sociologists, who, although emphasise differently the importance of status and income in their analyses, nevertheless share the drawback of ‘missing a balance between the status-distributive and the qualifying function of higher education vis-à-vis the world of work. They focus on the former and infer from it on the latter.’ (Teichler, 1999, p. 178). In the case of economists it can be
added that their concern for accurate, quantitative data, visible in the favouring of statistical tools of analysis and of large surveys, has biased even further the results of their research.

Two major paradigms compete for explaining the relationship between higher education and the labour market and a pragmatic sceptical attitude obliges us to pay attention to their strengths and weaknesses. The first approach is that of manpower requirements, whereas the second is the human capital paradigm. Both should be seen in close connection with the changing economic and political context, which influences the research agenda to a considerable extent. To give just one example, in the 1970s it was an all-pervasive belief that the investment in higher education is non-economic, as it was perceived to be well above the needs of the labour market for well-qualified workers. This idea of overeducation has been discussed ever since, as it has had some statistical evidence to support its claims: the higher educated labour market is considered to be supply-driven. From this observation follows a whole debate about the ways in which the surplus of graduates is absorbed eventually by the market. But the dominant paradigms of economics are also contested for their unrealistic assumptions, which lead the researcher away from acknowledging the actual embeddedness of the economic ‘variables’ in spatial-temporal changing contexts. Thus, until very recently (Hartoog, 2000) the human capital paradigm, closely related to the mechanistic law of supply and demand of mainstream economics, has worked under the ideal assumptions of the full transparency of information and the certainty of decision-making, whereas in actuality a fundamental feature of the relation between tertiary education and the labour market is that decisions of investment in education are taken in conditions of
uncertainty (hence the emergent work concerning risk analyses of educational investments). Research in this area has paid little attention to the geographical dimensions of the relationships involved. Issues of regional inequalities, of the relations between the nodes and the links of the educational network, of internal and international migration of qualified work-force (academics included), of the immersion of universities in the processes of place formation, of the role national differences have in the generation of ‘unique’ constellations of tertiary education-labour market-finance-governmental regulation relations are all crucial for a proper understanding of this area of research.

Geography has said very little in the educational debate within social science and this essay attempts to make more obvious the need for a geographical perspective in this respect. Exploring both the differences that space makes and the commonalities that can be found across local, regional and national systems might help us qualify the grand narratives sociologists and economists are usually inclined to offer. In addition, a geographical perspective can shed light on connections necessary to make in research on the labour market & tertiary education. We have in mind, for instance, the damaging divorce between economically focused research and research undertaken in the field of education sciences (concerned with curricula, teaching and evaluation strategies, sociologies of the professoriat, psychological research on student development, etc.). How could one understand the complex assemblage of the university and of its relation with the labour market without linking these distinct areas of disciplinary research, through analysing, for instance, the academic labour market, the student market, the management of the university as a whole, and the structural role socio-economic inequalities and globalisation processes
play in all these spheres? In what follows we shall render more obvious some of these intricate connections.

Inequalities, globalisation and labour market struggles

One of the most frequently met flaws in the economics of higher education has been the overestimation of the role institutions (notably universities) have in the generation of human capital. Key-factors such as the social and economic background of students, inborn qualities or predispositions, the role of on-job-training, or the cultures of work tend to be under-estimated. In addition, a national bias that tends to ignore the increasingly globalised labour market is still systematically at work.

In measuring the demand of qualified labour, one may consider either the sum of the job offers of economic agents or the estimations done through the institutions of governmental planning. Both of them pay scant attention to international migration and to the hardly predictable dynamics of foreign investments. And the controversial theory of over-education is based precisely on these measurements of labour demand. Some scholars explain the absorption of the apparent surplus of qualified labour by the tendency to fill with graduates jobs traditionally taken by less–skilled workers. This process would mean a ‘mismatch’ between their high skills and the low job requirements. Other scholars argue that graduates are actives agents of change: although they occupy medium-level jobs they tend to add value to those jobs, transforming the economic agents from inside, complexifying the initial mechanisms of work organisation. If this is the case, then the problem of mismatch would be artificial and investment in higher education would always generate increased returns. Recent work done within the human capital paradigm (Hartog,
2000) supports this latter hypothesis. The findings seem to suggest that:

a) The relation between returns and the increased number of years of education is U-shaped: there are higher returns for those who have primary education and postgraduate education as compared with those who do not have such polarised educational standings. At the level of secondary education, the rate of returns is not particularly high (i.e. one individual with secondary education does not earn much more than one who has only primary education);

b) The social rate of return is lower than the private rates of return. Given that, in general, education is more or less subsidised, the net gains from higher education of an individual are bigger than the net gains of the social system;

c) The rate of return is higher in underdeveloped and developing countries than in advanced capitalist economies. In the former situation the supply of graduates is usually lower when compared with the supply in the latter situation. Lower supply means that the equilibrium of the market tends to be established through a higher valuation of the fewer graduates;

d) However, the traditional measurements of return rates underestimate a series of additional benefits – both financial and non-financial – resulting from the completion of tertiary education. Thus, Luis Vila (2000) mentions among these additional gains health benefits, fertility benefits, benefits for children, occupational benefits, benefits related to consumption & savings (these gains all belong to the group of private benefits with some external effects), distributive benefits, stability of social structures, etc. (these gains belong to public benefits related to economic growth and development);
e) The general rule is that, irrespective of the job, better-qualified workers tend to increase the quality of the products;

f) The returns to undereducation are negative, whereas the returns to overeducation are positive, although not as high as those for perfectly matched (‘required’) education. Hartog (2000) estimates them to range between 1/2 and 2/3 of the returns to matched education;

g) The risks associated with educational investment decisions in conditions of uncertainty (i.e. in concrete situations) tend to be compensated. The dynamic of the labour market and of the economy more generally (increased shifts towards the so-called ‘knowledge economy’) seems to favour those who invest in education;

h) The inequalities among people caused by the different genetic endowment (intellectual abilities, aptitudes, etc.) tend to be exacerbated by a fully-developed educational system: those richly ‘endowed’ succeed in the meritocratic educational system and win access to advanced education, whereas those with less genetic endowment tend to be blocked at lower levels within the educational system. This issue places in a different light the concern with the influence of socio-economic inequality on access to tertiary education and on successful behaviour on the labour market. It is at this point that globalisation and the question of socio-economic inequality meet again. Consider for instance the case of a marginal country, underdeveloped, and with a national language other than English. It is clear that in such a context a big factor of success on the labour market is the possession of the skills of speaking English and of mastering some of the Information and Communication Technologies (ICT). Foreign investments, the need to establish links with the external world, or the very chance of emigrating in a more developed country are all related to the incentives for
acquiring such skills. Nevertheless, it is well known that access to education for these skills is quite expensive and tend to favour the wealthy. The alleged universal benefits brought by globalisation for particular underdeveloped settings are in actuality benefits for the wealthy from those settings. Through global connections within the thin layer of the ruling class, local (and ultimately global) inequalities are reinforced.

**A market for students?**

Analyses of the relationship between tertiary education and the labour market usually pay scant attention to the fact that students and prospective students constitute themselves a market, increasingly globalised. Within this market, a negotiation takes place between the financial affordabilities of students, their values, needs, and preferences, and the educational and financial packages various universities offer. Mechanisms for ‘suitable’ matching between exceptional students and famous universities are at work at a global level. Thus, any student from outside the United States who has a strong record and obtain an outstanding score at the famous GRE test (Graduate Record Examination) could easily find an American university offering him/her an advantageous ‘package’ (including full pay of tuition fees and living expenses). To give another example, in United Kingdom there is a number of 800 scholarships / year offered by the British government (the Overseas Research Scheme) for covering the differential between the ‘normal fees’ payable by British students and the overseas fees, which are more than the double of the former. They are aimed specifically at encouraging the selection of exceptional students from overseas. National boundaries do not bother too much the free circulation of talents, although
enjoying the benefits of globalisation presupposes economic access to not-readily-available skills, such as the mastering of English and of elementary ICT skills (such as computer and Internet literacy). Furthermore, what represents benefits for individuals (the prospect of studying abroad and, later on, of immigrating) constitute serious drawbacks for the states that have invested in the initial formation of those individuals. Inequalities and regional uneven development are ultimately reinforced.

A delicate political and moral issue, particularly in the context of the raise of extreme right ultranationalistic political forces (e.g. Austria, France, Italy, Germany) is the apparent lack of care of national governments for the protection of the local students. Instead of allotting money for their benefit, both universities and (sometimes) governments are tempted to invest only in the very gifted students, irrespective of their citizenship…Apart from that, the public support for higher education varies considerably from one country to another, the general tendency being a diminution of public financial support and an increase of the financial burden the student has to cope with. It is readily apparent that this neoliberal reactionary manoeuvre reinforces social and economic inequalities. This unfortunate change appears truly sad especially in Europe, where the emancipatory ideals 27 have for long underpinned public discourses and policies. Thus, in United Kingdom the recent introduction of tuition fees for all undergraduate students has been particularly criticised. Students have an increased access to ‘convenient’ loans, but these loans cover only a part of the total costs (tuition fees, living expenses) of a university degree. Therefore, most students have to work part-time, but it has been reported that many of them work

27 The Enlightenment Project included the commitment to bringing light (i.e. free education) to the masses.
well above the maximum number of hours allowed by universities’ regulations. No surprise than, that recent surveys revealed that 46% of the male students and 64% of the female students suffer from depression and/or anxiety and that one of the main reasons indicated for these symptoms has been related to financial difficulties and uncertainties. This aside, at the end of their first degree, they enter the labour market with the considerable burden of having to make savings to pay back their loans. The case of Britain is rather extreme at the level of the European Union. Thus, in Denmark, Finland, Netherlands and Sweden 90% of the students are eligible for a basic grant, which cover between 26% -in Sweden- and 66% -in Denmark- of the total costs of a university degree (Vossensteyn, J., 1999). Recent analyses of educational policies (De Weert, 2001, Moscati, 2001, Mora, 2001, Askling, 2001, Kivinen, Nurmi, Salmininty, 2000, Kellermann, Sagmeister, 2000) in other, more ‘welfarist’ European countries converge in saying that the general tendency is a shift towards higher financial responsibilities for the students. This neoliberal gift comes at a time when the general managerialist pressures on the universities drive their administrators to emphasise the research side of academic work – the main criterion for assessing the quality and competitiveness of a university. This emphasis goes hand in hand with a relative neglect of teaching - exactly that which is of most relevance for students. The data regarding the continuous degradation of students/ faculty ratio is illustrative for this state of affairs (see Enders & Teichler, 1997). In such a changing context, raising the question whether higher education really remains a worthy investment seems natural. Economists have been particularly keen to underline that in the judgements done in this regard, lay people and
students overlook a large part of the real costs: they consider only tuition fees and living expenses and forget to take into account the income which would have been earned had the respective student chosen to work instead of starting tertiary education. By and large, despite the financial inconvenient (in the short perspective at least) of higher education, people seem to value its non-financial returns, aspect which explains the increased interest in further education, despite the degradation of the social comfort of student life. Nevertheless, one should not forget that the very poor still do not have access to tertiary education and that the recent shift from welfarist to post-welfarist governance brings only bad news for them. Before moving to the next paragraph, concerned with the changing academic labour market and the academic work environment, we will remind the special case of graduate research students who have to work as teaching / research assistants in order to pay their tuition fees and living expenses. It is a particular case in which the labour market complicates to an extent poorly captured by traditional research. Those who will soon compete as highly qualified workers on either the academic labour market or the ‘normal’ labour market (e.g. in industry) are already caught within the intricacies of a segment of the academic labour market (that of low-level, flexible or rather precarious employment) and teach at their turn future generations of graduates.

The academic labour market and the changing (academic) work environment

There are few fields more globalised than the academic labour market of advanced capitalist economies. Criteria of excellence seem to surpass any other criteria (e.g. nationality, citizenship, etc.) in the selection of academics.
Anybody with the required qualification can compete for a position in academia in the United States or United Kingdom. Apparently, priority is given to American / British citizens, but in practice it is obvious that universities go for the most talented candidates. The gains for those who can afford to pay satisfactorily -advanced capitalist economies- are considerable. The less developed countries, which suffer the consequences of this systematic brain drain, have virtually no means to prevent it. The process happens with the Indian and Chinese scientists, with the Russian experts who used to work for the Red Army, with the Romanian mathematical school (apparently there are 400 chairs in mathematics in American universities held by Romanian professors), etc. Be that as it may, but what is even more spectacular is the brain drain among universities in advanced capitalist countries. Thus, Canadian and British universities are under the continuous threat of the American competition. The latter can afford to recruit Canadian and British faculty by paying a considerably higher salary.

This international picture becomes more complicated with the consideration of the migration of academics within the same country. Most of it is generated by offers of promotion made to younger faculty, but one should not overlook that it also occurs in the case of full professors, tempted by either a higher income, or the prospect of teaching in better-rated universities. These ‘definitive’ movements are accompanied by more flexible academic strategies, such as the fashion of the visiting professorship, which seems to be one of the most striking expressions of globalisation in academia. Real academic gurus (such as Spivak, Harvey, Giddens, Castells, etc.) hold visiting professorships and have to attend

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28 The political regime of George W. Bush has rendered American universities less attractive for both international students and international faculty.
conferences on both sides of the Atlantic. Since always, the prospect of travelling has been one of the major incentives for choosing to work in academia, and the global mobility of academics living in advanced economies remains impressive. The same cannot be said of academics who work in less fortunate contexts. Issues of socio-economic inequality within the same profession are closely associated with uneven regional development. Thus, Kris Olds (2001) reports the case of a recent international conference held in Singapore, where, despite the strive of the organisers to make it affordable to as many faculty as possible, the best represented were the Americans and the British. There were very few or no participants from Latin America, East Europe, former Soviet Union, Africa, or even some poor South-East Asian countries such as Myanmar. The struggle over economic resources influences the content of the academic production, favouring the over-representation in the scholarly discourse of the wealthy. The mundanity of the spaces of science appears here again, albeit from a different entry point. Through a curious coincidence, it is the former colonialists who dominate the rhetoric of post-colonial studies.

The social geographies of junior faculty (i.e. those who, in the American and Canadian systems, do not have tenure) seem to indicate a continuous degradation of their working conditions. Managers of tertiary education, confronted with the imperative to transform their institutions in competitive market-driven actors, tend to keep constant or to reduce the number of permanent faculty and to boost the number of students. The result is a worrying increase of the teaching load and a significant decline in the quality of the teaching environment: as the number of hours of teaching increases,

29 Following our discussion about the incorporation of the mundane in chapter five.
so does the number of students attending a lecture. The younger faculty are now asked to be excellent in all the three major endeavours of an academic: teaching, research, and administration, even though empirical evidence suggests that this is a rather absurd expectation. Those truly gifted in all these three areas are indeed very few. Younger faculty seem to find the prospect of an academic career less tempting than some decades ago. This dissatisfaction is in line with the need for increasing the number of hours of work in order to face the ever-higher expectations of the administration. The decline of the public profile of academe might be one factor, but the degradation of the working loads and of the average payment (as compared with industry) remains the most salient explanation. Thus, on the web pages of The Project on Faculty Appointments at Harvard University, one can read samples of the opinions of American doctoral students when asked to appreciate their temptation of pursuing an academic career (http://gseweb.harvard.edu/~hpfa/reluc4.htm#top):

Graduate school definitely taught me one thing: It is impossible to have a happy home life and be a professor.

I see how my professors live, and I’m not particularly interested in it. I want to have a family.

I’ve postponed having a life through graduate school. If I get on the tenure track, I won’t have any time for another seven years. Enough is enough!

Nevertheless, the results of the ‘International Survey of the Academic Profession’ coordinated by the Carnegie Foundation (Boyer, Altbach, Whitelaw, 1994) reveal that despite the degradation of the privileges enjoyed by the academic profession, most academics would have chosen again this career. The statement ‘If I had to do it again I
would not become an academic’ is endorsed only by a proportion ranging from 24% (the highest) in the case of Japanese middle rank faculty, to 7% (the lowest) in the case of Swedish faculty. Intermediate figures were obtained in the case of American faculty (9-12%), English faculty (19-21%), and German faculty (13-19%). The main reason for the continuous interest of being an academic comes from ‘the relatively independent nature of their jobs’ which ‘allows most academics to find areas of professional activity which are the source of professional attachment and satisfaction.’ (Enders & Teichler, 1997, p. 347).

The virtues of cultural globalisation and the potentials of exploiting the free globalised academic labour market in the sense of deliberately pursuing the construction of truly multicultural departments has been recently addressed by Olds (2001). He suggests that in this way the knowledge production process would benefit from the strengths associated with cultural diversity and different personal biographies. However, in the United States the policies of affirmative action within universities seem to be either abandoned or made less aggressive, so that the general context for multicultural experiments might be less favourable than hitherto.

Post-welfarism, globalisation, and the management of tertiary education

The changes in the last decades in the functioning of the economic system refer to a multitude of variables. Two of them are important for the immersion of the university in the economic jungle: first, the globalisation processes, second, the advent of the knowledge economy, in which the quality of human capital is the key to economic performance. The challenge of these two states of affairs for universities is
striking. Tertiary education institutions are not any longer isolated (have they ever been, though?) from the forces of capital. They are asked to assume the same role as any economic agent and to re-configure their organisational pattern and management according to the needs of successful performance on a competitive market. The ideal is to become a worldwide known research university, capable of entertaining networks with economic agents, with other universities, with governments. The new sense of academic strategy becomes the development of university’s capacity to maximize the potential benefits arising from the unique concentration of highly qualified labour force, research facilities, and teaching dimensions.

The imperative to go out in the international arena presupposes skilful trade with the tools of globalisation. This trade requires, among other aspects, outstanding public relations services, personnel capable of speaking English and of mastering the Information and communication technologies, as well as a professional managerial team able to cope with the economic logic of complex global agents. The web sites of famous universities display the distasteful spectacle of producing and re-producing the reactionary signs of prestige and quality. Through the web site of the University of Harvard, for example, one can access the research findings of various research groups within the institution provided that they pay a certain sum of money for papers, etc. Faculty in these institutions are asked to publish only in top international journals, to participate at international conferences, to develop networks beneficial for the integration of the university in the web of prestigious universities. The need to concentrate important resources to generate high quality areas of research has lead to the pursuit of dubious strategies of specialisation. The departments which are underdeveloped and do not perform well on the
research market are likely to be dismantled, the university obtaining by this cynical means additional resources to strengthen the already renowned departments. To the extent that at the top the competition is very fierce, success today does not guarantee at all success tomorrow.

This strategy has been applied in the USA, and more recently, in the United Kingdom, in close connection with the sad formal rankings established through the Research Assessment Exercise. Within the departments, there are pressures to focus the research activities on either original theoretical work or on marketable, 'relevant' research, the outcomes of which could be sold to economic agents. In the case of American geography, applied or technically oriented research is favoured to the detriment of fundamental, blue-skies research. Heads of departments try to develop particularly the fields of GIS, remote sensing, spatial data analysis, for which there is increasing demand from prospective students and enterprises. Other areas, such as historical geography, cultural geography, or political geography are rather marginalized. As hinted in the second part of this book, it is necessary to note that these 'external' influences will contribute, in the long run, at the very shaping of academic disciplines. Their research agenda is not so much a matter of internal 'dialogue' and progress, as it is a question of what society and the economy need now. From a **pragmatic sceptical perspective**, this shift is dangerous because it threatens fundamental research, it undermines emancipatory, activist work within academia and it attacks more or less directly the principle of academic freedom, which is fundamental for the proper functioning of

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30 The partners of the university are not the poor, the marginalized, and the oppressed, but those who have money; not the proletariat, but the bourgeois; not lay people, but governments and vicious multinational corporations.
the university and for defending the roles intellectuals have in society. As Altbach (2001, p. 218) has put it:

> Academic freedom is a core issue for higher education. It is largely overlooked, when it should be central to every debate about the university. It is as important as managerial accountability, distance education, and the other buzzwords of the new millennium. Indeed, without academic freedom, the central work of teaching and research cannot be truly effective. Moreover, academic freedom at the beginning of the 21st century is facing challenges, as much from the impact of new technologies and the restructuring of traditional universities as from forces that would violate academic freedom by persecuting professors. The future of the university depends on a healthy climate for academic freedom.

As academics, we have the moral obligation to systematically and aggressively remind the filthy political class and the administration of our universities that the management of higher education is more than successfully exploiting the economic benefits arising from a good relation with the industry, the government, and the labour market. The defence of the autonomy of higher education and of the freedom the space of the university has usually enjoyed becomes the urgent task for faculty and administrators alike. This is not for mean purposes (the preservation of the privileges of idle academics) but for the good functioning of society and for ensuring the normal development of science, through genuine, ‘internal’ negotiation among imperfect theories and weird conjectures.

*Given that both the labour market and the struggles for knowledge are increasingly globalised, any research that explores the relationship between them should pay itself considerable attention to the role globalisation processes play in this relationship.*
We should take into account how the structural problem of socio-economic inequality, and one of its geographical dimension - regional uneven development - underpin in obvious or obscured ways the mechanisms of the labour market, the affordability of higher education, and the processes of globalisation themselves. Tempted by the cultural turn, we run the risk of overlooking too easily to analyse and reveal the major problems our world faces: poverty, inequality, and injustice. We have to understand how and why the labour market, higher education, and globalisation processes produce, re-produce (and are themselves produced through) socio-economic inequality.

The relationship between tertiary education and the labour market is better understood if we get down to specifics and analyse particular variables within this relation, such as the (prospective) student market, the academic labour market, and the decision-making processes at the level of the managers of knowledge.

The recent managerialist turn in tertiary education is not absolutely bad or absolutely good. It provides some opportunities for better connecting universities with the needs of the social system, but in the same time it endangers important functions (e.g. fundamental research) and features (e.g. academic freedom) of the traditional university. Intellectuals should become more involved in researching these areas, as well as in engaging the political arena and the public debate. There is a stringent need to raise the general awareness about the vices and perils of the recent turns in higher education.

Inspired by the pragmatic sceptical celebration of the multiplicity of epistemic positions, we believe that one of the ways for overcoming some of the biases of traditional research on the relationship between higher education and the labour market (dominated by economists and
sociologists) is the adoption of a geographical perspective (Amin, 2002, 2004; Larner and Le Heron, 2002). Paying attention to the difference that space makes, to regional inequalities, to the agents of place formation, to globalisation, or to the overall image of a complex issue is likely to bring about a better, more fruitful account of the intricacies between the globalised struggle for knowledge in higher education and the globalised labour market.
10. Struggles over Geography and Physics

The first chapter of the third part of this book elaborated on the political economy of struggles over knowledge in a globalised world. Committed to the ideals of pragmatic scepticism, in this second and last chapter, we approach the question of struggle not from a political economy perspective, but from a political epistemology perspective. In order to maintain the coherence of the whole book, and to ensure the logical flow of its third part, we keep the same focus on the question of knowledge and the same background of globalisation, but we return to the narrower domain of geographical knowledge. Some of the recent struggles over geography have been prompted by the ideal to heal the painful divide between physical geography and human geography. In what follows, I engage this struggle by explaining the differences between my approach to this struggle and that of Doreen Massey, one of the central figures involved in this political-epistemic debate.

Doreen Massey ends her influential paper ‘Space-time, “science” and the relationship between physical geography and human geography’ (1999) with an invitation to dialogue based around her conceptualisations of time, space, and difference. I will point to some problems about the setting of this dialogue. The discussion of these problems might help not only to better assess Massey’s main argument (the linking of time, space, and difference), but also to re-think

31 I believe that in geography it is useful to think in terms of the qualitative equation Time is Space is Difference.
Struggles over Geography

Massey’s paper illustrates a series of disappointing states of affairs relating to the (mis)understanding in the English-speaking world’s metatheoretical debates of 1. geography’s history, 2. geography’s real geography, 3. geography’s ‘abstract’ geography, 4. physical geography. Let me elaborate on each of these facets.

Geography’s history

One of the most common rhetorical strategies in the building of an argument for some ‘new theory’ is to deplore the past and existing situation of the discipline. The problem with this move arises from the fact that it often caricatures those past and existing situations, or, worse, that the enthusiasm for the ‘new’ does not allow any room for substantive engagement with the past. Thus, Massey concludes that:

> By wrenching ourselves away from all vestiges of that old imagination, we might find at least a few elements of a common ground: that both physical and human geography—at least in large measure—are complex sciences about complex systems. (1999, p. 266)

I would be curious to know if there were any geographers ever who argued that geography is a simple science about simple systems. Has not the Earth’s complexity (including here culture – nature relations) always been the main
theoretical ground geographers found for their discipline (‘a
queenly science’, ‘a crossroads science’, exceptionalism, and
all the rhetoric of the overall perspective, of ‘bridging’
remote domains of knowledge)? Has not it been precisely
this complexity the main excuse geographers made for the
lack of quantification and laws in their messy objects of
study? Some years ago, I became interested in how
geographers have accommodated their traditional
approaches to ‘complexity’ with general systems theory and,
more recently, with complexity theory. I remember that
particularly in French and Romanian geography there were
many who noticed that these theories fitted well with the
existing internal theorisations, bringing more sophisticated
accounts to what geographers have always tried to do (Mac,
1996).
Similarly, Doreen Massey pleads for the ‘querying of the
tendency to see space as necessarily divided into closed and
bounded regions’ (1999, p. 263).
Again, I have doubts that there were many geographers who
have seen space in this way. To be sure, any region is a
matter of both specificities and openings / interconnections.
In the past, when the global and interregional flows were not
as intense as they are today, the focus was on the
coproduction of local elements into an emergent regional
specificity. The recent increase in the degree of regional
interdependency requires a more adequate balance between
the specific dimensions and the ‘opening’ dimensions of
regional formations. But it would be difficult to assert that in
the past regions were commonly understood as closed and
bounded (and see on this matter the discussions provoked by
Kimble’s ‘the Inadequacy of the regional concept’, 1951).
But there are many problems with geomorphology’s past
also. Massey tells us that ‘Spedding (1997) proposes a new
kind of question for geomorphology, one that gives priority to compositional relationships.’ (1999, p. 266), and that:

What is particularly interesting, however, about these developments in geomorphology is that in one way or another they are all rethinking the concept of time and their relationship to it. Whether it be through an emphasis on a more qualitative historical science, or via an analysis of the potential bifurcations … the implication is that time is truly open-ended. (1999, p. 271)

In 1979, however, Barbara Kennedy, in a spirited critique of the recent -at that time- introduction of the ‘analysis of environmental systems’, focused on prediction and control models in geography, and, more specifically, in geomorphology, wrote that:

...Our subject matter almost inevitably has a history and that history will frequently prove very important indeed in determining future developments and non-developments. (p. 551)
What has been very clear for a number of years now is that the configurational elements which must be taken into account in attempts to explain the past, present and future behaviour of the world in a geographical sense, are extremely difficult to handle in an explanatory framework which derives too narrowly from physics...(p. 552)
We began with the persistent problem of complexity … and with our predecessors’ simple-minded attempts to increase their understanding of that concatenation of history and geography which is the world we live in. We have been offered a new approach…Fundamentally it would call for an elimination of the historical or, indeed, the configurational in any guise, from our studies; (p. 558).

Traditionally in physical geography and geomorphology the historical approach has dominated:

32 The ‘analysis of environmental systems’ was focused on prediction and control models.
Thinking in physical geography over the last 100 years has shown a strong and understandable fixation with the role of time in the explanation of landscape. (Slaymaker and Spencer, 1998, p. 250).

The emphasis on timeless process studies in the 1980s and early 1990s was, as Hartshorne would have put it, a ‘deviation’ from the course of geomorphology’s development. And even if there might be discussions whether it has really dominated the English-speaking world’s geomorphology, it has obviously not dominated other schools of geomorphology (e.g. Romanian or Russian, etc.; see Mac, 1996). Massey draws on the work of some geomorphologists themselves associated with the quantitative paradigm and erroneously takes the internal evolutions of thought within that paradigm for the whole geomorphologic tradition. In so doing, she creates a false problem, which is used as a building-block in her argument.

**Geography’s real geography**

One of the long-standing rhetorical concerns of English geographers has been that of (re)-unifying geography. I applaud it. The problem is with what they take to be the objects of that reunification. To begin with, they almost always think of human geography and physical geography. Second, they almost always take for ‘geography’ what actually is the English-speaking world’s geography. In so doing, they enter into a vicious circle: they caricature a complex state of affairs; in turn that caricature makes the task of (re)bridging the discipline’s specialisms harder than it might actually be, and leads some (e.g. Sayer, 2000) to an acceptance of this caricatured state of affairs as normal and unavoidable.

Massey, whose paper is an exemplar for my previous appreciations, would have found much more commonalities
across the discipline if she had questioned the basic facts behind the problem she has attempted to solve. Ironically, as a critical geographer, she overlooks precisely the critical geography of geography. What is its complex state of affairs?

It consists in the insufficient exploitation of the ‘theoretical’ potential of the discipline, caused by a lack of systematic engagements with, first, the spaces between geography’s (national) traditions and, secondly, ‘the spaces’ between geography’s ‘specialisms’. As you can notice, I challenge the view that the main problem is the (re)bridging of physical geography and human geography as they are depicted in the English-speaking world, and argue that there are various geographical schools scattered over the continents, from Asia to South America, that should first be the objects of our strive for unification. It is here that we should look first. This is the major divide within geography and has always been so. It has had a decisive impact on the (caricatured) physical geography-human geography divide and, if we tried to explore the first, we would find a wide range of different ways of working out the second. You may question the need or, indeed, the possibility of such unification. But the arguments are compelling.

Without doubt, the size of a functional (unified) scientific community matters a lot for both the quantity and the quality of its findings, as well as more mundane issues relating to its survival and public profile. One might even disregard the standard histories of the discipline that interpret its low status through its failure to become a rigorous, quantified one. Has not place-boundness (by which I refer to the incapacity of national geographic schools -caused by linguistic, financial, political, and derived conceptual barriers- to communicate and to become parts in a truly international scientific community) contributed at least in
some measure to this status? Of course, some will hurry to reply that it is the lack of quantification (mathematics as a universal language) that caused place-boundness and that I am wrong in taking a derived effect to be the main cause. This would be a crude judgement, for mathematics is not the only way to overcome linguistic barriers and linguistic barriers are only one factor causing place-boundness (see Minca, 2000, Samers and Sidaway, 2000, Yeung, 2001, Olds, 2001). If we see things in this light, then the idea that it is place-boundness, which caused lack of quantification rather than vice-versa, might not seem so stupid after all.

The well-intended staff of the International Geographical Union might be outraged by my bold statement that geography is a generic name for a set of various scientific practices loosely held together33 and would argue that there are plenty of transnational and international projects which link (national) geographical schools. However, trips to IGU congresses, shakings of hands and multinational teams that work in more or less narrow research topics (from landslides to migrations) do not undermine my thesis. In the current institutional setting for ‘international co-operation’ it is rather conservatism and trade with old vestiges that triumph and not the genuine dialogue and diffusion of provocative ideas that should shape the present and future-making of a connected discipline.

Had Doreen Massey taken seriously the geography of geography, she would have found out that her appreciation of human geography is by no means true in the non-English-

33 To remind the readers, this is how I define geography in the second part of this book: ‘a generic name for a set of various scientific practices, loosely held together and thus identifiable by a common and long-standing concern for the big themes of “space” and “Earth-complexity”, as well as by the networks generated through its having a distinct position in the academic division of labour’.
speaking world. To give just an East-European example, the impact of communist ideology on the social sciences was so high that a lot of that ‘science’ transformed itself into ideology. One unfortunate solution found by human geographers was to avoid an engagement with theory (as the only theory allowed was ideology in one guise or another) and to focus on technical work such as the drawing of maps and the analysis of census data…No productions of space, no mappings of subject formation, no deconstructions, no rhizomes, no transformational politics of difference. And the geomorphologists on whom Doreen Massey draws would have found that, at least in some parts of continental Europe, geomorphology is a very historical discipline and that the paradigm of the ‘English quantitative school’ is not dominant34.

**Geography’s ‘abstract’ geography**
The explanation for the systematic overlooking of the geography of geography lies, I believe, in the long-lasting influence of traditional philosophy of science, which, in claiming to describe science, has actually attempted to shape it. To see, above all, the geography of geography (or of other disciplines) one really needs a pragmatic sceptical philosophy of science that operates with the mundane spaces of ‘science’ (practices scattered over space) and not the purely abstract ‘space’ of science of traditional philosophy. In science-in-the-abstract35 at one end lies the past of total

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34 E.g. it was first imported into Romania in the late 1980s, but most work on geomorphology has remained ‘historical’ / traditional. The marginal position of the approach might have been influenced by the lack of mathematical training of most geomorphologists. Nevertheless, they are experts in ‘reading’ or ‘perceiving by eye’ the landscape…; see also Mac, 1996.

35 See chapter five in this book.
ignorance, at the other end is the attainable future of total knowledge, the linking of the two being done through the tracks of various disciplines -efficient vehicles for systematic and uncontroversial progress. In such a ‘space’ we are used to think of the disciplinary neighbourhood of geography as being harassed by say geology and sociology. However, its uncomfortable status comes not so much from the petty rivalries with its neighbours, as from its special boundary position between two big families of disciplines: the social sciences and the hard / natural sciences.

As I have shown in earlier chapters, thinking only in these abstract terms has lead us on a wrong path, making inconspicuous what should have been -for geographers at any event- an obvious state of affairs. This abstract scientific spatialization has had even more detrimental effects on geography and the theorisations of its conceptual foundations as it has gone hand in hand with the tendency towards the purification of culture-nature (Latour, 1993) on which the Western civilisation has been built. Doreen Massey and virtually all the other geographers in the English speaking world who plead for the unification of geography are concerned with bridging the divide between physical geography and human geography, without sufficiently questioning that thinking in these binary terms may be one of the causes of the problem in the first place (see fig. 12).

That flawed thinking is then transferred into administrative and organisational settings within the discipline (see the two dictionaries of human / physical geography edited by Johnston et all/Goudie & Thomas, the two recent histories of these fields by Johnston / K. Gregory, the two journals Progress in Human / Physical geography, or the way job offers in geography departments are conceived) and through some materialist semiotics actually arrives at arranging the data to fit the model.
Figure 12. Forgotten in-between geographies
To be sure, the tendency towards purification has for long been with us, but in the distant past (before the ‘Quantitative revolution’) the human geography - physical geography divide was not quite so ‘obvious’ as it seems to be today. The reason lies, I believe, in geographers’ attempt to avoid being (any longer) suspected of defending environmental determinism. Nobody dares or bothers today to reassess the role of nature in the shaping of society, although the recent trends of blurring - at least in theory - the culture / nature divide and the sympathies towards the thesis of overdetermination, for example, should have led ‘naturally’ to re-place (in new ways and with new tools) on the agenda this forgotten and forbidden debate.

So far, I have pointed to three related aspects (the hegemony of thinking in terms of the abstract ‘space’ of science, the tendency towards culture / nature purification, and the fear of accusations of environmental determinism) which have contributed to thinking solely in terms of bridging the two big ‘halves’ of the discipline: physical geography and human geography.

But what about GIS, remote sensing, or cartography? They are general techniques and research areas within geography that have little to do with ‘the big divide’. A remote sensing specialist who is applying his expertise in a human geography topic has more in common with a remote sensing specialist working in physical geography than with most of his human geography colleagues (interested in all sorts of things, from embodiment to regulation theories).

Think then of regional geography. Could it really be reduced to human geography? The same question for environmental geography. As discussed earlier in this book, I did my undergraduate studies in a Romanian geography department, which was organised into four research groups (each having between 10 and 30 faculty): human geography, physical
geography, environmental geography\textsuperscript{36} and regional geography\textsuperscript{37}.

Take then landscape geography: in the English speaking world the concept has largely been appropriated by cultural geography, but in other parts of the geographical wood -to use Massey’s metaphor- landscape is a matter of physical geography (e.g. the Tbilisi School), and most frequently of in-between geography (largely due to the considerable impact in continental geography of George Bertrand’s, 1968 seminal paper). The same displacement of current dualistic thinking could be achieved by invoking metatheoretical branches (the history of geographical thought), overall perspectives (in some traditions there is an established branch called general geography), or applied geography, etc. And if we add in all these forgotten in-between geographies, we will notice that they constitute a potentially powerful\textsuperscript{38} bridge across the divide.

The geographical forest is much more complex than Doreen Massey apparently assumes: she systematically speaks in binary terms. There is a need to question our solidified way of conceiving the discipline of geography. I attempted that questioning in the second part of this book. One of the additional paths we might take is to leave behind the biased perspectives of the traditional ‘history of geography’ and engage in a new research program – metatheoretical

\textsuperscript{36} The same people who taught and researched ‘environmental science’ issues taught and researched ‘human’ (sic) environmental geography - the divide did not exist.

\textsuperscript{37} In the regional geography group many of the faculty had PhD’s in geomorphology; Territorial planning belonged to this research group, etc.

\textsuperscript{38} Even from a quantitative perspective: for instance 20\% of the AAG members belong to the GIS research group.
geography⁴⁹ - in which proper attention is paid to the significance of knowing how our discipline works and to the multifarious⁴⁰ ways in which such a knowledge could be achieved and then implemented in the education of new generations of geographers. But there is a need also to question our solidified way of thinking about reality -the geography ‘out-there’- where the noxious effects of our tendency towards theoretical purification are the most obvious (Whatmore, 2002).

**Physical geography**

To make a smooth transition from geography to physics (the next section), I shall point briefly to the problem with physical geography in Massey’s paper. It is hard to deny that among the branches of physical geography, geomorphology is the most theoretically informed and has a long tradition of work conceiving its theoretical foundations. Nevertheless, it is equally hard to deny that a discussion of physical geography, which takes from physical geography only (some) debates from geomorphology, is unlikely to lead us too far.

Throughout her argument, Massey ignores both the other branches of physical geography (hydrology, pedology, climatology- Rayner and Hobgood, 1991- biogeography-Kupfer, 1995, Stott, 1998) and, even more damagingly, the emergent theoretical-methodological developments of physical geography in general. For it has to be said that in physical geography (and human geography) most

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⁴⁹ I borrow the term from the Russian geographer I. G. Sauskin. See my previous book for an extended elaboration of this alternative path (Simandan, 2002).

⁴⁰ E.g. Subfields would include the geography of geography, history of geography, ethics of geography, the pedagogy of geography, logic and semantics of geography.
(meta)theoretical work has not been done at the level of sub-specialisms (some understood as very ‘technical’, even philosophically-free, as in the case of pedo-geography or hydrology), but at the larger level of the whole (sub)discipline. There is a strong tradition—from Mary Sommerville, Arnold Guyot, Emm. de Martonne, V.S. Kalesnik, Chorley and Kennedy, Morariu and Velcea, to the more recent contributions of Goudie, 1994, Slaymaker and Spencer, 1998, Posea and Armas, 1998, K. J. Gregory, 2000—of theorising an integrated physical geography, within which the focus would be on the ‘crossroads’ of the established specialisms, on ‘the big picture’ attainable by assembling the various specific findings and their emergent - structural properties. Furthermore, it is worth noting that this theorising has been accompanied by practice. As K. Gregory (2000, p. 243) noticed (with regard to United Kingdom, but the same could be said of some other ‘schools’):

Many academics are not confined in their research activity to a single branch of physical geography (19%) and tend to range across branches.

So far, I have pointed to some problems concerning the understanding of, and the engagement with the discipline of geography in Massey’s paper (and in the English-speaking world, more generally). In what follows, the focus will be on her engagement with physics, which has even more relevance to the (main) discussion of time, space, and difference.

**Struggles over Physics**

I found unconvincing Massey’s argument concerning the inadequacy of physics for geography. Some of the reasons for my reservation are outlined below.
Common-sense reasoning

To begin with, her claim that ‘one of the things holding physical and human geography apart for so long has been their relationship to physics’ (p. 261) does not fit very well with common-sense reasoning: if two things have a common referent, it is likely that that commonality helps to bring those parts together, not hold them apart. In addition, the urge to leave the model of physics behind and to orient us towards more historical sciences appears bizarre for that part of the discipline called physical geography. How can one think of doing physical geography without extensively using the findings of very traditional physics – the mechanics of solids and fluids and thermodynamics, for example? To point to a final issue about (non)common-sense reasoning, the prestige of physics is not due, as Massey would have us understand, to its having been privileged by standard epistemology and philosophy of science. The question Doreen Massey does not ask is why it is precisely physics that was chosen by philosophers as the model of science? The traditional internalist philosophies of science (the standard model of the Vienna Circle and Popper’s negative rationalism) which celebrated physics as model science have been gradually and irremediably discredited in the last 50 years (starting perhaps with Quine’s seminal ‘Two dogmas of empiricism’, 1951) but despite the decline of this epistemic parish, physics continues to enjoy a fine reputation among scientists and in society more generally.

Simple versus complex thinking

The overall image one has after reading Massey’s paper is that physics is a simplistic science about simple systems. Anyone who reads an intellectual history of physics (e.g. Krieger, 1992, Cushing, 1998) or has the courage to open at
chance a volume on, say, theoretical physics would disagree
with this caricature.
It is not by fortune or extra-scientific imposition that other
sciences have developed physics envy. Massey takes for
granted the absolute truth of one ‘side’ of one of the most
controversial debates in the history of science: that between
those who believe that the world is really complex,
irreducible to quantification or laws, and those who believe
that despite the baroque details/accidents of the perceived
world, below them lie some gothic laws which ultimately do
explain the apparently messy reality. Doreen Massey
endorses the first position, and so do I, but there are no
certainties in this debate. On the existence or non-existence
of objective laws of nature and on their controversial
universality and necessity, compare for instance the
regularity approach of Hume, Urbach, or Ayer, 1998, the
necessitarian approach of Bigelow, Dretske, or Armstrong,
1989, the eliminativist-instrumentalist approach of Mach,
Pearson, or van Fraassen, 1989, and the anti-realism about
natural laws of Nancy Cartwright, 1983; (see fig. 13).
It might be the case that our options are more a matter of
aesthetic sensibility than of ‘scientific’ clear-cut arguments.
Apparently, the belief in natural laws has gone hand in hand
with ‘the Judeo-Christian belief in a rational God’ (Hodgson,
2000, p. 330) who created a ‘beautiful and ordered world’.
No wonder than, that ‘the way taken by fundamental
physicists’ (p. 327) is ‘to think of the most beautiful way to
design matter, to express this mathematically and then to
look for experimental verification’ (p. 327; see also Zee,
2000). The conjecture that aesthetic sensibility plays an
important role in science matters has also been suggested in
analyses of the (apparent) main ‘competitor’ of the gothic
discourse of simplicity -namely complexity theory. Thus,
Thrift (1999, p. 36) believes ‘that complexity theory is
Figure 13. Approaches to laws in the philosophy of physics
deeply metaphorical’, notes that ‘some of its more subtle proponents see it precisely as an attempt to replace one set of metaphors with another’ (p. 36) and that ‘it has become the stuff of art, film, drama…it has even become a focus of garden design…’ (p. 39).

To come back to the belief Massey and I share: what if we are both wrong? Are all those hard scientists on a misleading path? Is it so wrong to simplify? As a pragmatic sceptic, I am peculiarly careful to detect these erroneous assumptions of the transparency of truth. It has to be said that in the case of many social scientists these assumptions go hand in hand with a heavily caricatured idea of what contemporary mathematics is capable of. The dichotomy Massey draws on (historical-complex-qualitative-correct versus ahistorical-simple-quantitative-wrong) is much more problematic than assumed in current debates in social science. Historians of ideas would argue that in the last fifty years the tendency in the dialectical struggle ‘holism vs. atomism’ (as competing approaches to reality) reveals a revival of the holist perspective. Perhaps this renewed holistic sensibility explains why we tend, as Massey does, to take for true something that is as controversial as ever.

So far, I have criticized the unwarranted tendency of firstly equating simple with backward, narrow or unsatisfactory, and then physics (a study of ‘simple’ systems) with a simplistic view of the world; and argued that simplicity is in itself a complex issue.

But there are problems with her approach to ‘complexity’ as well. Thus, Massey operates with a single definition of complexity -as presupposing the taking into consideration of time, of historicity. By no means, however, is this the single

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41 To be sure, the FOCAS program of the Santa Fe Institute is aimed at building simplified models of complex systems…(!) - see http://www.santafe.edu/sfi/publications/Bulletins/bulletin-spr97/TEXTW1.HTML
definition of complexity. Why not considering the range of the levels (from the quantic to the relativistic-cosmic one) addressed? One could see then that ‘complex’ disciplines like sociology or geography are concerned with merely one of the levels ‘simple’ physics is concerned with. If we add the perhaps-impossible task to search for a unified explanation (dubbed Theory of Everything; see Hooft, 1994, Barrow, 1994) of these levels, physics appears worthy to be on the list of ‘complex sciences’...Indeed, perhaps the greatest stake in contemporary physics comes from the prospect for a Theory of Everything, which, as Davies (1994, p. 226) has put it, ‘in its most ambitious form...seeks to combine all physical laws and principles into a single, unified mathematical scheme, hopefully captured by a single simple formula that you might be able to wear on your T-shirt.’ (note that such a theory, in Breuer’s words, 1997, p.138, ‘does not explain everything; however, this does not prevent such a theory being true of everything’). Given these states of affairs, one has to admit that the philosophical and scientific challenges raised by contemporary physics are not at all simple, and deserve our attention to the same extent as complexity theory or Massey’s favoured sociology and biology.

Nevertheless, even if we put aside alternative understandings of complexity and return to Massey’s emphasis on historical, holist, qualitative dimensions, it is still easy to find evidence to support my claim that her description of physics is inadequate. Thus, Peter Smith (a philosopher of science), commenting on one of the main ingredients of the rhetoric of complexity- i.e. chaos theory- and on its frequent depiction as a revolutionary step forward from traditional physics, argued that (1995):

42 Smith, 1995; 1998; for a systematic account of theorisations of space-time in physics see also Monk, 1997.
There is a problematic question about how trustworthy familiar chaotic mathematical models of physical processes can possibly be. These models are typically derived by the extreme simplification of some physically more realistic treatments (p. 213); ‘Qualitative’ concerns have always been central to dynamics...Any putative distinction between ‘qualitative’ geometric and ‘quantitative’ analytical approaches will simply cross-cut the distinction between chaotic and more familiar dynamical models (both approaches have been used in advanced work for at least a century). (p. 217)...what goes on in mathematical physics is indeed often much more like what...is going on in chaos theory than philosophers’ pictures of science always capture. Still, it is one thing to say that work on chaos is a vivid exemplar of common strategies in mathematical physics that philosophers tend to overlook; it is something else to say that chaos theory has its own distinctive styles of explanation or dynamic understanding. And the latter claim seems simply wrong. (p. 222)

With these thoughts in mind, we can now move to a different perspective. Nigel Thrift (1999) concludes his analysis of the place of complexity with two interpretations of the rhetoric of ‘opened times’, the second of which

Is that this more open time is actually simply a continuation of the older time senses by other means...and of imperialism by other channels, but now in time rather than space...In which case, what we may be seeing in the guise of expanded possibility is simply business as usual. (p. 59-60).

Reverential reference
Doreen Massey considers that geographers turn to physics with what she calls ‘reverential reference’, but I think she exaggerates the extent of this phenomenon and she is

43 It is totally peripheral in human geography.
unfair to point only to physics. What about the reverential reference towards sociology or continental philosophy in human geography (Barnett, 1998, Harvey, 1999)? What I find really troubling is that she does not explicitly say that reverential reference is bad per se in science matters. She deplores only the reverence to physics and in saying that ‘Perhaps disciplines that study “complex systems” (from meteorology to sociology) can now lead the way’ (p. 266) she leaves room for believing that only the object of reverence should be changed and not the very idea of paying reverences. In so doing, she ignores the rule of systematic doubt, of critical judgement, which is foundational to the scientific endeavour."44

The arguments
The logic, clarity and substance of the arguments against physics are problematic. With respect to Massey’s temporal imagination, throughout the arguments one could notice her systematic disregard of what was in the past (‘vestiges of that old imagination’, p. 266, in favour of ‘recent developments’). However, it is worth remembering that more recently does not necessarily mean better, and philosophers know this very well (the same cannot be said of social scientists). This observation aside, let us now follow the arguments themselves.
First, Doreen Massey says that we should contest the authority of physics because it ‘was established in relation to, and in the days of, a much older form of physics’ (p. 264). It seems in this passage that contemporary physics is not worthy of the trust of older classical physics (this would be a unique case in which the past is not disregarded)! However, on the following pages it is that oldish physics

44 In his classic account, Merton 1973 (orig. 1942) spoke of the norm of ‘organised scepticism’ as the very essence of science.
which is criticised. Thus, ‘there is a considerable literature
denyng the view of “physics” (in classical mechanical
 guise) as …the purest form of scientific knowledge.’(p. 265)
and on page 267 there is a suggestion to ‘overthrow some of
our…fascinations with nineteenth-century physics…’.
Similarly, as the argument goes by, the depiction of recent
physics shifts from apparent circumspection (p. 264) to an
almost enthusiastic-optimistic attitude (p. 269). But what is
somehow reassuring is that in actuality this ambiguous ballet
between criticising / applauding old versus new physics does
not really matter in the discursive economy of the paper. The
overall conclusion - to move away from physics and towards
‘complex sciences’- seems to have been established
irrespective of the (lack of) substance of the argument. As
Massey comments in the last end-note:

And anyway - a point which gives me pleasure and illustrates
the wider argument-some of chaos theory had its earliest
beginnings in meteorology; physicists were quite slow to take
it up… (p. 275).

Apart from reflecting about one’s personal preferences, you
might also notice that quite often meteorology is considered
to be a special branch of physics -the physics of the
atmosphere- and that, in the light of Smith’s (1995)
comments also, it is easily understandable why physicists
were so slow to take up chaos theory.

* I have chosen to analyse (what I take to be) the shortcomings
of Massey’s paper, namely the insufficient engagement with
the discipline of geography (its history, its critical
graphy, etc.), and the unconvincing argument concerning
the inadequacy of physics (see fig. 14).
Fig. 14. Struggles over geography and physics: argument structure in chapter 10
Pragmatic scepticism cherishes the values of scepticism, balance, practicality, and tolerance. So far my analysis of Massey’s paper has been too sceptical. To capture my overall attitude towards her paper, it is also necessary to mention two of its undoubted merits: first, Massey goes beyond the all-to-frequent strategies of ‘making-up’ arguments for supporting the fragile *institutional* unity of the discipline, and is concerned with producing *substantive* commonalities across geography’s divides, through engagements in common possible conversations. Second, by becoming interested and struggling to understand theoretical geomorphology (see her references), a field apparently so far from critical human geography, she provides the valuable evidence that crossing the divides is still possible. Although my mind remains sceptical, my heart hopes that one day more geographers will follow her example. It is my strong belief that this is the pragmatic key to (re)integrating geography.
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