Styles of thinking: assessing and developing Ian Hacking’s historical epistemology

Thesis

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Styles of Thinking:
Assessing and Developing
Ian Hacking's Historical
Epistemology

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Submitted to the Research Office of The Open University
for the Degree of
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by
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I should also mention that when I was in my writing up stage my mother fell ill. In those difficult months, had she not urged me to keep on pursuing my objectives I would have found it very difficult, perhaps impossible, to finish this dissertation. There are also a lot of other people that I want to thank, including those friends to whom I have not been able to devote much of my time during this long period of study. To them I say: I do hope that both the endurance in carrying out this project and the intellectual progress I have made have given me the wisdom necessary to be better than I was before.
In the 1980s the Canadian philosopher of science Ian Hacking outlined what he later called the project of scientific styles of thinking and doing, for short the 'styles project'. His main objective was to investigate the philosophical significance of the thesis that there exist distinct scientific ways of reasoning which have emerged and stabilized at different points of the history of the sciences. Although over the last forty years Hacking has added to his original nucleus of ideas several constructive suggestions, his original thesis has never been fully developed into a comprehensive theory. This dissertation can be placed between an assessment of Hacking's claims and an attempt to develop, correct and present in a more systematic way the styles project so to make of it something coherent and complete, what might be called the 'theory of scientific styles of thinking and doing'. Since to infer philosophical implications from Hacking's project and analyse them in order to pursue these two aims has been central to my methodology, my dissertation is also an analytical account of the styles project and its philosophical implications.

A few papers have captured my attention during the early stages of this dissertation. The first ones were Hacking's papers 'Language, Truth and Reason' and "Style" for historians and philosophers', respectively published in 1982 and 1992. In those writings, Hacking discussed the key expression 'style of reasoning', which he later abandoned in favour of the expression 'style of thinking and doing' to underline that in order to find out we do not only think but we also act. The
original label 'style of reasoning' came from the book *Styles of Scientific Thinking in the European Tradition* by the historian of science Alistair Cameron Crombie (1915-1996). In that work, the latter listed six major styles of argumentation or, as he conceived them, six *methods* of scientific enquiry and demonstration central in the formative period of Western scientific thought:

a) The method of postulation exemplified by the Greek geometry, which consists of proof by deduction on the basis of explicit axioms;

b) The method of experiment and measurement both to control postulation and explore by observation;

c) The hypothetical construction of analogical models in order to explain unknown properties of phenomena;

d) The ordering of variety by comparison and taxonomy;

e) The statistical analysis of regularities of populations and the calculus of probabilities.

f) The historical derivation of genetic development, a way of explaining the present as a development of the past regulated by certain laws.

In Hacking's hands the notion of style acquired a richer physiognomy with philosophical traits. He did not feel compelled to accept all the styles in Crombie's list, or their sequence and specific trajectory in time, and suggested that there have also been other styles.

When I started to reflect on these ideas I realised that, in order to understand whether the notion of style is useful for building up a picture of scientific reasoning, it was crucial to describe it more accurately. In particular, at a certain stage of my analysis of Hacking's notion it became important to make
clearer which modes of thinking really deserve the label ‘style of thinking and
doing’. These issues will be addressed in the second and in the third chapter of my
dissertation, after the literature review (chapter one). In those chapters, I shall
argue that it is possible to identify a set of features characterizing a style of
thinking and doing. Then I shall show that at least six ways of scientific thinking
satisfy those features and deserve the label ‘style of thinking and doing’, for short
‘SoT’. Some of them belong to Crombie’s list; others have been introduced by
Hacking or only mentioned by him. My analysis of these six SoTs will show that,
even though they share common properties, they also are very different in scope,
in their object of study and historical trajectory. Finally, in the fourth chapter I
shall argue that other ways of reasoning, which some scholars have presented as
SoTs, actually do not satisfy all the characterizing properties of the SoTs and
therefore should be considered as ‘spurious SoTs’. In sum, the result I shall
achieve in chapter two, three and four is that it is possible to characterize what a
SoT is and use this notion for providing a description of modern scientific
thinking and doing.

In his papers of 1982 and 1992 Hacking put forward the claim that SoTs are
modes of reasoning that emerge in particular historical periods and determine
what counts as rational or irrational. In particular, according to him, SoTs bring
about new sentences as candidates for being true-or-false: whether or not a
proposition possesses a truth-value depends on whether we have ways to reason
about it. For instance, in Renaissance medical textbooks we may find statements
that are unclear to us; Hacking maintains that this does not happen because they
are false when judged from our current mode of reasoning, but rather because
what settles their truth-value is only the style of reasoning of the Renaissance thinkers itself.

I took Hacking’s claims above as a particular philosophical position within a line of thought that emerged at least back in the 1930s. In those years several philosophers and historians, including Ludwig Fleck and Gaston Bachelard, mentioned by Hacking on several occasions, claimed that the analysis of history of thought reveals ways of reasoning very different from one another. Indeed, their models, although different, share a common nucleus: intellectual history is to be regarded as made of several disparate worldviews. This view may well be considered as a reaction to the logical positivists who, impressed by the exciting scientific advances of their time, considered science as a model of rationality and its growth as cumulative.

The ‘outside time and history’ investigation of scientific activities conducted by analytic philosophers of science had also been called into question in the 1960s and 1970s by philosophers of science such as Karl Popper, Thomas Kuhn and Paul Feyerabend. They rejected the idea that science simply grows by accumulation and proposed that earlier results may be rejected, replaced, or reinterpreted by new theories or worldviews. Nevertheless, these philosophers disagreed about how to interpret history of science. For instance, Popper believed that progress is achieved when one and the same episode displays both incorporation and overthrow; for Kuhn, on the other hand, periods of ‘normal science’, in which progress is achieved by incorporation, are followed by revolutionary overthrows that might accomplish discontinuous elevations of the level of scientific practice.
Given this diversity of positions within the debate on the evolution of scientific thought I considered it important to identify the specific line of thought to which the styles project belongs. This issue will be addressed in chapter five: I shall argue that the SoTs project can be better understood as the development of 'classical historical epistemology', a philosophical tradition born in France in the beginning of the twentieth century, when different thinkers started to reconstruct the historically situated conditions of scientific concepts.

In the rest of chapter five I shall compare and contrast the notion of SoT with the conclusions of other scholars. This comparative analysis will allow me to examine the concept of SoT from different perspectives so as to reveal its philosophical richness. The many-sided nature of the notion of SoT that will emerge explains why it is applied in many different areas of research. Ever since Hacking introduced this notion, many scholars have used it for investigating the history of concepts and of the sciences, the epistemic differences between distinct approaches to scientific research, the relationship between history and philosophy and even issues of learning and teaching science.

Another paper, 'Hacking's historical epistemology: a critique of styles of reasoning' by the philosopher Martin Kusch, prompted me to look into a question (discussed in chapter six) that became of crucial importance in the context of my research: does the notion of SoT imply a form of relativism? The conclusion I shall reach in chapter seven is that there are cases in which a scientific claim is justified for a community that adopts a certain SoT and unjustified for a community that adopts another SoT. This fact implies a form of epistemic incommensurability between different SoTs. The specific case that I shall examine refers to Hacking's
claim that several theoretical entities of physics exist. I shall argue not only that this claim is justified only for a community that adopts a given SoT but also that it engenders a problem of consistency within Hacking's philosophy. Indeed, the styles project - I shall argue - is at odds with Hacking's anti-relativistic stance and in particular with his realism about theoretical entities. To corroborate my findings, in chapter eight I shall compare Hacking's claims concerning SoTs with some other claims of Ludwig Wittgenstein in *Philosophical Investigations* and *On Certainty*. This parallel will help me to reveal the anti-foundationalism and the epistemic relativism inherent to the SoTs project. Further questions concern the role of contingency in the history of science and whether science approaches toward the truth in the long run. In chapter nine I shall discuss to which extent the emergence of SoTs is a contingent circumstance, why they are long-lasting, whether science is bound to converge on a single answer to a given question and, finally, whether science converges on truth in the long run.
Chapter One: Plan of the Work and Literature Review

1.1 Introduction

The structure of this dissertation, which I have outlined in the previous pages, mirrors the different topic areas intersected by my research. Before focusing on each of these areas, I shall briefly restate their main focus. My research will mainly consist of four steps corresponding to four research issues:

• The first research issue is summarized by the following questions: what is meant by the label ‘styles of thinking and doing’? Which common properties do styles possess? How can styles be distinguished from other ways of reasoning? I shall argue that there is a set of styles of thinking and doing that share all the properties mentioned by Hacking over the years: I will refer to them with the acronym SoT.

• My successive step will be a philosophical analysis of the notion of SoT. Some relevant questions in this connection will be: what is the philosophical background of the notion of SoT? How does it fit into the area of research of historical epistemology? What does a comparison between the SoTs project and other similar projects suggest?

• The third research issue will be my core research question: does the styles project imply epistemic relativism? Are SoTs incommensurable? Do certain
implications of the notion of SoT gybe with Hacking's realism about theoretical entities?

- Finally, the answers to the previous questions will produce further crucial questions: why do SoTs emerge? Why are they long lasting? Is scientific process bound to converge to a single answer to a given question? Does the evolution of the sciences converge on a true, coherent description of the world?

Below I shall devote a section to each of the four research issues outlined above. The aim of each section is to explain better and justify my research purposes in the light of the literature in my field of research.

1.2 Characterising Styles of Thinking and Doing

As I have mentioned, Crombie suggested that in the history of the sciences it is possible to distinguish six styles of scientific reasoning still practised in the modern sciences, whose development has been continuous from the epoch of the ancient Greece until now: the style of geometry, the experimental style, the style of hypothetical modelling, the taxonomic, the statistical and the historico-genetic style (Crombie 1978, 1988, 1992, 1994). His aim was to use his notion of style for describing how conceptions of nature and science, methods of inquiry and moral commitments have characterized Western natural science. Hacking modified Crombie's list in some respects (Hacking 1982, 1992d): he introduced the laboratory SoT and considered *Leviathan and the Air Pump* (Shapin and Schaffer 1989 [1985]-b) as a book about its origins; he also spoke of the now extinct
hermetic style of Paracelsus (Hacking 1982, 1983b, 1992d). In addition, he just
mentioned the Indo-Arabic style of applied mathematics, interested in finding
algorithms and distinct from the style of geometry (Hacking 1992a), and hinted at
the possibility of other SoTs (Hacking 1982) (Lakoff 2012).

Beside these suggestions he made precise claims about the features of SoTs
claimed that a style introduces new types of objects, evidence, laws, possibilities
and new true-or-false sentences, i.e. sentences whose truth-value hinges on the
SoT itself (for this reason they are called style-dependent sentences). Moreover, in
striking contrast with Crombie's continuism, he suggested that SoTs have sharp
beginnings and that some of them (e.g. the statistical and the laboratory style)
For him SoTs are 'self-authenticating' (Hacking 1982, 1983a, 1988a, 1991a, 1992b,
1992d, 1992c, 2007a, 2009, 2012), a term that refers to the circularity induced by the
following double claim: the truth of certain sentences is what we find by using a
SoT; in turn, a SoT is a standard of objectivity because it gets at the truth.

Hacking's views about SoTs evolved over the years: in recent papers, he put
emphasis on the idea that a style is a way of thinking and a way of doing, i.e. a way
of intervening in the world in order to know (Hacking 2012); furthermore, he
added that a style is rooted in human innate capacities and that it is a way of
finding out (Hacking 2009, 2012).
1.2.1 The Identity of Styles of Thinking: Unsolved Issues

Hacking's account of styles left several issues unsolved. First of all, it is not clear whether or not all the SoTs he mentioned actually possess all the properties he added over the years. For example, he did not put forward any argument for showing that styles such as the historico-genetic and the taxonomic one do introduce new types of objects, evidence, laws, possibilities and new candidates for truth or falsehood. If anything, in the literature there is no account of the reasons for considering them as SoTs of the same genus as styles such as the laboratory SoT. Strictly speaking, from Hacking's works it does not emerge why the postulational SoT introduces new evidence, why it is a way of doing and whether its birth is to be considered a discontinuous event in the history of thought. Still less clear is which properties other styles such as Paracelsus' one share with the styles in the Crombie's list; or whether the algorithmic style deserves the label 'SoT'. Another problem with Hacking's claims is that he does not say whether and why being self-authenticating is a feature of all the styles he mentioned. Is self-authentication a feature that should characterize each SoT? Or does it characterize only some of the most important scientific styles of thinking and doing? There is no clear answer to these questions: he maintained that self-authentication marks a SoT out as scientific and enable it to be stable over time (Hacking 1996); however, in previous papers (e.g. (Hacking 1992d) he had noted that every style has its own techniques of self-authentication without specifying which ones they are. Still less clear is whether a SoT is to be understood as a long-lasting way of thinking: on the one hand Hacking seems to consider endurance as a feature of SoTs, on the other hand he gives examples of extinct styles.

What makes things worse is the fact that it is unclear whether a SoT can be identified by necessary and sufficient conditions. Although he has often been
unwilling to state general characteristics of SoTs, his attitude towards this issue has been wavering: in 1992 he described certain features as necessary conditions (1992d, p. 14); in his lectures at the Collège de France he went further by speaking of necessary and sufficient criteria (Hacking, 2006-a); then, in 2012 he took a step backwards by denying the possibility of providing any identity conditions (Hacking 2012).

1.2.2 The Identity of Styles of Thinking: Unsolved Issues in The Literature

Of the many who have directed their attention to the notion of style, no scholar has really fully investigated these questions. Indeed, the secondary literature concerning the issue of identifying SoTs can be divided into two categories. On the one hand, there are scholars who have been mainly concerned with presenting new ways of reasoning as candidates for being SoTs; on the other hand, there are scholars who have focused on the criteria for establishing what a SoT is or who have modified the characterization of what a style is according to their views.

Among the scholars in the first group, Barry Allen has argued that, from the mid-fifteenth to the mid-seventeenth centuries, theologians and inquisitors who were committed to demonology shared a way of thinking that possessed some of the features illustrated by Hacking (Allen 1993). Forrester proposed to include, alongside Crombie's list, another style dominant in psychoanalysis and other disciplines: reasoning in cases (Forrester 1996). Schweber and Watcher claimed that mathematical modelling and simulation on computer constitute a new style of
reasoning (Schweber and Watcher 2000). In a different research context also A.I. Davidson employed Hacking’s notion of SoT as a tool for examining under which conditions we can comprehend various types of statements as true or false (Davidson 2001), and argued that around 1870 a new style of thinking emerged making possible new true-or-false statements about sexual perversions. Elwick used the label ‘style of reasoning’ to refer to two different sets of self-reinforcing norms in the pre-Darwinian research in biology (Elwick 2007). He noted that, in order to find out about questions concerning the development of embryos, some researchers adopted the style of analysis/synthesis, according to which the development proceeded through the fusion of parts, other researchers adopted the style of palaetiology, according to which development proceeded through a progressive specialization of the organism. Bensaude-Vincent argued that chemistry is a style of thinking by showing that it possesses all the features that Hacking attributed to his notion, including the existence of techniques of self-stabilization (Bensaude-Vincent 2009). Kusch noted that, following Hacking, the Christian-Catholic style should be included among the scientific styles (Kusch 2010). Finally, Belfer introduced an ‘Information Laden scientific style of reasoning’: according to him the impact of Information Theory on science produced a way of reasoning that satisfies Hacking’s characterization (Belfer 2011).

A point to be noted is that none of the authors of these studies has included among their criteria of identity all the features added by Hacking over the years. For example, both Allen and Kusch have not insisted on the point that a style in Hacking’s sense must be a way of doing and a way of finding out about
phenomena. This circumstance has led to a proliferation of the number of styles, a consequence that Hacking has always struggled to avoid by trying to stick closely to Crombie’s list (see Hacking 2012). In this sense, what the studies above make evident is the necessity of a better characterization of Hacking’s styles.

As to the second group of studies, Gayon has focused on the uses of the term ‘style’ in history and philosophy of science (Gayon 1996). His analysis explained how Hacking’s use of the term differs from other uses in literature but did not clarify which forms of reasoning can be considered as SoTs. Gayon pointed out that previous uses of the term expressed the idea of the singularity and the locality of the ways of knowing; in the hands of Hacking the meaning of the term ‘style’ acquired the idea of universality. Indeed, the category of style is a collective and historical product that, as we shall see, constitutes the base of objectivity.

Kusch moved his attention from the use and the meaning of the term ‘style’ to the issue of characterizing styles, which I consider relevant (Kusch 2010). He rightly maintained that the criteria of identity put forward by Hacking do not avoid the proliferation of styles: for instance, according to Kusch, Hacking cannot exclude Christian-Catholic reasoning. However, he did not undertake the task of offering convincing criteria for identifying SoTs.

More recently, the workshop on SoTs held at the University of Cape Town in 2011 left aside the problem of the number of styles posed by Kusch and returned to focusing mainly on the meaning of the notion of style. For example Winther analysed the relation between styles, paradigms à la Kuhn and scientific models (Winther 2012); Kwa looked at the origins of Crombie’s concept of style,
which for him can be found in art history (Kwa Unpublished). Other scholars drew on Hacking’s works to put forward a different notion of style of thinking. For example, Bueno considered a different understanding of Hacking’s concept of style: his notion of ‘narrow style of reasoning’ made room for the diversity of scientific practices within Hacking’s styles (Bueno 2011). Characterizations such as Bueno’s twist Hacking’s original intuitions and determine a proliferation of styles. As I want to characterize the notion of style by being faithful to Hacking’s understanding, I shall disregard them.

1.2.3 What a Style of Thinking is: Filling the Gaps

Ultimately, notwithstanding the considerable amount of interest in Hacking’s notion of SoT, there is still a gap in the literature: there are no studies that identify unambiguously what is meant by the label ‘style of thinking’. To fill this gap I shall argue that there are six ways of thinking that share a few precise features. I shall name these particular ways of thinking and doing with the acronym ‘SoT’ to distinguish them from other ways of thinking that do not possess the same features and somehow are spurious -- species of a different taxonomic category. I shall provide a description of these SoTs in order to show that they share certain properties, which I have called the SoTs’ characterizing properties (e.g. the property of introducing new true-or-false sentences and new evidence).

The first SoT I have described is the statistical one. Although Hacking never mentioned the notion of SoT in *The Emergence of Probability* (Hacking 2006 [1975]) I shall interpret his book as an account of the birth of this SoT. In particular, I shall explain how the coming to the fore of a new form of evidence in the seventeenth
century, described in that book, made possible the emergence of the statistical SoT. By doing so I shall put in connection different ideas of Hacking in different papers (e.g. Hacking 1992c), so to provide a complete account of the statistical SoT.

Another way of thinking and doing that satisfies the SoTs characterizing properties - I shall argue - is the Laboratory SoT, of which Hacking has written extensively but unsystematically in different works, e.g. (Hacking 1988a, 1991a, 2006d, 2007a, 2009). *Leviathan and the Air Pump* (Shapin and Schaffer 1989 [1985]-b) will be crucial to the construction of my systematic account: I shall reinterpret their book in the light of the SoTs project.

I shall also claim that another way of thinking, the algorithmic one, dedicated to finding algorithms in applied mathematics, which is not in Crombie’s list and has only been barely mentioned by Hacking (Hacking 1992d) (Hacking, 2006-c)(Hacking 2009), is a SoT. My argument for the thesis that this SoT satisfies the SoTs’ characterizing properties is illustrated by historical examples.

The fourth SoT I shall deal with is the postulational SoT. Netz reconstructed the missing diagrams in the Greek texts and provided an account of the emergence of demonstrative proofs in mathematics (Netz 1999). Although Hacking claimed that Netz’s work can be considered as an account of the crystallization of the postulational SoT (Hacking 2009), he did not provide a systematic account. I shall use Netz as a primary source for a comprehensive description of the postulational SoT but I shall also rely on other sources such as Hoyrup (Høyrup 1990b, 1990a; Høyrup 2010, 2012), which will be crucial for supporting Hacking’s view that the emergence of the postulational SoT represents a discontinuity in history of mathematics. Finally, Lloyd’s writings will help me to
identify the contingent factors that allowed its coming to the fore (Lloyd 2000; Lloyd 2004).

Finally, Hacking explicitly wrote that he has yet to provide a characterization of the historico-genetic and the taxonomic ways of thinking (Hacking 2012). I shall take on this task: my conclusion will be that they satisfy the SoTs' characterizing properties. As for the historico-genetic SoT, Mayr and Cleland made important points that help to distinguish the kind of explanations provided in laboratory research from that provided in the historical sciences (Mayr 2004) (Cleland 2002). I shall develop their points as well as those of other scholars by using examples drawn from the history of different historical sciences in order to provide a full characterization of the historico-genetic SoT. Similarly, scholars such as Atran, Mayr and Gould will be important sources for identifying the mental actions that characterize taxonomic reasoning (Atran 1990; Mayr and Bock 2002) (Gould 2000a); I shall also develop some ideas of Foucault in order to show that the taxonomic SoT introduces true-or-false sentences, as Hacking claims, and marks a discontinuity in the history of thought (Foucault 1994 [1966]).

To summarize, the first research issue of this dissertation concerns the possibility of identifying clearly the features of what Hacking calls 'a scientific way of thinking and doing'. My study will help to shed light on different unsolved issues: in literature there is no systematic characterization of the SoTs mentioned by Hacking; nor is it clear what can be considered a SoT and what cannot. Furthermore, it is unclear why, as Hacking recently claimed (Hacking 2012), we should stick to Crombie's SoTs and why each of them is self-authenticating.
1.3 Hacking's Historical Epistemology

Ever since his first papers on SoTs (Hacking 1982, 1992d), Hacking has maintained that his aim is to continue Kant's project of explaining how objectivity is possible. Indeed for Hacking SoTs represent the a priori conditions that make certain propositions objective, i.e. a candidate for truth or falsehood. In as much as for Hacking objectivity is made possible by SoTs that emerge at different points in time, it is possible to say that his aim is to historicize Kant. In chapter five, I shall ask: how can this project be better understood? I shall argue that it falls into an area of research called 'historical epistemology' (henceforth HE), of which Ian Hacking, Lorraine Daston, Peter Galison and Arnold Davidson are some of the most known practitioners. My analysis will shed light on the philosophical import of the notion of SoT.

The point that the SoTs project falls into the field of HE is not as obvious as it may appear. Hacking maintained that his project is different from those undertaken by other historical epistemologists (Hacking 1999a) and can be viewed as part of 'cognitive history' (Hacking 2009, 2012) (term borrowed from Netz (Netz 1999)), i.e. a study of how humans learned to use their cognitive resources. In contrast with Hacking, I shall maintain that the SoTs project is a work of HE because the latter asks what made certain propositions objective: in as much as SoTs may represent the answer to this question his project can be considered as part of HE. Although Kusch supports my thesis (Kusch 2010), his argument relies on a conception of HE as a program of historicization of epistemology, while I
shall insist on the fact that the SoTs project fulfils the aims of HE in that it asks questions about the preconditions required for the existence of certain ideas. My next step will be to provide a broader framework to the theory of SoTs by tracing out its connections with a tradition of thought born in France in the beginning of twentieth-century, when in different periods thinkers such as Lucien Lévy-Bruhl (1857-1939), Léon Brunschvicg (1869-1944), Gaston Bachelard (1884-1962), Georges Canguilhem (1904-1995) and Michel Foucault (1926-1984), started to reflect on the historical conditions of knowledge (see for example (Lévy-Bruhl, 1992 [1910]) (Brunschvicg, 1912) (Brunschvicg, 1934) (Bachelard, 1984 [1934]) (Canguilhem, 1968) (Foucault, 1969 [1972]) (Foucault, 1994 [1966])). By relying on studies that reconstruct these thinkers' research projects or discuss their contributions, e.g. (Chimisso 2003, 2008, 2010) (Gutting 2005) (Brenner 2006) (Gingras 2010) (Castelão 2010) (Rheinberger 2010) as well as by examining primary sources, e.g. (Foucault 1969 [1972], 1994 [1966]), I shall trace the lineage of some ideas of the SoTs project back to the French scholars above and thinkers such as Ludwik Fleck (1896-1961) (Fleck, 1969). Although Castelão (Castelão 2010) and Rheinberger (Rheinberger 2010) include Hacking in their discussions, mainly focusing on his reflections about the interventional character of modern science, the literature lacks a discussion of the philosophical influences on Hacking as far as his SoTs project is concerned.

In the second part of chapter five I shall offer different perspectives on the concept of SoT by comparing Hacking's ideas with those of thinkers such as Daston and Galison (Daston and Galison 2007). Their historical analysis is important for testing Hacking's claims. Kusch claimed that there is enough
resemblance between Hacking's investigations into SoTs and Daston's historical studies of epistemic virtues to justify the label 'historical epistemology' for both projects (Kusch 2011c). However, he did not address my question: whether or not their conclusions are consistent.

Another work that helps to shed light on the philosophical meaning of the notion of SoT is the book *Truth and Truthfulness* by the philosopher Bernard Williams (1929-2003). By drawing on Williams' book Hacking suggested a new perspective from which the concept of SoT can be looked at (Hacking 2004, 2005d, 2009). Williams showed that in different historical periods there have been different commitments to truthfulness; Hacking generalized Williams' analysis to the domain of scientific knowledge and interpreted the emergence of a new SoT as a change in conception of what it is to tell the truth about different aspects of the world (Hacking 2004, 2005d, 2009). Among the scholars who have discussed the SoTs project only Kusch (Kusch 2010) and Wanderer (Wanderer 2012) paid attention to Williams in their discussions. However, Kusch limited himself to presenting Williams as Hacking's ally; while Wanderer took into account Hacking's thoughts on Williams only in order to discuss the self-authenticating character of SoTs. For my part, I shall further elaborate the idea of SoTs as different ways of being truthful that Hacking drew on the work of Williams. By doing so I shall highlight another reading of the notion of SoT, which has not clearly emerged in all the writings about SoTs, including Hacking's. Indeed, as I shall argue, SoTs select and exclude aspects of their objects of study and therefore can be viewed as different perspectives on them, i.e. as different ways of looking at the world.
1.4 The incommensurability of styles of thinking

For Hacking the truths discovered by reasoning in a certain way are independent of how we have found them, i.e. truth is external to history. In other words, although only if we reason in a particular SoT can we attribute a truth-value to certain sentences, their being true or false has nothing to do with the fact that we reason in a particular SoT. Regarding this point, I think there are two important issues to look into. The first one is to examine the SoTs project in order to confirm Hacking's point that his characterization of SoTs is consistent with the claim that truth is external to history. Strictly speaking, this issue amounts to asking whether the SoTs project implies alethic relativism: can a claim be true in a SoT and false in another? Is Hacking's characterization coherent with a negative response to this question?

The second issue concerns epistemic relativism. In particular, the possibility of having an atemporal and independent criterion for justifying those sentences whose sense hinges on a given SoT. My question amounts to the doubt that a claim expressed by certain sentences (whose sense hinges on a certain SoT) could be justified only within their SoT, i.e. by using the standards of evidence, methods and the way of thinking and doing of that very SoT. As I shall explain in chapter six it can also be said that my question concerns the incommensurability of SoTs: the claim that there might not be common standards of evaluation external to the SoTs.
1.4.1 Why the Relativism Issue is Important

The importance of the question I have posed lies on the fact that, as I shall explain below, no univocal answer has emerged from the discussions on this issue in literature. Its importance arises from the further fact that whether or not the SoTs project invites relativism can be considered a subproblem of a wider issue hotly debated: is relativism a philosophical consequence of the historicization of epistemology? For example, Chimisso pointed out that the historicization of epistemological concepts carried out by the French thinkers such as Brunschvicg opened the door to relativism (Chimisso 2008, p. 70). More in general, Kusch reminded his readers that the relationship between historicism and relativism represented a problem for discussion already a century ago (Kusch 2010, p. 168). Nor can it be forgotten that another continental thinker, Fleck, who introduced the notion of ‘thought style’ and influenced Kuhn and Hacking, explicitly spoke of incommensurability of concepts and ideas. Although he did not put forward a systematic argument for the thesis of incommensurability between thought styles, in his writings it is possible to find many passages that suggest this thesis. Indeed, in Fleck incommensurability directly follows from the fact that he historicizes a great deal of elements involved in the knowledge of the world. From a thought style to another everything changes: the way we perceive reality, the language used to communicate, the research problems, the standards of truth and the reality created by the thought style itself (Fleck 1979 [1935], p. 100).

The relativism issue continues to be relevant today in relation to historical epistemology. In Objectivity Daston and Galison took care of remarking that relativism has nothing to do with their account of objectivity. I interpret this excusatio non petita as the symptom of the general worry, among historical epistemologists, about the relativistic implications of their conclusions. Whether or
not truth has a history – Daston and Galison wrote - cannot be concluded from the fact that the means devised to attain it vary over time (Daston and Galison 2007, p. 377). Maybe so, but this does not provide an answer to the tricky question of establishing how far we can go with our theses about what varies without ending up stating that there is no universal and atemporal justification for our epistemic claims. Hacking has been trying to allay any suspicion of relativism ever since he wrote his first papers on SoTs. For example, he insisted on the idea already mentioned that a style is characterized by a set of propositions that are true or false, not a set of true propositions, i.e. the actual truth-value of style-dependent sentences is external to the SoT. He distinguished form from content of science and remarked that, although the question of science are historically variable, the answers are not (Hacking 1999b, 2000a). That is: in a given historical context, once a question is fixed, there is only one correct answer to that question; no relativism about answers. By putting forward these ideas Hacking also distanced himself from those social contructionists such as Shapin and Schaeffer who spoke of the social construction of the actual answers to well asked questions (Hacking 2000a). As I shall show, Hacking’s defence is not sufficient to escape the accusation of epistemic relativism, although the SoTs project does not imply alethic relativism.

1.4.2 Solutions to the Relativism Issue in the Literature

Before illustrating which resources have been important for arguing for these theses, I shall examine the different contributions in literature regarding the relation between the SoTs project and relativism. Some of these contributions
propose a different thesis from mine, others present arguments that, in my view, do not really hit their target.

Newton-Smith claimed that the point in (Hacking 2002 [1982] –a) that a proposition can be determined as true by a SoT for which there is no external justification implies relativism (Newton-Smith 1982). Although Newton-Smith added that whether or not Hacking thinks that there are external justifications is not really clear to him, he suggested that one should describe Hacking’s ideas as a form of relativism, which he called ‘muted relativism’. However, this point did not prevent Newton-Smith from saying that it would possible to find a way out of the relativism issue by formulating an independent criterion to justify SoTs in terms of style-independent sentences (‘observational statements’ whose sense does not hinge on their SoT). However, Newton-Smith did not exhibit this criterion nor did he discuss how it could be found — one can suspect that this criterion does not exist.

Jardine mooted the possibility that one could show the superiority of the methods of a given SoT $M$ against those of a past one $M'$ (Jardine 1991). He imagined the case in which $M'$ is ancestral to $M$ and the local methods and the practice of $M'$ have gone through successive stages of modifications in the body of local practices: the outcome of these successive refinements, displacements and replacements of methods is $M$. Under these circumstances, Jardine concluded, it is possible to consider $M$ to be more reliable than $M'$ because derived from $M'$ by a sequence of reliability enhancing steps (Jardine 1991, p. 167). It will be clear later that Jardine’s point cannot be accepted in the light of Hacking’s insistence that the evolution of methods and criteria of evidence is discontinuous. A case in point is
that of Laboratory SoT. According to Hacking, SoTs may be characterized by methods that have not been calibrated against previous ones. He follows Shapin and Schaeffer in thinking that the air pump is the emblem of a new way of providing evidence: to create effects that did not exist in isolation. There is no possibility of considering this new way of doing as derived from that of another SoT through successive refinements.

Baghramian, as Newton-Smith did, pointed out that Hacking does not propose a form of alethic relativism: what varies from culture to culture is the availability of propositions, not their truth-value (Baghramian 2004). For her, Hacking's account is an example of relativism because for him what counts as evidence is internal to a given SoT. However, even without an independent criterion and by using only the norms internal to one's SoT, she suggested, it is possible to become convinced that certain questions are better addressed by the methods of an alternative style of reasoning. I consider this suggestion unjustifiably optimistic, as it will be clear.

As Kusch stated, epistemic relativism is the claim that the properties picked out by the predicates 'rational' and 'justified' are relative to different epistemic systems (Kusch 2010). For him this definition fits the case of SoTs: a style-dependent sentence can be justified in its own SoT but is meaningless outside of it; therefore its justification is relative to its SoT. The problem is that this point only countenances a possibility: the relativism issue in the context of the SoTs project remains unsolved until it is found an example of a scientific claim that is justified in a SoT and unjustified in another. Kusch said that an example that involves two scientific SoTs in the Crombie's list would not do the job because 'familiar forms of
epistemological relativism do not assume that one and the same individual possesses more than one epistemic system’ (Kusch 2010, p. 167). Therefore he brought in Christian theology and argued that it is a SoT; then he concluded that the sentence ‘God told me that I should do his will from day to day, in humility and poverty’ is rational and justified for a Christian but not rational and justified for him (Kusch 2010, p. 168). Indeed, he considers the Christian style incompatible with his commitment to other scientific styles.

I see at least two problems with this argument. First of all, Kusch did not provide any example of a scientific claim that is justified in a SoT and unjustified outside it, rather an example of a claim that, he thinks, is incompatible with one’s commitment to science. Consequently, Kusch left a doubt: in absence of an example of scientific claim one does not know whether, within science, there is incommensurability or not. Indeed, think of two different communities A and B that at different times adopt different SoTs; could B justify all the scientific claims made by A? This question remains unanswered. In sum, the lack of an investigation on the relation between relativism and the SoTs project, viewed as it is meant to be, that is a project on scientific ways of knowing, seems to me a serious gap in the literature.

Furthermore, Kusch noted that the same individual can adopt different scientific SoTs but added that for him the Christian style is incompatible with them. So there is compatibility between two scientific SoTs and there might be incompatibility between a non-scientific SoT and a scientific SoT. Kusch did not provide any argument to account for this asymmetry. In absence of arguments one is led to thinking that the incompatibility between the Christian style and a
scientific SoT lies on the fact that what is considered true in the Christian style is not true in a scientific style. If this were the case one could push further his argument by saying that the SoTs project does not only imply epistemic relativism but also alethic relativism, a conclusion that, as I shall argue, is untenable. Otávio Bueno disagreed with Kusch by claiming that Hacking has resources to avoid relativism (Bueno 2012). For him, statistical analysis could provide the relevant common standard to assess certain propositions of the laboratory SoT. In personal conversation he clarified his point by maintaining that the laboratory and the statistical SoTs have common standards. I disagree with this conclusion and the reason will be clear in chapter seven.

To summarize, from the SoTs project emerges the question whether or not Hacking’s ideas imply epistemic relativism. Despite Hacking’s dissent, most of the commentators claimed that the SoTs project does imply a moderate form of relativism. However, a few maintained that within Hacking’s project there is room for a universal criterion for comparing different SoTs. Unfortunately, none of them has put forward such a criterion. Consequently, the literature lacks a thorough investigation about its existence. Inter alia, my thesis will show that this criterion cannot exist within the framework of Hacking’s characterization of SoTs.

Finally, of those who claimed that the SoTs project implies epistemic relativism none has provided examples of scientific claims that are justified only within a certain SoT. Still worse, Hacking has been elusive on the core topics of justification. Notably, an in-depth analysis of how scientific claims are justified should have a role in any theory that claims the existence of different ways of finding out. This is for me another good reason to undertake this task.
1.4.3 My Solution to the Relativism Issue

I shall reformulate the incommensurability problem from the perspective of Edward Craig's approach to the concept of knowledge, known as State-of-Nature Epistemology. In 2011 Kusch critically related Craig's (and Williams') approach to Daston's historical epistemology (Kusch 2011a), but so far nobody has connected it to the SoTs project. Besides providing a clearer formulation of the incommensurability issue, the critical dialogue between Craig and Hacking that I shall conduct will help me to confirm the correctness of the claim that the SoTs project does not imply alethic relativism.

My next step will be to argue that the SoTs project implies that the existence of unobservables such as, say, electrons and muons, is justified in the laboratory SoT and unjustified for a community that does not adopt it. As I shall explain, Hacking justifies his belief that unobservable entities exist on the ground that they can be regularly manipulated by experimenters in order to find out and produce various phenomena (entity realism); he also maintains that no belief that our theories are true is required in order to be realist about unobservable entities (theory anti-realism). I shall argue that his justification about the existence of particles is relative to the laboratory SoT.

One of the outcomes of my general argument is that Hacking's entity realism (i.e. the claim that we know about unobservable entities) is at odds with the relativistic implications of the SoTs project. Both realism and the concept of SoT are relevant topics of Hacking's Representing and Intervening. It is surprising that, despite of the fact that this book is one of the widest read introductions to philosophy of science, and that several scholars analysed Hacking's realism (Morrison 1990; Shapere 1993; Resnik 1994; Reiner and Pierson 1995; Vallor 2009), none has asked whether the latter jibe with the concept of SoT.
I shall also provide a more general argument (not focused on the laboratory SoT) for the claim that the SoTs project implies epistemic relativism. It consists of a critical comparison between Hacking’s SoTs project and Wittgenstein’s *Philosophical Investigations* (Wittgenstein 1997 [1953]) and *On Certainty* (Wittgenstein 1995 [1969]). Such an analysis is lacking in the literature, although Wittgenstein and Foucault are the tools used by Hacking for doing philosophy. In particular, the origins of anti-foundationalism in the SoTs project as well as of the concept of self-authentication have not been discussed so far. My analysis will show that they can be connected to some of Wittgenstein’s ideas.

1.5 Styles of Thinking, Contingency and the Evolution of Science

Besides the incommensurability problem there is another aspect of the SoTs project that has important implications: since the emergence of SoTs is contingent, and SoTs are tools for finding out, the achievement of certain scientific results might not be an inevitable fact. In literature this issue is framed in many other different forms: for example, often it is asked whether a particular field of science could have taken a different route from the actual one or whether the achievement of certain scientific results has been contingent. Apart from the problem of framing it, the contingence issue includes many sub-problems since different items can be contingent: questions, methods, theories, laws of physics, form of equations, constants of physics, experiments. Moreover, each of these items
requires a specific analysis and its being ‘contingent’ is always relative to something to be specified. The immense importance of the contingency issue can be grasped by recognizing its bearing on the question of convergence: if contingency is a relevant feature of the evolution of the sciences, we might never achieve a complete description of a part of the world.

1.5.1 The Contingency Issue in the Literature

The contingency issue can be found in embryonic form in Fleck (Fleck 1979 [1935]): his doctrine that scientific facts exist only within thought styles raised the doubt that they are not inevitable at all, rather determined by contingent social forces. Although the idea of contingency was central to the work of many French philosophers such as Foucault, it did not play any fundamental role in the debates on the growth of knowledge that raged soon after the World War two in the Anglophone philosophy of science. With the advent of social constructionism the idea of contingency gained momentum. Shapin’s conclusion that reality is capable of sustaining different accounts given of it suggested that our theories might have been different from the actual ones. Pickering’s book Constructing Quarks (Pickering 1984) encouraged the thought that the quarky physics was not inevitable at all: the theories about quarks, the experiments performed to test them and that convinced the scientist of their existence represent the product of an historical process. The theme of contingence also played a role in some discussions on the interpretation of Quantum Theory. Cushing made a case for thinking that a plausible temporal reordering of certain scientific achievements could have engendered a deterministic world-view instead than an indeterministic one.
His point raised the important question as to whether or not science is bound to converge on a single answer to a given well-posed question or can take alternative routes at certain critical junctures.

Hacking, reflecting on these thinkers, brought the contingency issue to the fore in its full-fledged form. He framed the issue in (Hacking 1999b) and developed it with some variations in (Hacking 2000a, 2014) on the basis of ideas expressed also in (Hacking 1988b, 1992b, 1996) and later in (Hacking 2005b, 2005a). In particular, he prompted the following formulation of the contingency issue:

If the results R of a scientific investigation are correct, would any investigation of roughly the same subject matter, if successful, at least implicitly contain or imply the same results? (Hacking 2000a, p. S70)

This formulation generalizes the question posed by Pickering’s point as to whether a non-quarky physics, as triumphant as the high-energy physics that evolved after World War II, could have developed and become our current physics. In March 2006 the philosopher Léna Soler organized a conference on the contingency issue (Soler 2008a, 2008b) (Trizio 2008) (Sankey 2008) (Franklin 2008b), and a second one in 2009 with the participation of Hacking and Pickering.

One of the upshots of those discussions was that Hacking’s formulation of the contingency issue is only one of the many possible ways of framing the issue (Soler 2008a, p. 222-227). Those conferences made it evident how vast is the range of philosophical sub-problems of the contingency issue (Soler 2006, note 3 p. 364). For example, Soler showed how Hacking’s formulation of the contingency issue
above is ambiguous (Soler 2008b). Hacking substantially defined contingentism as the claim that a science *radically different* from the actual one, but as *successful and progressive*, would be possible. He also asked whether such a science, although different, should imply or reach, at least in the long run, the same results as the current science. The problem with this formulation is that, terms such as ‘results’, ‘successful science’ and ‘correct’ need clarification. Soler reformulated contingentism in terms of two successful physics that have developed in complete isolation from each other (Soler 2008b). Then, by means of a thought experiment, she focused on what ‘different’ should mean, that is what kind of differences these two physics should present for giving rise to a form of contingentism. However she left out the problem of clarifying what a ‘successful’ physics really is and did not focus on the adjective ‘correct’ as I have done in chapter seven and nine in criticizing Hacking. Trizio investigated the connection between inevitability and success by examining three conceptions of scientific success: truth, empirical adequacy and robust fit (Trizio 2008). One of the implicit results of his analysis has been to show how Hacking’s formulation of the contingency issue above is not neutral. Indeed, of course, an attribute such as ‘successful’ depends on philosophical commitments – e.g. a conception of success as truth implies inevitabilism - and Trizio clarified how. Very recently Hacking showed more awareness of the fact that the inevitability question is inextricably connected with the question of success although he did not provide any in-depth analysis (Hacking 2014, p. 117).

Sankey understood Hacking’s formulation in terms of convergence on a single unified theory of the word: for him a contingentist would hold that science,
properly conducted, might well have led to a completely different theory of the world from that of contemporary science (Sankey 2008). So he ended up discussing contingency in relation to realism. His conclusion was that, although realism does not entail inevitabilism (in the sense of convergence on a whole description of the world), the employment of reliable methods would in fact guarantee that science continues to move closer to the truth about a portion of the world but non-necessarily on the whole truth. On the other hand, Franklin discussed historical case studies in order to show that experiment provides constraints to the evolution of science (Franklin 2008a, 2008b). He concluded that in many cases there is no room for reasonable alternative options.

1.5.2 The Contingency Issue: a Gap in the Literature

I think it is unfortunate that so far the discussions above about the contingency issue have not involved the notion of SoT. Indeed, it is important to note that the account of SoTs given by Hacking and developed in the first two chapters represents a picture of the development of scientific thought: precise claims are made about when and why certain SoTs have emerged and developed so to allow the achievement of certain results. Therefore, that account can provide the framework in which the contingency issue can be more easily studied. Think for example that, as the theory of evolution provides the framework for a hypothesis of evolutionary relationships of species through time, so the SoTs theory presupposes a precise order in which certain SoTs have evolved. The same mental experiment, described by Gould in Wonderful Life (Gould 2000b, chapter one), of replaying the ‘life’s tape’ to see if the repetition would lead to the actual
phylogenetic tree could be conducted in the case of the evolution of SoTs. One could imagine pressing the rewind button and going back to the origins of the first SoT, the algorithmic SoT, in order to see if a replay of the tape would lead to a science different from the actual one.

1.5.3 My Take on The Contingency Issue and the Question about why Styles of Thinking Endure

My brief discussion of the works on the contingency issue in literature shows that no progress has been made as far as a formulation of contingentism is concerned. I prefer to discuss single questions by referring to concrete examples rather than discussing contingentism in general. In chapter nine I shall examine the contingency issue in the light of the SoTs theory by addressing four fundamental questions. First of all, I shall discuss to which extent the emergence of the SoTs at a certain point of history is a contingent circumstance. In the following section I shall ask whether the endurance of SoTs is inevitable by tackling the connected question as to why the SoTs are long-lasting. Afterwards, I shall focus on the growth of knowledge within a community that adopts one or more SoTs by dealing with questions such as: if a certain SoT continues to be employed and if Q is a 'live question', is science bound to converge on a single answer to Q? Finally, on the basis of my previous reflections I shall look into the question of the convergence of science in the long run.

As to the first question, my answer will be that the emergence of each SoT is the result of both contingent and inevitable factors, although there is no contingence in the order in which SoTs have appeared. As for the second question I shall put
forward the thesis that one of the causes of endurance of each of the six SoTs I shall present is, together with their self-authenticating character, the fact that they all are grounded in objective features of that portion of the world they study. No scholar, Hacking included (for reasons that will be evident later), has ever turned his attention to the possibility that a SoT endures because it is appropriate for dealing with particular objective structures of our world.

As for the third question, I shall agree with Hacking, although for different reasons, that the convergence of science on correct answers to single questions is inevitable; and that it is contingent that a given question has been asked. Finally, in relation to the fourth question, I shall argue that the idea of convergence of science on a single true, coherent and complete description in the long run is at odds with the following implication of the SoTs project: the growth of many different epistemological items is open-ended and never final and complete.
Chapter Two: The Styles Project

2.1 Introduction

Since the early 1980s, the term ‘style of thinking’ has become widespread in the literature of the history and philosophy of science. In spite of this, its meaning cannot be defined univocally because the scholars who have used it belong to different areas of study. The focus of this chapter will be the concept of ‘style of thinking’ as intended by Hacking (SoT). I shall illustrate the SoTs project: a set of ideas and suggestions, put forward in various papers of Hacking, which should be understood as a programme of research (Hacking 2012).

In order to lay bare the content of the concept of SoT, I will have to deal with a difficulty: Hacking has never given a definition of SoT as he maintains that its characterization should be based on examples drawn from the study of past and present scientific practice (Hacking 1983a, p. 456) (Lakoff 2012, p. 227). This being the case, it seems appropriate to set out the concept of SoT going from usage.

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1 In the last four decades many scholars have used the term ‘style’ in diverse contexts with different meanings. For example: Winifred Wisan (Wisan 1978) speaking of Galileo’s new style in the study of motion; Stephen Weinberg (Weinberg 1976) and Noam Chomsky (Chomsky1980) mentioning Galileo’s style for making abstract models (Husserl1970); Jan Lacki distinguishing physical from mathematical thinking (Lacki 2003); with different meanings. For example: Winifred Wisan (Wisan 1978) speaking of Galileo’s new style in the study of motion; Stephen Weinberg (Weinberg 1976) and Noam Chomsky (Chomsky1980) mentioning Galileo’s style for making abstract models (Husserl1970); Jan Lacki distinguishing physical from mathematical thinking (Lacki 2003); Jonathan Harwood arguing that there are national research traditions rather than an unitary scientific method (Harwood 1993); Boon in relation to experimental and mathematical traditions (Boon 2011). The term ‘style’ has also been used in mathematics since the early twentieth century (see Mancosu 2010).
to abstract characterization: to grasp the features of SoTs as well as to assess the
SoTs project in the next chapters will be easier by referring to concrete examples.
In particular, I shall not start by presenting Hacking’s general remarks on the
features of the SoTs: without an example, his characterization would remain
vague. On the contrary, in the next section I shall show the features that make the
SoTs distinctive by discussing the genesis of a specific SoT, the statistical SoT.
Only later, on the basis of this example, will I present the main claims of Hacking’s
SoTs project.

With the purpose of providing insight into the statistical SoT, in the next
section I shall mainly base my analysis on Hacking’s *The Emergence of Probability*
(Hacking 2006 [1975]). There is no mention of the concept of SoT in that book: it
appears in Hacking’s later studies on probability (Hacking 2008 [1990], 1992c).
However, as will be clear in the next section, although Hacking got the idea of SoT
from Crombie in 1978 (Hacking 2009, p. 6), three years later the publication of *The
Emergence of Probability*, his inquiry into the concept of probability contained in
that book can be read, with hindsight, as an account of the birth of a particular
SoT, namely the statistical SoT. Only in the third section shall I turn to Crombie’s
notion of style of reasoning, the starting point of Hacking’s reflections on the
concept of SoT, and discuss the further development of the latter’s ideas. Finally,
in the fourth section I shall examine Hacking’s thoughts concerning the laboratory
SoT.

The aim of this chapter is to connect different suggestions of Hacking about
SoTs so to present them in a systematic way. I just want to explain and highlight
what deserves to be studied or developed in the next chapters. It is for this reason
that I shall restrict my analysis to the statistical and the laboratory SoT, leaving aside other SoTs: Hacking has mainly offered suggestions about these two SoTs and has only mentioned or discussed other SoTs en passant. Conversely, in the next chapter, I shall develop his SoTs project by outlining a ‘theory of SoTs’. My claim will be that there are at least four other SoTs that share a set of features with the statistical and the laboratory SoT. The conclusion will be that it is possible to give substance to Hacking’s notion of SoT by exhibiting six SoTs that share a set of common features, i.e. those features of SoTs suggested by Hacking over the years. It can be said that this chapter presents the SoTs project and the next one develops it into a theory.

2.2 The Roots of the Styles Project: *The Emergence of Probability* as a Study on the Statistical Style of Thinking and Doing

In the late Sixties, when he was an associate professor in Uganda interested in logical questions about statistical inference, Hacking read the first abridged edition of Michel Foucault’s *Madness and Civilization* (Foucault 2006 [1961]) and *The Order of Things* (Foucault 1994 [1966]). It was under Foucault’s influence that in the following years he wrote *The Emergence of Probability* (Hacking 2006 [1975]) and *The Taming of Chance* (Hacking 2008 [1990]), which examined the transformations
in scientific understanding of probability. The Emergence of Probability, in particular, applied that kind of analysis that Foucault called archaeology (Hacking 2006b, p. 2): Hacking dug into the past in order to understand what made it possible for our world to be dominated by probabilities. By offering an historical account, he argued that the concept of probability was absent before the time of Blaise Pascal (1623-1662) -- what made the concept of probability possible was the coming to the fore of a new form of evidence. This is a point relevant to understanding the concept of SoT. In order to explain it carefully I shall now summarize Hacking’s account.

To start with, Hacking reminds us that, in medieval thought, scientia represented the knowledge of universal truths that is obtained by demonstration; conversely, opinio was associated with beliefs, resulting from argument or disputation, which could not be demonstrated. The word probabilis did not suggest the idea that a hypothesis was better supported by evidence than others but meant ‘worthy of approbation’, i.e. approval by ‘intelligent people’ (Hacking 2006 [1975], p. 22-23).

In Renaissance medical textbooks, an opinio was supported by signs such as planets in conjunction, comets and other natural phenomena. While physicists were dedicated to demonstrative science, physicians had to predict the likely course of a medical condition on the basis of signs. In the case of an ill person, the causes had to do with efficient causes of the illness and signs were anything by which a doctor could make a prognosis (Hacking 2006 [1975], p. 28). Ultimately, in the Middle Ages an opinio was probable when supported by ‘intelligent people’.

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2 Hacking revealed this autobiographical detail in an interview (Lakoff 2012).
authoritative people, or ancient books. Later on, in the writings of Renaissance physicians, opinions were probable when supported by signs of Nature, rather than the written word. There was no antithesis between these two epistemological attitudes: since for the Renaissance authors Nature was the writ of God, its signs represented approval by the ultimate authority.

Beside approbation, there was no other concept of evidence related to opinion. What was lacking was the evidence provided by things, not to be confused with that provided by the data of senses. To use the example given by J. L. Austin (1911-1960) and quoted by Hacking (2006 [1975], p. 32), pig-like marks and buckets of food outside a sty represent the evidence of things for the statement that in the sty there is a pig; the coming into view of the pig represents the evidence of the senses - I can see what animal it is. Obviously, in the Renaissance people did use the evidence of things, but for Hacking the concept was absent: 'dogs and boars can tell there is a pig, and do not thereby have a concept of evidence' (Hacking 2006 [1975], p. 34). For us books and testimony represent indirect evidence reported by other people. The Renaissance had the order reversed: testimony and authority were primary, things counted as evidence only insofar as they resembled the authority of testimony and books.

The concept of sign underwent several changes - for instance, Paracelsus (1493-1541) ignored the fact that the names of stars were conventions and thought that the 'true names' of celestial bodies were signs; but later on, the distinction between conventional signs and natural ones was clearly made. The physician Girolamo Fracastoro (1483-1553) wrote that, among the signs in the sky, air, soil or water that are premonitory
some are almost always, others are often, to be trusted. Therefore one ought not to consider them all as prognostications, but only as signs of probability (Hacking 2006 [1975], p. 28).

These changes opened the way for the evidence of signs to turn into the evidence of things. In the passage above, we may note that, since not all signs are to be trusted with certainty, the idea of probability is connected with frequency, that is, with what happens 'almost always'. Here, we already recognize some of the features of our statistical concept of probability. According to Hacking, in a text published in 1650 by Thomas Hobbes, the concept of evidence of things conjoined with that of frequency had already taken full shape and probability had emerged in all but name (Hacking 2006 [1975], p. 48). In 1662 also the Port Royal logic, published by members of the Jansenist movement, distinguished arbitrary and conventional signs -- the concept of sign as evidence had become endemic; stable and law-like regularities became worthy of observation. Later on, the studies of the Jacques Bernoulli (1654-1705) culminated in the central limit theorem; in 1756 Thomas Simpson (1710-1761) applied the theory of errors to the discussion of uncertainty; finally, in 1820s the first public statistics were published (Hacking 2008 [1990], p. 133) and some years later the statistical laws were used to explain phenomena.

The coming to the fore of the evidence of things produced a new way of reasoning whose features I am going to illustrate. Among the sentences formulated by the alchemists and astrologists of the Renaissance, Hacking takes the following sentence (henceforth called SI) of Paracelsus as an example:
(S1) Mercury salve is good for syphilis because mercury is signed by the planet Mercury, which signs the marketplace where syphilis is contracted (Hacking 1983b, p. 71).

He claims that for us S1 is not a candidate for truth or falsehood (Hacking 2002 [1982]-a, p. 171). How can this claim be justified? The point is that, for Hacking, Paracelsus’s reasoning was based on a kind of evidence that died out in the seventeenth century and became unknown afterwards: ‘the way propositions are proposed and defended’ (Hacking 2002 [1982]-a, p. 170) by Paracelsus is alien to us. In order to make sense of S1 it is not sufficient to know the meaning and the connotations of all the words. In short, it is Paracelsus’s way of reasoning that we need to reconstruct. Only when we have done so can we talk as Paracelsus talked. To make Hacking’s point clear, let us examine Paracelsus’s way of reasoning behind the sentence S1. The starting point is that diseases must be cured by similarity. Bearing that in mind, this is the logic chain of Paracelsus’s reasoning: syphilis is contracted in the marketplace; the planet Mercury has signed the marketplace; this is evidence that the metal mercury, which has the same name, can treat syphilis, given that diseases are cured by similarities. Ultimately, for syphilis, the name ‘mercury’ of the metal mercury is the sign that a good physician must know. It is a sign that provides evidence for the proposition ‘Mercury treats syphilis’. This way of reasoning is not ours: propositions are defended on the basis of a system of evidence (signs), rules (e.g. similarities) and practices now fallen in
disuse. Because of that, Hacking maintains that they do not have truth-value for us.

Following Hacking, one could say that, just as some propositions are candidates for truth or falsehood only in the Renaissance way of reasoning, so certain propositions acquire a truth-value only as consequence of the emergence of probability (or, better, of the evidence of things). For example, the sentence:

\[(s2) \text{The adult height for one sex in an ethnic group follows a normal distribution}\]

did not have any truth-value in the Renaissance not only because the term 'normal distribution' was not defined but because the truth-value of the sentence \(s2\) hinges on the evidence of things. This is a kind of evidence that, as I have said, according to Hacking was not yet conceptualized before the mid-seventeenth century\(^3\).

Moreover, to reason about this sentence means to take all the steps of a complex process – selecting a sample, measuring, classifying, dealing with errors or assessing the difference between the experimental distribution and the theoretical one. Ultimately, to prove the sentence above involves, for all the means and purposes, not only a way of thinking but also a \textit{way of doing}.

Hacking’s point is that certain sentences may be meaningful only in a given epoch not because some terms are not defined but because their having a truth-value hinges on different ways of thinking and doing. To appreciate his point, let

\(^3\) Hacking’s own example is ‘the gross national product of Württemberg in 1817 was 76.3 million adjusted to 1820 crowns’. For him, this sentence did not have a truth-value before 1821 ‘because there was no procedure of reasoning about the relevant idea’ (Hacking 1992c, p. 143).
us compare $S_1$ and $S_2$ with this passage from *The Merchant’s Tale*, a bawdy story from *The Canterbury Tales* by Geoffrey Chaucer (1343-1400):

> 'Allas', quod he, 'that I ne had heer a knave
> That koude clymbe! Allas, allas', quod he;
> 'For I am blynd!' 'Ye sire, no fors', quod she;
> 'But wolde ye vouche sauf, for Goddes sake,
> The pyrie inwith youre armes for to take,
> For wel I woot that ye mystruste me,
> Thanne sholde I clymbe well ynogh', quod she,
> 'So I my foot myghte sette upon youre bak' (in Gooden 2009, p. 66).

In his *The Story of English* (Gooden 2009), Gooden says that in the passage there are several challenges for the modern reader. One has to know what certain words stand for, e.g. *quod*, past tense of *queten* – ‘to say’; *pyrie* – ‘pear tree’; *knave* – ‘servant’; *no fors* – ‘no matter’ (Gooden 2009, p. 66). However, although the passage is full of *double-entendre*, once the archaic forms are defined, its literal sense becomes clear - it becomes meaningful. It refers to what happens when the servant Damyan climbs up a pear tree in the garden of January, the main character, and waits for the arrival of the young May and her husband. Not so for Paracelsus’s sentence – understanding it is for us an entirely different exercise that requires an effort beyond that of expressing the sentence in modern English. Not so for the sentence $s_2$ either: for Hacking a Renaissance thinker would not have been able to assert or deny it, even if expressed in her language.
Taking a cue from these comparisons, another important point can be noted. The sense of both S1 and S2 rests on two different kinds of evidence, each of them peculiar to the respective ways of thinking. On the other hand, another passage of The Merchant’s Tale, which precedes the passage just quoted, reads:

(s3) She taketh hym by the hand

(i.e.: ‘She takes him by the hand’). Hacking would say that the sense is straightforwardly clear because it hinges on the evidence of senses, which of course does not belong exclusively to a given way of thinking. In any epoch, once the terms are defined, it can be asserted or denied – come and look. Indeed Hacking stresses that, beside sentences whose truth-value rests upon a style of reasoning, there are ‘observation statements’ (Hacking 1992c, p. 133), or in other words ‘factual statements’ (Hacking 1983b, p. 173). Hacking is adamant that these sentences do not require any reasoning to be assessed as true or false: they rely on the evidence of senses, which is foundational, ahistorical.

The idea of ‘observational statements’, or ‘protocol statements’ in the original terminology of the Vienna Circle, has been much criticised in the last decades. For example, Norwood Hanson (1924-1967) and Paul Feyerabend (1924-1994) maintained that observation statements are theory-laden (Hanson 1958, chapter 1) (Feyerabend 1988 [1975], chapter 3). Hacking agrees with them only to a certain extent:

The core objection to a correspondence theory is that there is no way in which to identify the facts to which a statement corresponds,
independently of the statement itself. That is true in general, but not of a lot of the run-of-the-mill sentences of the sort beloved by logicians (Hacking 1992c, p. 134).

For sentences that make sense only against the background of the ‘accepted’ way of reasoning, Hacking thinks that ‘Moritz Schlick’s motto, “the meaning of a sentence is its method of verification”, points in the right direction’ (Hacking 1992c, p. 135). In other words, he does not think that ‘there is one theory of truth, or one semantics, that applies to all contingent empirical sentences investigated in the sciences’ (Hacking 1992c, p. 135). On the one hand correspondence theories of truth apply to style-independent sentences (e.g. s3), i.e. sentences whose truth-conditions are not determined by the way we reason. On the other hand, the methods of verification, which may change over time, apply to style-dependent sentences (e.g.s2), making them candidates for truth or falsity.

The verificationist account of meaning is particularly important for the styles project, and especially for establishing whether or not certain propositions are style-dependent, that is candidates for truth or falsehood. Hacking’s concern is chiefly with the fact that Schlick put forward ‘a test for marking off the meaningful utterances’ (Hacking, 1975 p. 97): as we shall see, within the styles project whether or not a community that adopts a certain SoT possesses methods for assessing a proposition as true or false can be crucial for establishing whether or not that proposition is meaningful for that community. Indeed, by saying that certain sentences are not meaningful Hacking often assumes the verificationist principle. As an example one can consider the sentence s2 above: for him whether or not it is meaningful for a certain community, that is, a candidate for truth or falsehood,
depends on whether or not that community possesses the methods of the statistical SoT or its kind of evidence.

This leads to another important point. The verification methods that make statistical sentences meaningful are refined versions of the procedures for testing errors and making estimations that have emerged since the eighteenth century. Hacking notes that these methods of verification are themselves couched in terms of probability:

the conditions of assertibility of statistical hypotheses are themselves to be determined [...] in terms of yet a new layer of sentences that themselves are statistical (Hacking 1992c, p. 151).

This remark can be explained by considering once again S2 (i.e. ‘The adult height for one sex in an ethnic group follows a normal distribution’). What makes S2 true or false is the existence of criteria for assessing whether the normal distribution fits the observed statistical distribution of the adult height. Typically, these criteria are measures of goodness of fit such as the chi-squared tests, which evaluate how well the observed frequencies follow a Gaussian distribution. However, the chi-squared tests are themselves statistical – for example, to ‘verify' S2 one has firstly to use the differences between the theoretical and empirical frequencies for calculating a value called chi-square; then, on the basis of this value and a statistical distribution (chi-square distribution), one works out a probability. If this probability is lower than a certain number, it is considered very unlikely that S2 is
true\(^4\). In conclusion we use a statistical distribution and other probabilistic methods to assess a statistical sentence.

Hacking maintains that there is some circularity in the process:

> There is an odd way in which a style of reasoning and truth-conditions of some sentences are mutually self-authenticating. The truth is what we find out in such and such way. We recognize it as truth because of how we find it out. And how do we know that the method is good? Because it gets at the truth (Hacking 1992c, p. 135).

In terms of my example, Hacking’s claim is that the truth conditions of \(S2\) are determined by a process of reasoning that includes the use of statistical procedures such as the chi-square test; on the other hand, this way of reasoning has become ‘the correct standard’ of assessing \(s2\) because it has, to use a phrase cited of C.S. Peirce (1839-1914) by Hacking, the ‘truth producing virtue’ (Hacking 1992c, p. 135) i.e. it is able to identify whether or not a proposition is true. It is to this circularity that Hacking alludes to when he says that a way of thinking is self-authenticating.

So far I have illustrated by examples the following points of Hacking. In the Renaissance the evidence of things was lacking and the world testified by its signs. Certain sentences of the Renaissance thinkers (e.g. \(S1\)) are not among our candidates for truth-or-falsehood because their sense hinges on a kind of evidence and a way of reasoning different from ours. Later on different scholars maintained

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\(^4\) An introduction to the use of the chi-squared test can be found in (Taylor 1982, chapter 12).
that signs can be imperfect: for example, in 1650, in Hobbes's *Human Nature*, signs are considered imperfect and only 'very often' right. Credibility and frequency were thus linked. Then conventional signs were distinguished from natural signs and the concept of the evidence of things emerged. In the seventeenth century the new evidence of things opened a space for a new way of reasoning and, in particular, for new candidates for truth-or-falsehood. Hacking thinks of this transformation as a *discontinuity* in the history of human thought:

there was hardly any history of probability to record before Pascal,
while after Laplace [...] a page-by-page account of published work on the subject became almost impossible (Hacking 2006 [1975], p. 1).

He even gives a date for the birth of probability: the decade around 1660 (Hacking 1992c, p. 141).

Incidentally, the coming to the fore of probability was not only followed by the emergence of new style-dependent sentences (e.g. S2) but also by new criteria (e.g. best fit), types of explanation (e.g. statistical distributions), laws (e.g. the law of large numbers), classifications (e.g. society is divided into genres of people (Hacking 2008 [1990], p. 134)) and new objects (e.g. the population characterized by a mean and standard deviation).

All these points of Hacking can be summarized by saying that there exists a mode of thinking and doing, which we may call the *statistical style of thinking and doing*, with the following characterizing properties:
The statistical style of thinking and doing relies on a new kind of evidence for finding out; it is self-authenticating and introduces new candidates for truth-or-falsehood, criteria, types of explanation, laws, classifications and objects.

The emergence of the statistical style of thinking and doing represents a sharp break in the history of Western thought.

Hacking introduced the term ‘style’ in his paper of 1982 ‘Language, Truth and Reason’ (Hacking 1982). By the term ‘style’ or, more precisely, by the expression ‘style of thinking and doing’, he means a way of thinking and doing with the features discussed above, as I shall explain in section 2.4. For my part, as I have said, I shall use the acronym SoT. That is: a SoT is a way of thinking and doing in the sense of Hacking -- it possesses all the features listed just above (which I shall call ‘the characterizing properties of SoTs’).

Although Hacking introduced the concept of ‘style of thinking and doing’ seven years later the publication of The Emergence of Probability, the latter already illustrated the concept of style of thinking and doing that he later on elaborated, as my discussion above shows. For his concept of style Hacking acknowledged his debt to the Australian historian of science Alistair Cameron Crombie (1915-1996) of whom I will say more in the next section.
2.3. From Crombie’s Styles of Thinking to Hacking’s Styles Project

In 1978, three years after the publication of The Emergence of Probability, Hacking was invited to a conference in Pisa in which Crombie (1915-1996) lectured on what he called ‘styles of scientific thinking in the European tradition’ (Crombie 1978). Crombie spoke of exactly six styles of scientific thinking still practised in the modern sciences, each of which emerged at a specific time in history and evolved in its own time frame. That lecture suggested to Hacking the ‘idea of a small manifold of SoTs’ (Hacking 2009, p. 6), as he revealed later (Hacking 2012, p. 1). In order to explain how he turned Crombie’s ideas into his own philosophical project I shall first summarize the latter’s account of styles.

The point that six styles can be distinguished in the history of Western science is a recurrent view in many of Crombie’s writings (Crombie 1978, 1988, 1992, 1994). Since the mid-seventies, his aim had been to write an encyclopaedic history of their growth, but it was only in 1994 that his ideas finally appeared in an ambitious three-volume book, entitled Styles of Scientific Thinking in the European Tradition (Crombie 1994).

In that book, Crombie maintained that a ‘postulational style of thinking’ was adopted by the ancient Greeks in mathematics, Aristotle’s works on natural sciences, Hippocratic medicine, ethics, law and metaphysics (Crombie 1994, p. 74). He conceived this style, the first he described, as a method that consisted of a two-step process: search for the simplest and fewest premises and deduction of their implications by ‘proof’, a Greek invention (Crombie 1994, p. 20). For example, the
demonstrative power of geometry was used by Claudius Ptolemy (90, 168) to calculate celestial motions and to build a theory of the process of vision in optics. In their scientific works, the Greeks set up a rational scientific system in which ‘the sequence of theoretical reasoning from premise to conclusion matched the sequence in nature from cause to effect’ (Crombie 1994, p. 20). In other words, the phenomena were reproduced by an abstract deductive formal system, which embodied postulated principles and offered causes of regularities. The world came to be thought as rational, entirely deducible by first principles.

Crombie also spoke of an experimental style that came about in ancient Greece because observation and measurement were necessary either to control the explanations obtained by the method of postulation or to explore the possible causes of phenomena. For Crombie, the use of experiments became the more and more sophisticated from 13th to the 17th century through the strong relation between the postulational style in the natural sciences and engineering (Crombie 1994, pp. 311-423). Then, the habit of measurement and observation fully developed in the seventeenth century beside the growth of mathematics.

By ‘the hypothetical style of modelling’ Crombie meant ‘the method of elucidating the unknown properties of a natural phenomenon by simulating the phenomenon with the known properties of a theoretical physical artefact’ (1994, p. 74). For him, the style of hypothetical modelling had its origin in ancient Greece, in particular in ancient theology and astronomy. He illustrated in detail this thesis by examining thinkers who lived from 400 AD onwards (i.e. from Augustine to Kant) (Crombie 1994, pp. 1081-1231). For example, the armillary sphere (astrolabe) was a model of celestial sphere invented by Eratosthenes of Cyrene (276 - 195 B.C)
in order to describe the movements of stars and to map the constellations.

Hypothetical modelling fully developed in application to early modern perspective, painting and engineering; then, from these fields of study it was transferred to modern science.

Furthermore, Crombie maintained that the fourth style, the taxonomic one, is ‘the method of ordering variety in any subject matter by comparison and difference’ (Crombie 1988, p. 11). In the first volume of his 1994 work he pointed out that this style emerged as an explicit logic of classification in Plato and Aristotle and ‘in many ways it is the foundation of all natural science, establishing fundamental similarities and difference’ (Crombie 1994, p. 75). Then, in the fifth part of the second volume, Crombie illustrated the search of a natural system throughout history from Andrea Cesalpino, who proposed a classification based on the reproductive parts, to Linnaeus, who funded his natural system on the fixity of genera.

The fifth style described by Crombie is the statistical one. He proposed for this style an origin alternative to that suggested by Hacking. Indeed, Crombie’s story of probable reasoning begins in ancient Greece and leads to Laplace without any sharp transition in systems of thought such as that emphasized by Hacking (Crombie 1994, p. 75). On his part, Hacking stressed the fact that his account of the emergence of probability is alternative to Crombie’s continuist one (Hacking 2006 [1975], p. X).

Finally, for Crombie the style of historical derivation, originated in ancient Greece, aimed at explaining the present as a development of the past regulated by certain laws and at deducing the past from present regularities (Crombie 1994, p.
Again, Crombie conceived the development of the style of historical derivation as continuous from Greeks to Darwin; however, these methods were developed in application first to the history of languages, then to the geological history of the Earth and the evolution of living organisms (Crombie 1994, vol. III chapter seven).

In his seminars at the Collège de France, Hacking referred to Crombie’s styles above by using the following aides-mémoire:

a) The *postulational style* (in Hacking’s writings the terms ‘geometrical style’ and ‘style of mathematics’ are also found (Hacking 2005c, p. 543) (Hacking 2012))

b) The *experimental style*

c) The *style of hypothetical modelling*

d) The *taxonomic style*

e) The *statistical style*

f) The *historico-genetic style*

Henceforth, I shall refer to this list of styles of thinking as ‘Crombie’s list’.

Crombie introduced his concept of style to organize his history of western sciences. Sometimes, he used interchangeably the terms ‘method’ and ‘style’ (e.g. Crombie 1988, p. 10); in other occasions he added the specification that ‘styles of scientific thinking are distinguished by their objects and their methods of reasoning’ (Crombie 1994, quoted in Hacking 2009, p.90). It is in a book published only in Italian that he provided a more precise definition of what a style is:
We can define what I call a 'scientific style' in terms of three features: 1) its form of argumentation (its methods of discovery and demonstration); 2) its conception of nature (beliefs about what exists and is to be discovered); 3) the mental habits (especially the expectations and the answers concerning innovation and change; the dispositions of a society and individuals within it) (Crombie 1992, p. 103 my translation).

Feature one concerns the model of discovery that scientists in distinct periods use (Crombie 1988, p. 2): for example, in certain circumstances, the geometrical model in which the laws of phenomena are derived from first principles might be preferred to an analogical model, in which phenomena are understood and explained by building their mechanical representations.

Feature two concerns the conception of nature associated to every style. For example, the statistical style opened the way to quantum theory (see Hacking 2006 [1975], p. 174), whose formalism cannot be interpreted as if probabilities reflected our ignorance. As a consequence, statistical laws have become an irreducible fact of the world and classical physics' deterministic conception of nature has given way to an indeterministic one. In this sense, the advocates of statistical style are committed to an indeterministic conception of nature.

Feature three concerns the fact that distinct societies have been more or less ready to accept change because of their beliefs: for example, as we shall see, according to

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5 The works of the physicists John Von Neumann (von Neumann 1955), John Bell (Bell 1966), Simon Kochen and Ernst Specker (Kochen and Specker 1967) show that it is impossible to provide a 'classical' reformulation of the formalism of quantum theory.
G.E.R. Lloyd it was precisely because Greek society was so polemical and open to debate that an axiomatic-deductive mathematics came about in the ancient Greece and was put into general practice (Lloyd 2004).

A point to note is that all of Crombie’s styles emerge in ancient Greece and develop continuously. Both Kerry Magruder and Robert Iliffe pointed out that Crombie’s continuity thesis about the development of styles is to be linked to his continuism concerning the Scientific Revolution (Magruder 1995, p. 408) (Iliffe 1998, p. 20). In Crombie’s view, the experimental methodology of Galileo and Newton was first developed and practised by medieval natural philosophers. Crombie accepted that the crucial steps in the emergence of modern science took place during the Scientific Revolution, but he thought that the essence of history of science was a continuous development of scientific thought from Greek thinkers onwards. As Kusch noted, ‘the tool-box of Crombie-inspired styles contains no ingredients to understand historical change’ (Kusch 2010, p. 166). Indeed, Crombie’s notion has been conceived from a historical perspective different from that of SoT:

Crombie’s vision of the history of the European sciences favours continuity. My instinct is exactly the opposite. I like to tell the history of each style as having at least one sharp moment of crystallization, a fixing of how to go in the future […]. I acquired this habit early, in The Emergence of Probability, some years before I had ever heard of Crombie (Hacking 2009, p. 14)
In this passage Hacking reveals that it was his study on probability that made him sceptical about continuism. So much so that he supposes that other ways of reasoning possess the same feature. Consequently, what he wants is a notion that describes ways of reasoning whose beginnings mark a discontinuity point in the trajectory of Western thought.

In 1982, four years after his encounter with Crombie’s ideas, Hacking published his first paper ever on SoTs, ‘Language, Truth and Reason’ (Hacking 1982), which was included by the English philosopher Martin Hollis (1938-1998) in a collection of essays entitled *Rationality and Relativism* (Hollis and Lukes [Eds.] 1982). A clear point emerged from that paper: despite Hacking’s admission that Crombie had been the starting point of his SoTs project, their enterprises must be regarded as entirely different. Indeed, Hacking launched his own philosophical project by making a distinctive use of the latter’s notion of style. I am going to illustrate this project in the next section.

### 2.4. The Styles Project

‘The Style Project’ is the label used by Hacking in his ‘‘Language, Truth and Reason’’ thirty years later’ (Hacking 2012) to describe the programme sketched in his earlier papers on SoTs, namely ‘Language, Truth and Reason’ (Hacking 1982) and ‘‘Style’’ for historians and philosophers’ (Hacking 1992d). However, later thoughts expressed in works such as *Scientific Reason* (Hacking 2009) might be included in this project. In this section I shall present those points
of the SoTs project made by Hacking that mainly concern his characterization of
SoTs and I shall leave for later discussion other philosophical questions touched
on by him. My point will be that the SoTs project contains ideas that need to be
developed or better clarified.

2.4.1 Objectivity and Meaning

Ever since his 1982 paper Hacking’s reflection on SoTs has centred around
the point that ‘whether or not a proposition is as it were up for grabs, as a
candidate for being true-or-false, depends on whether we have a way to reason
about it’ (Hacking 2002 [1982]-a, p. 160). For him SoTs fix the sense of certain
sentences by making them candidates for truth or falsehood. The sentence S2 (‘The
adult height for one sex in an ethnic group follows a normal distribution’) in the
previous section illustrates well this point: its sense hinges on the SoT appropriate
to it, i.e. the statistical SoT. Indeed – Hacking would argue - what determines its
truth-value is the statistical SoT: its methods, its way of thinking and doing and its
kind of evidence.

Hacking distinguishes positive propositions, i.e. propositions that are true-or-false,
from bivalent propositions, i.e. propositions that have a definite truth-value, true or
false (Hacking 2002 [1982]-a, p. 166). ‘Positive’ is a less strong feature than
‘bivalent’ because a positive proposition is a proposition potentially susceptible of
being true or false, given the existence of a SoT; on the other hand a bivalent
proposition is a proposition that is actually true or false. Hacking gives this
example: once J.C. Maxwell (1831-1879) said that some propositions about the
relative velocity of light were intrinsically incapable of determination; but few
years later A.A. Michelson (1852-1931) invented a technology to give a precise
answer to Maxwell’s questions (Hacking 2002 [1982]-a, p. 167). Maxwell’s
sentences were only positive; but after Michelson’s experiments they became
bivalent. The point is that for Hacking a proposition is positive if it makes sense,
even if the correct way of assessing it by the standards of truth and methods of its
SoT has not yet been found. SoTs have to do with positive sentences rather than
bivalent sentences.
The fact that Hacking essentially thinks of SoTs as structured classes of
propositions that are capable of being true or false marks a profound difference
between Crombie’s notion of style and Hacking’s notion of SoT. What for the
historian Crombie is a device for describing the Western scientific tradition, in the
hands of Hacking becomes the tool ‘we need to understand what we mean by
objectivity’ (Hacking 2002 [1992], p. 181). What Hacking means can be explained
by noting that he often uses statements such as ‘the proposition has a truth-value’
or ‘has a sense’ or ‘is positive’ or ‘is meaningful’ or ‘is objective’ interchangeably.
In particular, the expression ‘becoming objective’ is used in reference to
propositions that become candidates for truth or falsehood; or in reference to
concepts, questions, conjectures, solutions that come to be uttered, shared,
discussed, i.e. become ‘possible’, as when the emergence of the evidence of things
made possible the sentences of the statistical SoT. So what Hacking means by
saying that the notion of SoT is ‘what we need to understand objectivity’ can be
grasped by looking at his study on probability: he thinks that the idea of a
statistical SoT is a useful tool for understanding how certain sentences became
candidates for truth or falsehood and how the concept of probability itself became objective. More generally, Hacking wants to use the notion of a SoT to capture the way in which the past has determined our present scientific conceptions. To adopt Foucault’s phrase, Hacking wants to do a ‘history of the present’ (Hacking 2009, p. 4), i.e. to dig into the past to reveal the conditions of possibility of our present knowledge. This point will be relevant to tracing the philosophical lineage that has shaped the SoTs project.

2.4.2 Hacking’s Styles of Thinking

Over the years Hacking has mentioned as SoTs different ways of reasoning. Although in 1992 he declared that Crombie’s three-volume work provides a historical account useful for characterizing his SoTs (Hacking 2002 [1992], p. 186), he did not accept as SoTs all the styles in Crombie’s list. For example, he observed that Crombie’s styles b) and c) (the experimental style and the style of modelling) on their own cannot account for the considerable sophistication undergone by the experimental method over the centuries. Hacking did not deny that both experimenting and modelling can be in play in particular areas of research (Hacking 2002 [1992], p. 186), but he claimed that the concept that captures the genesis and development of experimental thinking is, as he called it, the laboratory style of thinking and doing. I shall characterize it better in the next section. For the time being, I want to outline the evolution of Hacking’s thought as regards this SoT.

For a start he has always been convinced that the salient feature of this SoT is that, ever since the time of the Scientific Revolution, humans have build
experimental apparatuses to elicit new phenomena, phenomena that had never been observed before (Hacking 2009, p. 43). The emblem of this new way of doing is Robert Boyle (1627-1691), who made the air pump in order to investigate the capacity of the air to exert pressure; this is a thesis that Hacking drew from Steven Shapin and Simon Schaffer’s book *Leviathan and the Air Pump* (Shapin and Schaffer 1989 [1985]-a), as I shall show. Ultimately, one way to think of this SoT is to regard it as a particular deployment of Crombie’s styles b) and c) plus the creation of phenomena by man-made machines (Hacking 2007a, p. 3).

Furthermore, in the paper *The Self-Vindication of the Laboratory Sciences* (Hacking 1992b), Hacking argued that laboratory sciences tend to produce a sort of self-vindicating structure in that background theories, hypotheses, modelling of apparatus, data analyses and other items of knowledge are mutually adjusted to each other. He referred to this particular internal dialectic between different items of knowledge as a *self-stabilizing technique (or self-vindicating technique)*, the process peculiar to the experimental thinking that, as I shall explain, makes the laboratory SoT self-authenticating.

Hacking has entertained the idea of a ‘laboratory SoT’ at least until 2007, when he gave a talk in Taiwan with the telling title ‘The Laboratory of Thinking and Doing’ (Hacking 2007a). Yet in 2012 he corrected his claim. He said that Occam’s Razor should move us to regard the emergence of the laboratory SoT as a phenomenon internal to the life of Crombie’s experimental style (this time considered a SoT) rather than a SoT to be added to Crombie’s list (Hacking 2012, p. 5). Incidentally this last suggestion is difficult to put into practice. Boyle’s new way of thinking cannot be fitted into Crombie’s experimental style: Boyle
represents a radical change whereas the development of Crombie's style is continuous.

Concerning Crombie's postulational style, Hacking has always insisted that it can be considered a SoT with this proviso: the real novelty brought into being by Greek thought is 'proof' rather than, as Crombie believed, postulation. In 1992 he had made it clear that a comprehensive account of the postulational SoT, which he never offered, should explain why 'mathematics has the astonishing power to establish truths about the world independently of experience' (Hacking 2002 [1992], p. 183). Some years later, in *Scientific Reason* (Hacking 2009), he declared that the *Shaping of Deduction in Greek Mathematics* (Netz 1999) by the historian of ancient mathematics Reviel Netz is the study he wanted (Hacking 2009, p. 67): an investigation on the cognitive and social factors which shaped the emergence of proof.

With regard to the remaining styles in Crombie's list, the taxonomic style and the historico-genetic, the styles project says almost nothing, as Hacking explicitly admits (Hacking 2012, p. 10). On the other hand, in his 1992 paper he mentioned new SoTs that could be added to Crombie's list. For instance, he suggested that 'the Indo-Arabic style of applied mathematics, little interested in postulation but dedicated to finding algorithms' (Hacking 2002 [1992], p. 185) might represent a distinct style, which he called 'algorismic' SoT (Hacking 2002 [1992], p. 185). Later, in his lectures at the Collège de France (Hacking 2006c, p. 7) and in *Scientific Reason* (Hacking 2009) he brought up this idea again. Finally, he made no mention of it in his 2012 paper, in which he rejected some proposals of other SoTs in literature.
It should not be forgotten that in ‘Language Truth and Reason’ (Hacking 1982) as well as in \textit{Representing and Intervening} (Hacking 1983b), Hacking mentioned another style of thinking, that of Paracelsus (section 2.2), and in his 2012 paper he also explicitly referred to it as a SoT (Hacking 2012, p. 9). However, in a note of his paper, Robert Kowalenko (Kowalenko 2011)\textsuperscript{6} wrote that Hacking, in personal communication, told him that Paracelsus’ style is not ‘scientific’, a point that Hacking has never clarified.

Hacking’s last word about the number of SoTs was that ‘it is better not to multiply six [the number of styles in Crombie’s list] beyond necessity’ (Hacking 2012, p.6). Even so, it remains unclear why certain styles he mentioned or others that have been proposed by other scholars should be excluded. Is there a deeper reason than the simple observance of \textit{lex parsimoniae}?

\textsuperscript{6} Kowalenko’s paper was presented at the third Bi-annual Conference of the European Philosophy of Science Association in Cape Town and has not been published so far.
2.4.3 The Characterizing Properties of Styles of Thinking

A way of reframing this issue is that of asking whether there are necessary and sufficient conditions for being a SoT. As I am going to explain, Hacking has been undecided about the answer to this question. In section 2.2 I have introduced the 'characterizing properties of SoTs', exemplified by the features of the statistical SoT. Indeed, over the years Hacking has mentioned each of those properties in his general discussions about SoTs. For example, I have already pointed out that for Hacking the statistical and the laboratory SoT had clear beginnings. Might there exist other ways of reasoning with this property? The SoT project contains no answer to this question. On the other hand, in the second section of this chapter I have also explained that the statistical SoT introduces new objects, new candidates for truth or falsehood, criteria and laws, relies on the evidence of things and is self-authenticating. This is exactly what Hacking says in general about SoTs:

Every style of reasoning [SoT] introduces a great many novelties including new types of:

Objects

Evidence

Sentences, new ways of being a candidate for truth or falsehood

Laws, or at any rate modalities

Possibilities

One will also notice, on occasion, new types of classifications and explanations (Hacking 2002 [1992], p. 189) (see also Hacking 1995, p. 75).
Further on in the text he added that:

"Styles become standards of objectivity because they get at the truth. But a sentence of that kind is a candidate for truth or falsehood only in the context of the style. Thus styles are in a certain sense 'self-authenticating' (Hacking 2002 [1992], p. 191).

Although the sense of these quotations appears clear if one refers to the statistical SoT, some points are still ambiguous. It is unclear whether all the potential candidates for being identified as SoTs mentioned by Hacking are self-authenticating and why. This is not a trivial matter. In 1996, in respect of the question 'how do the sciences differ from the humanities?' Hacking suggested that only the sciences exhibit ways of reasoning that have developed self-stabilizing techniques (Hacking 1996, p. 74). If so, establishing which ways of reasoning are self-authenticating is relevant to characterizing science itself.

Incidentally, I object to Hacking's use of the verb 'to introduce' in relation to evidence. Considering as an example the statistical SoT, it is not that it 'introduced' new evidence; if anything, the order is reversed: only when the new evidence of things was conceptualized did a 'space' for the birth of the statistical SoT open. So I think this verb does not really do the trick, at least when we want to describe the statistical SoT's internal mechanisms. One should simply say that to adopt a new SoT involves the use of a new type of evidence.

Furthermore, it is unclear whether the features I have mentioned above by Hacking are necessary and sufficient conditions for being a SoT. In 1992 he wrote that there is 'a necessary condition for being a style of reasoning: each style should
introduce novelties of most or all of the listed types [above]' (Hacking 2002 [1992], p. 189). Later, in his lectures at the Collège de France he went further by claiming that the existence of the features above is a necessary and sufficient criterion for a SoT (Hacking 2006a, p. 3 my emphasis). But then, in 2012, interviewed by Andrew Lakoff, he said:

It is not surprising that one can’t give a definition, a set of necessary and sufficient conditions, for being in the Crombie’s list. You can’t define science: a fortiori you cannot define styles of scientific reasoning. [...] There are general things that one can say, but they tend to be too general (Lakoff 2012, p. 227).

2.4.4 Doing
In his later writings (e.g. Hacking 2009) about SoTs he insisted on another point that I have already touched on: SoTs also are ways of doing. Crombie focused on ‘knowing by doing’ in the style of hypothetical modelling and the experimental style: for example, perspective was a new way of seeing, depicting and manipulating (Crombie 1994, pp. 444 - 60) and imitation of nature was a new way of knowing. In Hacking this inextricable combination of knowing and doing is much more developed: he is often keen to remark that humans are embodied creatures that use both their minds and bodies to think and act in the world (Hacking 2012, p. 2). For example, he stressed that there is a strong link between thinking and doing in mathematical proofs (Hacking 2011). In order to underline this point in his earlier writings he had favoured the locution ‘style of reasoning’ because he thought that ‘thinking is too much in the head’ (Hacking 2002 [1992], p.
189), whereas he wanted to stress that assessing the truth of a style-dependent sentence has to do with ‘doing’ as well as ‘thinking’. In his latest writings, to be more explicit, he preferred the label ‘style of thinking and doing’ to ‘style of reasoning’ (Hacking 2012, p. 2).

I have remarked in the second section of chapter one that some of Hacking’s critics have passed over this remark. I am with James Elwick, who said:

[There is] a tendency of many scholars to transform ‘styles of reasoning’ into ‘styles of thinking’ despite Hacking’s insistence that hand-work [sic] is just as important as head-work [sic] (Elwick 2012, p. 1).

This tendency might be ascribed to the fact that Hacking has not clarified why all the SoTs he mentioned can also be viewed as ways of doing. This is another suggestion that I want to develop.

Finally in 2012 Hacking made it clear that SoTs should be understood as scientific ways of finding out

Another available moniker [for SoTs] is: [...] ways of finding out in the sciences. Not only finding out ‘that’ so and so, but also, finding ‘how to’. Finding out what’s true, and finding out how to change things (Hacking 2012, p. 3).
In this sense, to develop the SoTs project means to explain how we have found out about our world and altered it. This leads us to viewing the SoTs project from another perspective.

2.4.5 The Styles Project as a Study in Cognitive History

Ever since his lectures at the Collège de France (Hacking 2006a), Hacking flirted with the idea that every SoT is grounded in one or more modules, sorts of separate structures of the mind, which are the product of evolution by natural selection and can be studied from the point of view of cognitive science and neuroscience (Hacking 2009, p. 48). For example, he conjectured that there might be a module for geometrical reasoning and one for numerical and combinatorial reasoning (Hacking 2011) (see also Spelke 1994).

In addition to that, Hacking also regarded the SoTs project as an 'ecological' study (Hacking 2012) (see also Crombie 1994, p. 6). The term ‘ecology’ comes from the Greek ‘oikos’ - ‘house’, and indicates the branch of biology that studies the relations of organisms to their surroundings, their ‘house’, so to speak. So Hacking means that the SoTs project is not only about human beings and their capacities but also about the relations to their intellectual, moral and physical environment (Hacking 2012, p. 9). Finally, Hacking also used the term ‘cognitive history’ to describe the SoTs project, borrowing the term from Netz (Netz 1999, p. 6). Hacking thinks of his theory of SoTs as a further exploration of how certain cognitive capacities, within certain historical contexts, led to certain ways of acquiring knowledge (see also Netz 1999, Introduction).
In 2012 Hacking tried to put an end to the issue of defining what a style is by saying that the word SoT is a word that describes and that SoTs 'are what we use' (Hacking 2012, p. 3). I do not think that this minimal definition is helpful. It does not chime in with some of his remarks about the necessity of certain features for being a SoT, which suggest that one must say more about what a SoT is. Firstly, Hacking insists that SoTs also are ways of doing; but not all the ways of thinking we use are ways of doing as well. For example, as John Forrester reminds us (Forrester 1996), the basic pattern of common law thinking consists of going from case to case: a proposition that describes a given case is transformed into a law that applies to other similar situations. There is no kind of handwork that plays a significant role in reasoning in cases. Therefore it is necessary to establish whether (some of) the ways of reasoning we use in the sciences are also ways of doing. A notion of SoT that captures this practical aspect of scientific knowledge would help us to better characterize and order our ways of thinking about the world.

Secondly, a commitment to discontinuity for the statistical SoT and the laboratory SoT suggests that the 'sudden' emergence of new standards of evidence could be a feature of our scientific ways of thinking and doing. If so, the notion of SoT could account for their punctuated trajectories. Thirdly, Hacking has given over the years too much emphasis to certain features of SoTs, implicitly suggesting that they are necessary in the discussion of certain ways of thinking. Therefore, they should inform the content of the notion of SoT, if it has to be a useful tool for understanding the present.

In addition to that, if a notion such as that of SoT has to be central to a project that deserves names such as 'cognitive history', it should shed light on the
features exemplified by the statistical SoT. Indeed, a cognitive history should look into our ways of finding out, the new forms of explanation ‘invented’ by the human species and the relations between finding out and doing in different contexts. Ultimately, Hacking has brought up features of scientific ways of thinking that cannot be left out from the kind of inquiry he has in mind. In the next chapter I shall argue that there is a set of styles of thinking and doing that, although different one from another in their histories and in their historical trajectories, share all the features suggested by Hacking over the years and presented in this section. I wish to list all these features (the characterizing features of SoTs) below:

1) A SoT is a way of thinking and doing. In particular, a way of intervening in the world and ‘finding out “that” so and so, but also, finding “how to”. Finding out what’s true, and finding out how to change things’ (Hacking 2012, p. 3).

2) A SoT relies on a new kind of evidence for ‘finding out in the sciences’ (Hacking, 2012 p.3)

3) A SoT introduces new candidates for truth-or-falsehood, new types of explanations and/or new criteria, laws, classifications

4) A SoT is self-authenticating

5) A SoT represents a sharp break in the history of Western thought

In section 2.2 I had pointed out that the statistical SoT possesses the characterizing features 1)... 5). I had also made it clear that ‘SoT’ is the label I use for this particular set of ways of thinking and doing that have the features 1)...5). In this
section I have explained that Hacking has never listed together all the characterizing features of SoTs 1)...5): first he listed features 2), 3) and 4) (see pp. 67-68); in his more recent writings he added and stressed that when he uses the label 'style of thinking' he alludes to ways of thinking that should be thought of as characterized by features 1) and 5) (see p. 60 and subsection 2.4.4).

It is important to note that the set of SoTs is not empty: not only does the statistical SoT belong to it, as I have illustrated, but also does the laboratory SoT, as I am going to show. In the next chapter I shall argue that other ways of thinking have the characterizing features of SoTs (1)...5)) and can be called SoTs. It is my contention that by this move it is possible to provide a better characterization of scientific thinking: there will be a set of ways of thinking that will be called SoTs while other ways of thinking will satisfy only some of the characterizing features of SoTs.

2.5. The Laboratory Style of Thinking

In this section I shall show how some of Hacking's ideas can be connected with one another and used to argue that there is a way of thinking that satisfies the characterizing properties of SoTs: the laboratory SoT. Crombie placed the emergence of the experimental style in ancient Greece; Hacking retorted that experimental thinking has no real beginning because human beings have always measured and explored (Hacking 2009, p. 42). Furthermore, Crombie was disinclined to see any sharp break, any discovery of a new human potential in the long development of experimental thinking (Hacking 2009, p. 95); on the contrary,
Hacking asserted that the programme of experimentation developed in the seventeenth century introduced a novelty in the way humans used experimenting and modelling to know: the invention of a place, the laboratory, in which instruments are built in order to elicit phenomena that do not exist in isolation. This point deserves an explanation.

In his *Representing and Intervening* (Hacking 1983b), Hacking had highlighted certain features of laboratory research. For instance he had distinguished between *effects* and phenomena. Substantially, Hacking separated phenomena that can be recognized and observed without a direct intervention in the world from phenomena that do not exist in isolation but require special devices to be elicited (i.e. effects). He gave the example of the Hall effect, that is, the voltage difference induced by a magnetic field perpendicular to an electrical current. The Hall effect occurs at any time and anywhere in nature once there is a particular arrangement of conductors and magnetic fields: for Hacking, it is a kind of regularity different from, say, the rise and fall in sea level produced by the attraction of the moon and the sun. The Hall effect requires a piece of equipment to be 'noted', whereas the phenomena connected with the tidal force are 'given in nature' and only need to be observed. Hacking often uses the verb 'to create': by saying that certain phenomena are 'created' in laboratory or are 'artificial' he just wants to say that nowhere in nature is there an arrangement such as the apparatus that produces the Hall effect. More importantly, '[an effect] can be seen as a regularity only against the further background of theory' (Hacking 1983b, p. 225), that is, as I interpret him, only against a further theory of the working of the apparatuses that produce it.
The concept of the laboratory SoT is meant to account for this feature of scientific research: the production of effects. When did it start? As I have said, Hacking mooted that the production of effects settled into routine in scientific research in the seventeenth century. He drew his answer from Shapin and Schaffer's *Leviathan and the air pump*, which he considers a book about the origin of the laboratory SoT, as I am going to explain. This book presents the thesis that in the late 1650s Boyle and the English experimental community, still in its infancy, established new rules of discourse by which matters of fact had to be both generated and defended. They maintained a certain form of discourse by agreeing upon what should be considered a fact or a hypothesis, how disputes had to be settled, how matters of facts had to be explained.

Beside the air pump, the other protagonist of *Leviathan and the air-pump* is the philosopher Thomas Hobbes (1588-1679), who takes the role of Boyle’s opponent: he did not accept the conventional basis of the latter’s way of producing knowledge. Hobbes was an opponent of the social setting and the practice of acquiring knowledge, with its conventions and standards of truth, which was about to be widely adopted in scientific research.

Shapin and Schaffer tell that Boyle instructed Robert Hooke (1635-1703) to build for him an air-pump, a closed vessel (receiver) from which air could be extracted by using a pump-mechanism. Boyle produced an approximate vacuum in the receiver so to be able to perform a series of experiments to investigate the composition and compressibility of air (e.g. he repeated Torricelli’s barometric experiment enclosing a Torricellian apparatus in the receiver). These experiments were carried out in what Shapin and Schaffer call the new *place*, the laboratory,
namely a site for finding out by making: for example, finding out that the air is necessary for a candle flame by ‘creating’ the phenomenon of vacuum in a vessel.

The nascent laboratory was a ‘public’ space: to be public meant that the validation and the constitution of a piece of knowledge was obtained when all the individuals who attended the performance of an experiment agreed about what they saw. An aspect that Shapin and Schaffer contrast with ‘the private work of the alchemists […] , who produced their knowledge claims in a private and undisciplined space’ and with ‘radical individualism – the state in which each individual set himself up as the ultimate judge of knowledge’ (Shapin and Schaffer 1989 [1985]-a, p. 78). However, the new place was also ‘private’: only the opinion of those who were able to understand how an instrument works and followed certain conventions could contribute to the validation of knowledge.

Shapin and Schaffer’s book is considered one of the most influential approaches to social constructivism. For instance, Jan Golinski wrote that ‘[for Shapin and Schaffer both the laboratory] and the wider realm beyond its walls can be viewed as an arena in which knowledge is constructed’ (Golinski 2005 [1998], p. 37). Indeed Shapin and Schaffer argued that, ‘Boyle’s technologies [of persuasion] contributed to a common strategy for the constitution of the matter of fact […] That is to say, each technology functioned as an objectifying resource’ (Shapin and Schaffer 1989 [1985], -a p. 77). In this sense, Hobbes against Boyle, as conceived by Shapin and Schaffer, is a quarrel about what should count as evidence: is evidence what we find among us, as Hobbes thought, or is it what apparatuses produce, as Boyle suggested? Both Hobbes and Boyle, argue Shapin and Schaffer, had justified and rational arguments, but those of Hobbes had no force: had the social context
been different, Hobbes' objections would have found a different reception. For Shapin the social world of seventeenth-century England with its values of civility, honour and integrity formed the basis in which knowledge was established (Shapin 1994). These ideas fit the constructivist standpoint according to which truth is the product of contingent historical or social forces.

2.5.1 New Evidence and Discontinuity

Hacking wrote that '[Hobbes] saw that [Boyle's] was a new and threatening style of reasoning' (Hacking 1991a, p. 240). 'Hobbes saw' because he was aware of the conventions by which knowledge was acquired and validated in the new place that developed into the laboratory. Put differently, Hobbes was able 'to see from outside' the features of the new emerging style and did not accept them. The first of these features is represented by a new type of evidence: for Boyle 'evidence' was not only what can be found as 'given' in nature but also what can be made by man. Hobbes, for his part, refused to accept that the result of an experiment could represent the evidence on which to base our claims of knowledge. His point was that all the experiments conceal theoretical assumptions, e.g. about the functioning of the apparatus and the set up of the experiment, whose validity could always be disputed. For him, whatever explanation was made to account for an artificial phenomenon, there is always a superior explanation that proceeds from different assumptions about the experiment.

The emergence of a community of people that used as a new type of evidence phenomena produced by devices such as the air pump, which did not previously exist in isolation, is a punctuated event in the history of science. To
illustrate this point, let us look at the social context in which Galileo worked. Although just a handful of years separates Galileo and Boyle (Boyle was sixteen years old when Galileo died) their milieus could not have been more different. Mario Biagioli has argued that ‘Galileo and his readers did not belong to a professional community that could provide the kind of credit and rewards he sought’ and that in his field there was a ‘lack of consensus about style of argumentation and standards of evidence as well as [a] scant interdependence among the members’ (Biagioli 2006, p. 97). Biagioli shows that Galileo discoveries were discussed in an intellectual milieu radically different from that of Boyle. The former’s social setting included scholars dispersed over different universities and courts linked through correspondence networks; the latter included a tiny elite of people in London, who explored the ‘secrets of nature’ in the same public place. Galileo’s milieu could do little to favour the establishment of new standards of truth; the mutual trust of the gentlemen of London’s scientific societies was crucial for the coming into being of a new form of evidence.

How radical was the metamorphosis brought about by Boyle’s community appears even more evident when one considers that until the first decades of the seventeenth century it was impossible to trace a demarcation line between ‘magicians’ and ‘scientists’, i.e. between the hermetic tradition and the emergent laboratory style. Many scholars, included Boyle, accepted various hermetic doctrines (see Trenchard More 1941) and were committed to the laboratory style. But in the span of Boyle’s life, ‘secrecy for science became a disvalue’ (Rossi 2000, p. 34) -- making one’s knowledge public became a value whereas keeping one’s opinions secret came to be considered akin to treason.
Did the creation of phenomena mark out the beginning of a new style of thinking and doing? I shall answer this question in the next subsection.

### 2.5.2 A New Way of Thinking

Once a question about the world is asked, e.g. whether or not the flame of a candle needs air to burn, finding out the answer in the style of laboratory means, one can imagine, to perform three types of mental and physical actions. The first type comprises actions such as to design and calibrate a machine that, as Shapin and Schaffer say, 'stands between the perceptual competences of a human being and natural reality itself' (Shapin and Schaffer 1989 [1985], p. 77). These actions involve not only the brain but also the body:

> Doing is more important for the laboratory than for mathematics. [...] In the hypothetical-deductive reasoning of laboratory science I include the making and the calibrating as well as the designing and the deducing (Hacking 1991a).

The second kind of mental actions is illustrated by this passage:

> The final 'working' of air pump technology is a matter of using a lot of different plastic resources -- the material apparatus, the background theory, ideas about what the machine does, and how things in it behave - each of which is adapted to the other (Hacking 1991a, p. 237).
In brief, the actions of the second type are represented by choices that make different epistemological elements of the experiments cohere: what is considered a fact, theories about nature, the set up of the apparatus and the pieces that constitute it, the interpretation of data. For example, the actions of the second type include: to distinguish matters of fact from speculative hypotheses and to establish how theory and interpretation of the results of the experiment are connected.

Finally, the previous two types of mental and manual actions are made visible to everybody so that the knowledge produced becomes the result of a collective enterprise. This is the aim of the third type of actions. Particular rules of discourse and conventions of social relations are followed, e.g. what is to be considered as ‘the experimental fact’ is isolated and described in detail so to facilitate its replication by as many people as possible.

In Shapin’s account, the third type of actions evolved out of the early program of Boyle and the Royal Society, who wanted to ‘build a solid factual foundation for a reformed natural philosophy by soliciting more and more testimony and extending networks of justified trust further and further’ (Shapin 1994, p. 205) -- by the third type of actions the fact becomes objective.
2.5.3 Techniques of self-vindication

The actions of the second type are relevant for grasping Hacking’s thesis that the laboratory SoT is self-authenticating. Shapin and Schaffer explained that Boyle, in his *Two Essays Concerning the Unsuccessfulness of Experiments*, offered a repertoire of reasons why an experiment might not produce the expected outcome: from the construction defects of the materials to their composition, from the context of the experiment to the background theories involved (Shapin and Schaffer 1989 [1985], p. 185). However, they noted, he never considered any of these reasons to reject the idea of air’s pressure: for example, when two cohered thin discs placed in the bulb of the air pump did not separate as expected for the absence of air, Boyle blamed the leakage and continued to refuse his opponents’ idea of *horror vacui*.

Shapin and Schaffer concluded:

Any physical hypothesis could be saved from an admitted experimental failure either by pointing to a range of subsidiary hypotheses or by modifying the key hypothesis so that this could be seen as unaltered for all practical purposes (Shapin and Schaffer 1989 [1985], p. 186).

In 1992, in the paper ‘The Self-Vindication of the Laboratory Sciences’ (Hacking 1992b), Hacking made a point that can be put in relation to Shapin and Schaffer’s: laboratory sciences tend to produce a sort of self-vindicating structure in that background theories, hypotheses, modelling of apparatus and data analyses are mutually adjusted to each other.

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7 I should make it clear that Hacking has never distinguished different types of actions within a SoT.
To start with, the concept Shapin and Schaffer express in the passage above could be reformulated in terms of the Duhem thesis: in the *modus tollens* if \( p \) entails the evidence \( e \) then \(-e\) implies \(-p\); however, Duhem observed that it is always \( p \) plus \( a \), where \( a \) is the whole of auxiliary hypotheses and background knowledge, rather than only \( p \), which implies \( e \). Therefore, if \(-e\), one can only conclude that either \( p \) and/or one of the auxiliary hypotheses included in \( a \) are/is false. Going by the *logical* point made by Duhem, Hacking pointed out fifteen different ‘plastic resources’, included in \( a \), which can be modified in order ‘to keep \( p \) alive’. Here is his full list of the plastic resources:

1) *questions* about theories or the subject matter of the experiment;

2) *background knowledge*, that is those beliefs that are taken for granted;

3) *systematic theory*, namely general theories of high level about the subject matter of the experiment (e.g. quantum theory can be taken as the systematic theory for the Stern-Gerlach experiment);

4) *topical hypotheses*, that is rules that connect theoretical concepts or symbols with observational terms (e.g. how the concept of speed vector is connected with the observed values of the motion of a body);

5) *modelling of the apparatus*, theories or background lore about how the apparatus works;

6) *target*, which indicates a part of the experimental material, such as a population of bacteria to be studied;

7) *source of modification*, it is that part of the experimental apparatus that interferes with the target, e.g. a beam of light;

8) *detectors* to measure the modification of the target;
9) tools, which are the objects on which an experimenter must count, such as a chemical substance or a ruler;

10) data generators, which are particular tools such as a camera or anything that counts events;

11) data such as graphs, tables, photographs, measurements of physical magnitudes and so on;

12) data assessment such as statistical or not statistical estimations of errors;

13) data reduction such as theory neutral mathematical methods to transform types of data into other kind of data, e.g. Fourier transforms;

14) data analysis, a kind of data reduction that is not theory neutral;

15) interpretation of the data, which is always made by using a theory (Hacking 1992b) (Hacking 1988a).

Hacking thinks that in laboratory research there is an interplay not only between theory and observation, as certain scholars had suggested before him (Pickering 1984, p. 8), but also among all the fifteen elements listed above:

We can 1) change questions; more commonly we modify them in mid-experiment. Data 11) can be abandoned or selected without fraud; we consider data secure when we can interpret them in the light of, among other things, systematic theory 3) [...] Data processing is embarrassingly plastic [...] in the case of data assessment and reduction, 12) and 13) [...]. Data analysis is plastic in itself; in addiction, any change in topical hypotheses 4) or modelling of the apparatus 5) will lead to the introduction of new programs of data analysis (Hacking 1992b, p. 54).
Hacking’s conclusion is that ‘our preserved theories and the world fit together so snugly less because we have found out how the world is than because we have tailored each to the other’ (Hacking 1992b, p. 31). For him theories are not refuted or confirmed by a direct comparison with the world but by making all the other fourteen items in the list above cohere. If anything, he thinks, we should limit ourselves to saying that the sciences of laboratory are true to the phenomena elicited by man-made machines. This feature makes the laboratory science a sort of self-vindicating and stable structure. Turning to the terminology of the SoTs, I express his point by saying that there is a circularity in the way of thinking and doing of the laboratory style: in order to confirm theories, experimental apparatuses that provide data are made; and how do we know that the experimental apparatus has been designed properly and the data are correct? By checking whether or not the data fit our theories. That is to say, the laboratory style possesses a technique of stabilization that consists of the iterative back and forth, whereby the fifteen elements above are compared and discussed. In a nutshell, the symmetrical exchanges between theories and data that go on in laboratory research make its style self-authenticating.

The practice of climate science is a case in point. It would be wrong to say simply that empirical data validate climate models, as it is shown by a study of

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8 It is important to note a point that will be clearer later: Hacking does not draw constructionist conclusions from the plasticity of the fifteen items. He only notes that this plasticity makes SoTs self-authenticating and therefore science stable (see Hacking 1988a). For him only a little number of the possible combinations of the fifteen items above can endure. As he says, ‘Not all the descriptions [of a phenomenon] can be accepted but there is no reason for thinking that there is only one possible description’ (Hacking 2005b, p. 5). His metaphysics is anti-constructivist and pluralist.
Hélène Guillemot (Guillemot 2010). She explained that the simulations in climate science are confronted with data provided by three types of sources: networks of meteorological stations; instruments on satellites; data collected during field experiments. These sources do not provide 'raw data': at least three models that combine the initial data with other factors are necessary to acquire the surface temperature from a signal captured by a satellite; and computer models are employed to harmonize all the data coming from the surface stations (Guillemot 2010, p. 245). Among the different kinds of validation of models, Guillemot outlines the features of the so-called 'bottom-up validation' used for example for the parametrization of clouds in the global climate model. In that case, the model equipped with the parametrization is tested by comparing it with another model validated by observation (Guillemot 2010, p. 247). In other cases models are used to complete the data or to test the methods of their interpretation (Guillemot 2010, p. 249). Therefore, the practice of climate studies reveals that there are symmetrical exchanges between many elements mentioned by Hacking.

2.5.4 New candidates for truth or falsehood

Up to now, I have described some features of the laboratory style. It is a new way of thinking and doing that involves three types of mental and physical actions originally practised within a small community of thinkers in the seventeenth century. These thinkers brought to the fore a new type of evidence – the evidence produced by man-made phenomena – by which the knowledge produced was validated. Hobbes refused to embrace this new criterion of validation of
knowledge and did not accept the rationale of the three types of mental and physical actions. He maintained that the philosopher's model for knowing consequences and causes was provided by geometry. Since the postulational SoT adopted in geometry is a SoT, as I shall show, it is possible to say that Hobbes adopted a SoT, the postulational one, different from that adopted by Boyle, the laboratory one. Notice that Boyle opened a new space of discourse in which new questions were admissible, a feature that, as I shall explain, belongs to all the SoTs. In Shapin and Schaffer's words:

Was the Torricellian space a vacuum? Did the exhausted receiver constitute a vacuum? The platform from which Boyle elected to address these questions was experimental: the way of talking appropriate to experimental philosophy was different in kind to existing natural philosophical discourse (Shapin and Schaffer 1989 [1985]-a, p. 45)

The last feature of the laboratory style I shall deal with concerns the point that the birth of the laboratory style brought into being new candidates for truth or falsehood. Hacking provided examples of both observational statements whose truth-value is independent of the laboratory style and theoretical statements whose truth-value is determined by that style itself (style-dependent statements). He noted that the sentence ‘my skin is warmed’, which is used by William Herschel (1738-1822) in his theory of heat to describe the effect of filters of some

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9 Shapin and Schaffer cite Hobbes' Leviathan: 'As when we know, that, if the figure shown be a circle, then any straight line through the centre shall divide into two equal parts [...] And this is the knowledge required in a philosopher' (Shapin and Schaffer, 1989 [1985]).
colours on his skin, is of the former class and referred to it as a ‘sense-datum sentence’ (Hacking 2002 [1982] -a, p. 173). Conversely, Hacking cited another sentence formulated by Herschel:

The heat which has the refrangibility of the red rays is occasioned by the light of those rays.

According to Hacking this sentence is style-dependent:

If another culture has acquired the styles of reasoning enumerated by Crombie, it can perfectly well learn Herschel’s physics from the ground up – that is just what I do in making sense of Herschel’s text. The problem is that the sufficiently foreign person will not have Herschel’s kind of sentence as the sort of thing that can be true-or-false, because the ways of reasoning that bear on it are unknown [...] [Archimedes] would not be able to effect a translation until he had caught up on some scientific method (Hacking 2002 [1982]-a, p. 173).

In 1800 Herschel proved that the different colours of the spectrum are of different temperatures and discovered the infrared radiation of sunlight (Herschel 1800b). He used prisms or filters (i.e. ‘man-made devices’) in his telescopes to create a ‘new effect’: the separation of the rays of light; then he measured the temperature of each of them by a thermometer. In some cases he even tried to feel the rays from different filters as heat on the skin.
Hacking argues that Archimedes did not possess the SoT of Herschel and therefore he could not make sense of Herschel’s sentence. To interpret Hacking one can focus on the fact that the important novelty introduced by the laboratory style – that phenomena elicited by apparatuses constitute the evidence for certain propositions – is also an essential element of what, since the seventeenth century, is considered the scientific method. Bearing this in mind, Hacking’s point would amount to say that Archimedes could not make sense of a sentence such as ‘the heat which has the refrangibility of the red rays is occasioned by the light of those rays’ because its sense hinges on a new kind of evidence which emerged only at Boyle’s time.

Although it is clear enough that Archimedes did not possess the methods that bear on Herschel’s sentence, I do not think that Hacking’s is the best example that can be given. Herschel’s effect was not really a phenomenon that ‘did not exist in isolation’: it is well known that a spectrum of light is naturally produced in the sky by reflection and refraction of sunlight by rain droplets in the atmosphere. A better example might concern the early experiments on the scattering of alfa-particles in Rutherford’s laboratory. These experiments elicited a phenomenon never seen before: we need a particular arrangement of experimental apparatuses to see the effects of the scattering of a beam of charged particles. So, an example of style-dependent (theoretical) sentence in the laboratory SoT is:

The scattering has cylindrical symmetry about the beam axis and therefore the cross section is independent of the azimuthal angle.
Among the style-independent sentences, there is a particular class that I want to bring to the fore for my purposes in chapter seven. In his book *Representing and Intervening* (Herschel 1800a, 1800c), Hacking defended a particular form of realism according to which the word ‘real’ refers to the possibility for a class of entities of affecting macroscopic objects. He is a realist about entities such as electrons because we can use them to intervene in the world affecting other objects. For example, he says that what convinced him of realism was the fact that in some experiments scientists succeed in spraying positrons to increase the charge of a niobium ball:

If you can spray them then they are real (Hacking 1983b, p. 22).

The point I want to mention here and explain in detail later is that for Hacking the causal effects produced by particles are described by observational propositions, i.e. propositions that are not ‘theory-loaded’, a catchword used by Hacking to say that they do not carry a ‘load of theory’. For him propositions such as:

The gauge indicates that the niobium ball is altered

must be considered observational and consequently style-independent because their sense, Hacking claims, hinges on a kind of evidence – the ‘evidence of senses’ – which is ahistorical.

To sum up, in this section I have connected some ideas of Hacking scattered in different writings in order to make this point: around the time of
Boyle a new style of thinking and doing, which satisfies all the characterizing properties satisfied by the statistical SoT, emerged. From now on, I shall refer to it as the laboratory SoT. It is a corollary of my argument that Shapin and Schaffer’s account in *Leviathan and the Air Pump* can well be reread as a quarrel between two men that adopted different SoTs.

### 2.6 Conclusions

In this chapter I have presented Hacking’s SoTs project, a set of suggestions concerning the thesis that in history of science it is possible to recognize the emergence and the development of a few styles of thinking and doing. In order to explain this project, of which I have still to say a lot more, I have started by giving the example of the statistical SoT: I have reread the book *The Emergence of Probability* in terms of the notion of SoT. In the following sections I have used this example to explain better how Hacking characterizes the notion of SoT. Then I have explained that *Leviathan and the Air Pump* can well be reread as a quarrel between two men that adopted different SoTs. My analysis in this chapter has shown that many points of the styles project need to be developed in order to make Hacking’s suggestions into a full-fledged theory. Such a theory could help to characterize scientific thinking by exhibiting the SoTs that compose it.
Chapter Three: Developing the Styles Project: Towards a Theory of Styles of Thinking

3.1 Introduction

In this chapter I shall be engaged in turning the SoTs project into a more comprehensive analysis of scientific thinking. One of the points implied by Hacking's thoughts is that two ways of thinking, the statistical and the laboratory SoT, share a set of features ('the characterizing properties of SoTs' presented in section 2.2). The label 'SoT' wants to suggest that, despite their differences, these ways of thinking are species of the same genus.

Still at issue is how we can further characterize scientific thinking: can we identify other SoTs so to provide a more accurate description of it? It is a safe guess to say that the spectrum of our ways of finding out about the world is quite broad: there might be other ways of finding out that possess all the characterizing properties of SoTs in addition to ways of thinking that do not satisfy them. Actually, I shall put forward two related theses. My first thesis is that, beside the statistical and the laboratory SoT, there are at least four other SoTs. My second thesis, which I shall discuss in the next chapter, is that the spectrum of scientific
ways of finding out also includes ‘spurious SoTs’, as I shall call all those ways of thinking that fail to satisfy one or more characterizing properties of SoTs. To prove the first thesis, in the following four sections I shall argue that the algorithmic, the postulational, the historico-genetic and the taxonomic way of thinking possess the characterizing properties of SoTs. My argument for the second thesis is contained in section 4.2: I shall consider most of the candidates for being SoTs presented in the literature and explain why they do not possess all the characterizing properties of SoTs.

The brief comments on each of these four SoTs provided by Hacking are not sufficient to give a full account of their properties and in some cases I shall object to them. However, in his discussions Hacking has drawn the reader’s attention to certain historical works that provide profound insights into the nature of these SoTs. I shall rely on these works, although they have not been written within the framework of the idea of SoT. Nevertheless, other sources will receive my attention for their connections with my claims.

In the following chapters, when it will be necessary to address philosophical questions related to the SoTs project, I shall often refer to the SoTs discussed in this chapter and in the previous in order to make my points clearer. However, the conclusions I will draw will be independent from my own characterization of scientific thinking. They will follow from Hacking’s characterization of what a SoT is, i.e. from its characterizing properties.
3.2 The Algorithmic Style of Thinking

When in 1992 Hacking hinted that there is a SoT of applied mathematics distinct from the postulational, he called it ‘algorismic’\(^\text{10}\) (Hacking 2002 [1992], p. 185). This term stems from ‘Algoritmi’, the transliteration of the name of the Persian mathematician al-Khwārizmī (780-850) whose works, translated into Latin in the twelfth century, introduced to Europe several algorithms. As the historian of mathematics Carl Boyer put it, ‘[al-Khwārizmī’s work] is to algebra what Euclid’s *Elements* is to geometry’ (Boyer 1968, p. 258): for Hacking it was with the introduction of algebra that the algorithmic way of thinking flourished in Europe (Hacking 2002-2003, p. 543).

The use of the term ‘algorismic’ instead of ‘algorithmic’ reminds us that this way of thinking has to do with following step-by-step procedures in *calculations* rather than in more general problems that do not involve numbers. Indeed, while in the Middle Ages the Latin name ‘algorismus’ denoted a genre of works on methods of calculations (such as *Carmen de Algorismo* by Alexander de Villa Dei (1175-1240) or *Algorismus Vulgaris* by John of Sacrobosco (1195-1256)) (Karpinski and Waters 1928, p. 45), the term algorithm emerged much later from ‘algorismus’ and took on the *generalized* meaning of a set of rules to be followed in problem-solving operations. At any rate, Hacking regarded the algorithmic way of thinking as a *mathematical* SoT that, together with the postulational one, forms the ‘style of mathematics’ (Hacking 2002-2003, p. 543): to adopt the algorithmic way of

\(^{10}\) To my knowledge, Hacking has never used the term ‘algorithmic’ in English. Though, in French he has used the term ‘algorithmique’ rather than ‘algorismique’. As I shall explain below I prefer to call it ‘algorithmic’ (Hacking 2006c, p. 7).
thinking means to follow methods of calculation, i.e. step-by-step list of rules that might be represented by a formula.

In his latest overview of the SoTs project (Hacking 2012) Hacking did not even mention the algorithmic way of thinking. By contrast, I believe that this way of thinking cannot be left out from any account of scientific thinking since it occupies a vital role in all the branches of scientific knowledge. Beside the intimate connection of knowing and doing, a theory of scientific thinking should also highlight the fact that the sciences combine qualitative and quantitative methods of analysis. Mathematics does not only give a formal expression to disciplines such as physics or economics but also provides a way of seizing the measure of certain magnitudes. According to Michel Paty, from nineteenth century science, ‘measurable quantities […] have meant uniquely, and perhaps restrictively, quantities taking numerical values, as it was most usually the case in classical physics’ (Paty 2003, p. 126). The magnitudes to be calculated are not only those that already exist in the physical corpus, they are also introduced by scientists (and expressed by functions or constants): entropy in physics, affinity in chemistry, volatility in economics are some examples (Lacki 2003, p. 274). The use of the algorithmic way of thinking is therefore ubiquitous in scientific research. Incidentally, in order to know the value of a physical constant it might be necessary to perform a measurement that consists of step-by-step procedures. As I intend to account for this aspect of scientific research too, I prefer to use the term ‘algorithmic’ rather than ‘algorismic’: I want to describe a SoT that accounts not only for the use of standard procedures in calculations but also for the use of

\[ \text{Footnote 11} \text{ Paty provides a review of the conceptions of quantity or magnitude over the centuries and investigates how these conceptions have legitimated the mathematization of physics.} \]
standard procedures to be followed in more general problem solving operations that may involve numbers. So to call this SoT 'algorithmic' seems to me more appropriate. In the following lines I shall describe the 'algorithmic style of thinking' and argue that it is a SoT. Let us start by giving an example.

3.2.1 Algorithmic Thinking: an Example

The first known algorithms in history go back to civilizations more ancient than Greece. In particular, they can be found in one of the Egyptian papyri that have survived the ravages of time, known as 'Rhind Mathematical Papyrus' or 'Ahmes Papyrus' in honour of Ahmes, the scribe who copied it down in around 1650 BC. One of the sentences in the papyrus states:

There are 7 houses, 49 cats, 343 mice, 2401 ears of grain

The scribe was providing an answer to this problem:

In each of seven houses
there are seven cats
each of which eats seven mice,
each of which would have eaten seven ears of grain.

Adding up houses, cats, mice, ears of grain, how many objects are there in total?

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12 I shall focus my discussion on the Babylonian and the Egyptian civilizations. However, my arguments could also be tested against examples drawn from the mathematics of Mayan people between the third and ninth centuries (see for example Magli 2009 [2005]).
13 The Egyptian history is usually divided into three large periods that cover a period from 3100 to 332 BC: the Old Kingdom, the Middle Kingdom and the New Kingdom. The Ahmes papyrus belongs to the Middle Kingdom, between about 2050 and 1600 BC.
14 The Ahmes papyrus presents several arithmetical, algebraic and geometrical problems. For a review of the mathematics in the Ahmes papyrus see: (Boyer 1968, pp. 12-23)
This riddle can be iterated adding as many objects as one wants, e.g. one can imagine that each of the seven ears of grain had seven seeds and each seed was made of seven items and every item contained seven objects and so on.

The simple answer to the riddle is that there are

\[7 + 7 \times 7 + 7 \times 7 \times 7 + 7 \times 7 \times 7 \times 7 = 7 + 49 + 343 + 2401 \text{ objects}\]

Today, a mathematician would recognize at first glance that these numbers represent the first four terms of a geometric series of ratio 7 whose sum of the first \(n\) terms is given by a standard formula. By performing a standard procedure consisting of successive steps, the Egyptians were able to solve the riddle whatever the total number of the objects added by iteration was. For example, if the iteration stopped at \(n\), the first step to take would have been to consider \(n\) terms of which the generic \(m\)th term of the series was 7 multiplied by itself \(m\) times. The other steps would have been to work out each of the \(n\) terms of the sum by performing the multiplications. According to Boyer (1968, p. 15), in Ahmes' day multiplication were performed by a method of successive doubling. Indeed, by using an abacus it is easy to add up, multiply and divide by 2. Suppose they would have to calculate the fourth term of the sum, which is 7 \(\times 7 \times 7 \times 7 = 7 \times 343 = 2401\)\(^{15}\). Since 7 = 4 + 2 + 1, they would have doubled 343 and got 686, then doubled 686 and obtained 1372, i.e. four times 343. Afterwards, they would have add up 1372 + 686 + 343 obtaining 2401. By using the abacus this is an easy task.

\(^{15}\) Of course the Egyptians did not denote numbers as we do. They had a system of numeration derived from base ten and did not possess a placed-valued system. When operations were performed they grouped together the symbols in no particular order.
because to double requires a small number of operations. The Egyptians also had another very similar method to work out multiplications: it is today known as the Russian farmer’s method and involves divisions and multiplications by two. Thus, in principle, they were able to ‘check’ a result by using different methods of calculation. To sum up, in order to provide the answer to the riddle, whatever the number n of iterations, the Egyptians used the algorithmic way of thinking: they followed a procedure expressed by a list of well-defined instructions, which involved the use of the abacus.

The Egyptians used algorithms to solve many arithmetical problems, such as that of finding solutions of linear equations (Boyer 1968, p. 16) or that of calculating areas and volumes (Boyer 1968, p. 19-23). On the other hand, the Babylonians were able to calculate the correct values of many astronomical phenomena even though their geometry was not as developed as in the ancient Greece: for example, by measuring and implementing algebraic algorithms they managed to make predictions of the phenomenon of lunar eclipses every eighteen years, the so called Saronic cycle (Mason 1962, p. 19). The ancient Greeks made use of algorithms too: the Euclidean one for finding the greatest common divisor of two integers and the numerical solutions of algebraic equations in Diophantus’s *Arithmetica*, which was read by the Arab mathematicians of the ninth century, are the examples that stand out.

In all the historical examples I have given above, to reason in the algorithmic SoT meant to follow a set of elementary rules in order to reach a certain objective. This way of reasoning is fundamentally distinct from that used
in proving a theorem. I interpret Hacking in the passage below as making just this point:

There is nothing perspicuous about calculation. There is no feeling of compulsion. There is no sense of understanding (Hacking 2000b, p. 101).

Applying an algorithm to solve Ahmes' riddle is different from thinking in order to demonstrate that an angle inscribed in a semi-circle is a right angle. In the former case there is no feeling of understanding; in the latter, there is a strong feeling that the conclusion must follow from two facts: the base angles of isosceles triangles are equals and the sum of the angles of a triangle is equal to 180 degrees. Whereas the steps to be taken in order to solve Ahmes' riddle iterated $n$ times are fixed, within a line of reasoning from hypotheses to thesis every statement must be deduced from the previous one.

Algorithm and demonstration are the two faces of mathematical thinking. A mathematician has to switch from the way of thinking of demonstration (the postulational SoT described in the next section), which allows her to prove a theorem, to the algorithmic way of thinking, which for example allows her to determine the result of an operation, a number.

3.2.2 Techniques of self-vindication

As I have pointed out, the Egyptians were able to use different algorithms for solving the same problem; therefore, one of these algorithms could serve as a means for checking the result obtained. Examples are: the already mentioned
method of successive doubling and the ‘Russian farmer’s method’ for performing multiplications; the ‘method of false positions’ and the ‘method of factorization’ for solving linear algebraic equations (see Boyer 1968, p. 17).

In other words, the algorithmic way of thinking does not answer to any criterion except its own: the criterion to prove whether or not what has been found out is correct still relies on another algorithm. In this sense the algorithmic SoT possesses what Hacking would call a technique of self-vindication: ‘the solution of the riddle iterated \( n \) is \( x \)’ is what we conclude by using the algorithm \( A \); and how do we know that we are correct? We know that because by using another algorithm, the algorithm \( B \), we find \( x \). That is: the algorithmic way of thinking is self-authenticating.

3.2.3 Mathematical Ways of Thinking in Egypt, Babylonia and Greece

Many of the 110 problems in the Rhind and Moscow papyri, from which we derive our knowledge of Egyptian mathematics, have a practical origin. Similarly, Babylonian cuneiform tablets do not present general formulations in geometry but deal either with specific practical problems or with more ‘theoretical exercises’ for the joy of pure mathematics, to be worked out according to a certain algorithm (Høyrup 1990b, p. 65)\(^{16}\). Referring to the contrast between Greek and Babylonian (or Egyptian) mathematics Jens Høyrup defined the former ‘scientific knowledge’ and the latter ‘sub-scientific knowledge’: while the ancient Greeks pursued scientific knowledge *systematically* and *for its own sake*, the Babylonians and the

\(^{16}\) The Babylonian texts to which Høyrup refers belong to the Old Babylonian period, from 2000 to 1600 BC.
Egyptians acquired and transmitted only sub-scientific knowledge, that is, a specialist knowledge pursued only *in view of its applicability* (Høyrup 1990b, p. 64 and p. 71). The Egyptian and the Babylonian mathematicians did introduce problems with no practical application such as Ahmes’ riddle but they were determined by methods already at hand. On the contrary, ‘Greek mathematics [was] determined by *problems* for the solution of which new methods would have to be developed’ (Høyrup 1990b, p. 65).

The Babylonian mathematicians did not have an algebraic notation as transparent as ours. They represented their algebra by using a geometry of measurable segments, squares and rectangles and: ‘[their] prescriptions describe[d] what is done in the geometric representation, just as we may describe in words what we are doing to the equation’ (Høyrup 2010, p. 34). According to Donald Knuth, Babylonian algebra consisted of step-by-step lists of rules, i.e. of algorithms for representing algebraic equations and, ultimately, computing them (Knuth 1972, p. 672). In brief, in Knuth’s understanding the use of algorithms is the only relevant insight to read Babylonian mathematical texts. Høyrup stigmatized this view as a mistake built on the translations current at the time (e.g. Neugebauer and Sachs 1945) and gave his own interpretation (Høyrup 2010): as we do, the Babylonian mathematicians presupposed the existence and the properties of the objects they were looking for and operated on geometric representation in order to find them. Not only the algorithmic thinking but also decision-making was involved: ‘[Babylonian texts] describe a particular type of geometrical manipulation, which like modern equation algebra is analytical in
character, and which displays the correctness of its procedures without being explicitly demonstrative' (Høyrup 2010, p. 1 my emphasis).

These points lead to the conclusion that the algorithmic way of thinking had already settled down into a routine in the Egyptian and Babylonian civilizations. Importantly, by no means can we conclude that Babylonians did not have a 'reasoned' mathematics, as Høyrup explains:

> Procedures were described in a way which [...] turns out to be as transparent as the self-evident transformations of modern equation algebra and in no need of further explicit arguing [...] (Høyrup 2012, p. 17).

Nevertheless,

> No surviving text suggests that all this was ever part of an explicitly formulated programme, nor do the texts we know point to any thinking about demonstration as a particular activity (Høyrup 2012, p. 17)

This passage suggests we should think of the Babylonian mathematicians as possessing an algorithmic way of thinking but not a demonstrative way of thinking: as I shall explain in the next section, only as a consequence of a different historical and social environment did a style of demonstration (postulational SoT) emerge in the ancient Greece.
3.2.4 Doing

By saying that ‘mathematics is embodied’ and that ‘it is in the hands and in the arms that move around’ (Hacking 2011) Hacking reminded us that the first men who counted needed to perform precise acts with their hands. For example, to solve Ahmes’ riddle meant to carry out automatically multiplications by manipulating pebbles from one side to another of a tablet. Similarly, as I have already said, the Babylonian algebra had a geometrical representation; and some operations that we might call ‘addition’ and ‘subtraction’ consisted of specific actions: they were concrete operations. To make few examples: ‘to append’ meant to join an entity to another one, which conserves its identity; ‘to tear out’ meant to remove an entity from another quantity of which it is part; ‘to compare’ meant to observe that a length or an area exceeds or fall short of another (Høyrup 2010, p. 6-7). The above-mentioned concrete operations upon objects, which were involved in the implementation of Egyptian and Babylonian algorithms, show that the algorithmic SoT is a way of doing.

3.2.5 The Emergence of the Algorithmic style of Thinking

Research shows that basic numerical competences are rooted in biological built-in resources that can be explored in animals and humans (for a review of this research see Nieder 2005). Different species of animals have the sense of the cardinality of a set, although they have distinct upper-numerosity discrimination limits (Köhler 1957) (see also Davis and Perusse 1988). On the other hand, pre-verbal human infants possess a sense of the numerosity of a set of objects and can engage in basic additions and subtraction operations with objects (Wynn 1992)

17 Interestingly, the Babylonian way of doing mathematics has gone extinct. This shows that, even if a SoT is long-lasting, the ways of implementing it can die out.
(Feigenson, Dehaene, and Spelke 2004); Amazonian populations that lack words for numbers beyond five can compare large sets of items (Pica et al. 2004).

Furthermore, it seems that humans have an innate ability to follow recursive rules: procedures for finding the number of elements in finite sets such as finger counting were in use in human societies before the invention of writing. For example, according to the researchers who found it, a bone of a baboon marked with 29 notches and dated to approximately 35,000 B.C was used as a calendar stick (Bogoshi, Naidoo, and Webb 1987). To record the passage of days it was sufficient to follow a simple rule: for each sunset a notch had to be etched into the bone.

However, although our capacity for number as well as the faculty of language relies on recursive rules (Hauser, Chomsky, and Fitch 2002), neither the existence of fixed procedures for counting nor the sense of numerosity implies the existence of an abstract concept of number. To acquire this concept it is necessary a step forward: to represent a particular value by a symbol or a word, i.e. to represent a feature of sets of objects that is independent from the characteristics of the objects themselves – this feature is the cardinality of a given grouping. In as much as the concept of number implies the idea of a magnitude to be represented, it requires the existence of a language that makes it possible to process numerical information with precision. When did this concept emerge? Certainly, when the first human societies started to make use of commodity money a concept of number had to come about: money is a measurement of value (as well as a medium of exchange and a store of value). Therefore, I maintain that the algorithmic SoT came to the fore when the concept of number came about, in turn a consequence
of the emergence of complex social structures. We may think of the birth of the algorithmic SoT as a discontinuity in human history by considering that small groups of hunter-gatherers possessed only the ability of finger counting and had no symbolic representation of number (Rudman 2007, chapter one). It was only with the Neolithic transition from a life-style of hunting and gathering to one of settlement and agriculture (about 10 thousands of years ago) that an abstract concept of number emerged. According to Peter Rudman, the concept of division and of prime number came to the fore after 10,000 BC (Rudman 2007, chapter one).

The Neolithic transition, also called ‘the Agricultural Revolution’, was very rapid, as archaeological and demographical studies have documented (Bocquet-Appel 2011). It is plausible to say that it produced a sharp break between the way of thinking and doing of the hunter-gatherers and that of the first complex communities who computed land areas or granary volumes by using algorithms.

3.2.6 New Sentences, Evidence

My last point is that there has been a time in which a sentence such as

The number of objects in Ahmes' riddle is given by multiplying the following three terms: $7 \times 7, 7 \times 49, 7 \times 343$ and adding up them to 7

had no truth-value. It was a time in which, in order to make sense of that sentence, people would have had to possess an abstract concept of number and share the algorithmic way of thinking and doing. Humans may well have an innate ability to follow step-by-step procedures, but only when the first complex societies came
about did they use algorithms as evidence of the truth of certain (style-dependent) propositions. In order to make sense of the sentence above, those humans who lived before the Agricultural Revolution would have had to possess certain concepts, standards of truth, methods, in brief, a way of thinking and doing. For them it would not have been so important whether the sentence was written by using the Egyptian numeral system or our placed-value system: how to use a numeral system can be learned. The point is that only for someone who is part of a community that shares the algorithmic way of thinking and doing does the sentence above acquire a truth-value.

In chapter eight, I shall connect some Wittgenstein's thoughts with my considerations about the self-authenticating character of the algorithmic way of thinking. For the time being, my conclusion is that the algorithmic way of thinking and doing satisfies all the characterizing properties of SoTs and, therefore, deserves the label of 'algorithmic SoT'.

3.3 The Postulational Style of Thinking

This section contains a broad characterization of the deductive way of thinking involved in the demonstrative proofs of geometry. As I have explained, according to Høyrup the deductive method emerged in ancient Greece and was not a Babylonian practice. Another scholar, the already mentioned Reviel Netz, in his book *The Shaping of Deduction in Greek Mathematics* (Netz 2000) advanced the same thesis. Actually, Netz's reconstruction of the missing diagrams (i.e. geometrical
figures composed of lines) in the Greek texts provides deep insights into Greek mathematics that I shall use to argue that the deductive way of thinking possesses the characterizing features of SoTs.

Greek mathematical texts consist of lettered diagrams and words. The deployment of these two elements is what makes Greek mathematics a recognisable genre. As we shall see, Netz maintains that, on a logical plane, lettered diagrams and words combine in necessity-preserving ways to form deductive chains and to yield knowledge of general validity: not knowledge of a particular triangle but of all the triangles (Netz 1999, chapters 5 and 7). On a historical plane, while lettered diagrams are the mark of a literate society, deductive proofs are idealised versions of oral arguments, the mark of a polemical society (Netz 1999, chapter 7 especially p. 312). On a cognitive plane, Greek demonstrations are the result of an interplay between visual resources and indices (letters that signify a point by standing next to it) (Netz 1999, p. 47).

The logical, historical and cognitive analysis offered by Netz provides fertile ground for my arguments. Indeed, first of all Netz gives sound reasons for claiming that the diagram is a necessary element in the reading of the text (Netz 1999, pp. 26-27): for example, some assertions of Greek proofs must be deduced from the diagram (Netz 1999, p. 47). This is a point that will be important for my argument that the postulational style can be viewed as a way of doing: as I shall show, assertions are mediated via specific actions.

Furthermore, in the last chapter of The Shaping of Deduction in Greek Mathematics Netz makes another point that, as I shall point out, is germane to the question as to whether the early development of the postulational style was
gradual. For Netz, the practice of the Greek mathematicians would not have come about without a culture of agonistic debate, the existence of an intellectual network, a tradition of oral and literate culture. Finally, it is possible to make it clearer what Hacking meant when he maintained that the SoTs project is a study in 'cognitive history'. As I have said, 'cognitive history' is an expression that Netz used in the introduction to *The Shaping of Deduction in Greek Mathematics*. He reminded the reader that the foundations of the theory according to which some functions of mind are modules, i.e. task specific capacities, were laid by the cognitive scientist Jerry Fodor (Fodor 1983) (Netz 2000, p. 5). For example, according to the latter, syntax is a module: a biological capacity hard-wired in the brain that allow human beings to perform syntactic computation without even relying on other knowledge. However,

central processes such as the fixation of belief are not modular [...] we can never have a neat universal model of such functions as the fixation of belief. [...] For the historian, study starts where universality ends. It is clear why cognitive history is possible. While there are no general, universal rules concerning, for example, reasoning, such rules do exist historically, in specific contexts (Netz 1999, p. 5).

In other words, processes such as vision and language are modular and single modules can be analysed by the methods of cognitive sciences. On the contrary, more complex cognitive processes, such as ways of thinking and doing, cannot be reduced to general rules: instead of a cognitive science we need a 'cognitive
history’ that searches for rules in specific contexts. ‘Cognitive’ because it is a study of the practices of knowledge and the way human cognitive structures come to interact; ‘history’ because the only rules that exist concerning the reasoning are not universal but historical.

Hacking generalized this point by conjecturing that, if the research program of innate cognitive modules will develop, then ‘no single module will correspond to exactly one style of thinking. Each demands many and modules of different types’ (Hacking 2009, p. 39). So, each SoT represents a particular way in which different modules interact with each other. In the case of the deductive thinking,

- diagrams [...] are the Greek mathematical way of tapping human visual cognitive resources. Greek mathematical language is a way of tapping human linguistic resources (Netz 1999, p. 6)

In his book Netz made no mention of Hacking’s concept of SoT, although he did use the term ‘style’ a couple of times (e.g. Netz 1999, p. 9) in its common-sense usage to hint at a Greek ‘style’ of mathematics. Hacking on his part wrote that ‘the styles project does not engage in historical research but it is delighted by new historiographical ideas that it can exploit. Netz provides one such’ (Hacking 2012, p. 5). In Hacking’s mind, just as Leviathan and the Air Pump represents an analysis of the emergence of the Laboratory SoT, so The Shaping of Deduction in Greek Mathematics represents a history of emergence of the postulational SoT (Hacking 2012, p. 5). Though, these two books belong to different philosophical perspectives. Indeed, Netz plays with the word ‘shaping’ in the title of his book in order to distance himself from constructivist stances:
This book should not be read as if it were 'The Shapin of Deduction', an attempt to do for mathematics what has been so impressively done for the natural sciences. [...] I do not see 'deduction' as a sociological construct. I see it as an objectively valid form, whose discovery was a positive achievement (Netz 1999, p. 3).

Yet this caveat did not prevent Bruno Latour, who was recommended to read the book by Hacking (Hacking 2009, p. 67), from considering The Shaping as belonging to the field of science studies:

Netz’s book does exactly what he says he does not want to do: it offers for the origin of formalism what Shapin and Schaffer have done for the origin of experimental science (Latour 2008, 442).

On the other hand, Hacking did not share Shapin and Schaffer’s constructivism:

In the case of Netz, my reading is closer to the author’s intentions than is Latour’s. In the case of Shapin and Schaffer, Latour’s reading is certainly closer to the intentions of the authors than mine. Latour very much plays down the aspect of Netz indicated in the subtitle, cognitive history [...] For me, it is also the first account of the discovery of a fundamental human cognitive capacity, the ability to make demonstrative proofs (Hacking 2009, p. 71).
In the following lines I shall show that Netz’s book can be interpreted as a study on the emergence and development of what Hacking has called the postulational SoT.

### 3.3.1 Postulational Thinking

Euclid’s *Elements*, which contain the concepts that constitute the foundation of Greek mathematics, were written in Alexandria in about 300 B.C. The treatise consists of thirteen books that collect definitions, axioms, postulates, propositions and mathematical proofs. As T.L. Heath explained, 1) an *axiom* asserts a self-evident truth that is taken for granted; 2) a postulate assumes the truth of a statement without proof but, unlike an axiom, is not self-evident and may assume that something exists; 3) a *proposition* may be a *theorem* or a *problem*. A theorem is a conditional statement; a problem, once solved, *proves* the existence of a geometrical entity (Heath 1908 p. 117-119).

Consider the problem represented by Proposition 1 Book 1:

On a given finite line to construct an equilateral triangle (Heath 1908 p. 241)

For a start Euclid states:

Let $AB$ be the given finite straight line (Heath 1908 p. 241)

Then he provides the directive to construct the equilateral triangle on the straight line $AB$: consider the circle with centre A and radius AB; and the circle with centre
B and radius AB. The circles intersect one another in two points on opposite sides of the line AB. Call C one of them and join with a straight line B and C and with another straight line A and C. AC = AB because they are radii of the same circle with centre A; and BC = AB because they are radii of the same circle with centre B. It follows that AC = BC = AB

Euclid’s proof follows this schema:

(1) AC = AB and (2) BC = AB so (3) AC = BC

The assertions (1) and (2) follow from Definition 15 Book I that defines what a circle is:

A circle is a plane figure contained by one line such that all the straight lines falling upon it from one point among those lying within the figure equal one another (Heath 1908 p. 153).

The implication (3) follows from Axiom 1 Book I according to which things that equal the same thing also equal one another (Heath 1908 p. 155).

Netz maintains that the building-blocks of proof, which he calls the ‘atoms’ of necessity, are the starting points, i.e. all the assertions which are not subject to argument in the text, and the arguments (Netz 1999, p. 168). To think in a postulational way means to combine in necessity-preserving ways these two atoms. For example, the proof above combines a definition and an axiom (starting points) with an argument (e.g. steps (1)-(3) constitute a single argument). We could add another atom of necessity, e.g. that (4) AB > DE, where DE is another straight line and conclude that (5) BC > DE and AC > DE. That is:
(1) \( AC=AB \) and (2) \( BC=BA \) so (3) \( AC=BC \), (4) \( AB>DE \) so (5) \( BC>DE \) and \( AC>DE \)

According to Netz's terminology, (1)-(3) and (4)-(5) are different arguments of the same proof. Step (4) could be a starting point or the conclusion of another argument, which is introduced at the required moment. In this case the proof would involve two arguments ((1)-(3) and (4)-(5) plus three starting points.

Notice that the postulational way of thinking requires not only to do deductions, such as in the steps (3) and (5), but also postulations, such as when the existence of a line AB is postulated. In this sense proofs involve a way of thinking that is both postulational and deductive.

The Greeks' proofs can be much more complicated than my example: they can form long almost linear chains, i.e. chains that include asides and breaks such as when a mathematician sets aside the point and switches to another argument. Furthermore, the Greeks' proofs prove results that are both necessary and general: for example, proposition 1 above is true for all the straight lines AB. Netz maintains that '[necessity is explained] in terms of atomic necessity producing elements, which are then combined in necessity-preserving ways' (Netz 1999, p. 240). On the other hand 'generality is the repeatability of necessity' (Netz 1999, p. 270): generality derives from the possibility of repeating for any other case a proof about specific objects in specific diagrams.
3.3.2 Doing

In order to realise that a proof involves not only a way of thinking but also a way of doing we need to take note of the fact that the diagram is a necessary element in producing and reading a proof. Let us re-examine the proof of Proposition I above. First of all, consider the first opening sentence ‘Let $AB$ be the given finite straight line’. This is not an assertion but a sentence that invites an action: to individuate the letters and the line in the diagram. Consider next the point that two circles with centres A and B meet: this is an implicit assumption based on the diagram (Netz 1999, p. 27). Besides, in the course of the proof, the line $AB$ must be examined within its context: is it the radius of a circle? Does it intersect another line? Is it also the radius of another circle? Thinking and doing, mental and visual resources, interact in order to construct the equilateral triangle: the proof consists of drawing a line and two circles with a ruler and a compass (action); of individuating a point C and drawing the lines AC and BC (action); finally, of deducting (3) from (1) and (2). In Latin, the acronym used to indicate that the task set in propositions such as Proposition 1 Book 1 has been accomplished is Q.E.F., i.e. *Quod Erat Faciendum* - ‘Which Was To Be Done’.

Once the assertion is proved, proposition 1 can be inserted in a chain of deduction so to prove another proposition. To sum up, the text points to objects via letters and leaves to the ‘eyes’ or the visual imagination the task of identifying them; the arguments are mediated through visual actions.
3.3.3 Techniques of self-vindication, new evidence, new objects

Hacking made the point that the postulational way of thinking is self-authenticating because the result of a proof is always confirmed by other proofs:

There is no standard of what is correct proof, than proof itself. Proofs are self-authenticating. Many a proposed proof has proven to be fallacious, but the standard of validity or fallacy is proof itself (Hacking 2009, p. 40).

This is a claim that can be better understood by noticing that the structure of mathematical proofs is completely autonomous: 'the necessity of assertions is either self evident (as in starting-points) or dependent on nothing beyond the immediate background' (Netz 1999, p. 215). The immediate background is constituted, in Hacking’s terminology, by the new objects ‘introduced’ by the postulational way of thinking, e.g. straight points, lines and planes. When doing proofs a mathematician refers visually to these representations, which is to say that the diagram supplies the evidence to which the propositions refer -- the diagram is the standard of evidence within the postulational style. To say that there is no other standard of what is correct proof amounts to saying that there is no way to establish that the diagram is a valuable standard of evidence. That is: the diagram represents the inescapable universe in which knowledge is produced. It is the postulational way of thinking that has ‘introduced’ the diagram and it is the diagram that supplies its universe of discourse.
3.3.4 New Sentences

According to Hacking,

those very sentences used to express the geometrical *a priori*

propositions could not have that sense unless they were embedded in

the practice of geometrical demonstration (Hacking 1983a, p. 457).

Let us consider three examples of sentences:

Straight lines which are parallel to the same straight line and are not in the same plane

with it are also parallel to one another (Heath 1908 Elements IX,9 p. 290).

Similar polygons inscribed in circles are to one another as the squares on the diameters

(Heath 1908 Elements XII,1 p. 369).

Any prism which has a triangular base is divided into three pyramids equal to one

another with triangular bases (Heath 1908 Elements XII,7 p. 394).

For Hacking the sense of these sentences is determined by the way in which we

can reason for their truth or falsehood. To see why consider the first two

propositions listed above. They could not have been thought or understood by,

say, the Babylonian mathematicians, even if a definition of infinite planes, straight

lines and similarity of triangles had been given to them. Indeed, terms such as

‘point’ and ‘straight line’ in those sentences are embedded in ways of doing

unknown to Babylonian mathematicians. The Babylonian and the Greek texts
reveal different ways of doing: the former refer to objects that were actually moved, the latter refer to diagrams; the former are dependent from the context, the latter are abstractions. A Babylonian would have had to learn to act as a Greek and to think independently from the context in order to make sense of those sentences above.

On the other hand, in the last sentence listed above, there are no terms that, once translated, would have had no sense for an Egyptian; but the sentence is not a candidate for truth or falsehood for the presence of the quantifier ‘any’, which gives generality to the proposition. Indeed, ‘Babylonian mathematics is limited, compared to Greek mathematics, by being tied to the particular operation upon the particular case’ (Netz 1999, note 154): the quantifier ‘any’ would have made no sense for a Babylonian because generality was a ‘historical novelty’ (Høyrup 2005, p. 143) brought into being by the Greek mathematical thinking.

Finally, it is worthy of note that in the few lines that precede Hacking’s passage above, Hacking reminds the reader that:

empirical propositions, to be established from case to case by measurement are to be distinguished from a priori and demonstrable propositions of geometry.

In the language of the SoTs project, we can say that my examples of style-dependent propositions above must be distinguished from style-independent propositions such as:

The side of this pyramid is one meter long
Two sides of this trapezoid are parallel

Indeed, for Hacking these sentences above have a truth-value that is independent from any way of thinking and doing.

3.3.5 Discontinuity

As Netz declared, his vision of Greek intellectual life has been shaped by G.E.R. Lloyd (Netz 1999, p. 292). According to the latter, while in Mesopotamia the investigation of the heavens was a matter of state importance (Lloyd 2004, p. 17), in Greece there were no intellectual institutions. Greek society was very argumentative because ‘lacking state institutions that gave stable employment, they [the Greeks] were in open and more or less continuous competition with one another’ (Lloyd 2000, p. 3). As Jean-Pierre Vernant had already stressed ‘speech became the instrument of the city’s political life’ (Vernant 1982 [1962], p. 52). So, argued Netz, the development of rigorous arguments in philosophy and mathematics must be seen against the background of rhetoric: the fact that the latter failed to meet certain standards of incontrovertibility led to a form argument that goes beyond mere persuasion. It was in Greece, thanks to this particular historical setting that proof emerged and came to be considered as the paradigm of what it means to settle an argument. Before the late fifth century there were no conditions for the emergence of proof.

Netz concluded that the origin of Greek mathematics has been a ‘sudden explosion of knowledge’: it is impossible to imagine that the early history of Greek
mathematics was gradual and that there has been a rudimentary form of pre-Euclideanism:

Whoever was capable of proving one Euclidean result was capable of proving most of them. I therefore suggest that the origin of Greek mathematics could have been a sudden explosion of knowledge [...]. No more than a generation would be required to find most of the elementary results of Euclid [...] (Netz 1999, p. 273).

This is exactly Hacking’s point that the discovery of the possibility of demonstrative proof represents a sharp break in the development of mathematical reasoning. To conclude, the Greek style of mathematics possesses all the characterizing properties of SoTs and therefore can be called ‘the postulational SoT’.

3.4 The Historico-genetic Style of Thinking

‘Historico-genetic’ is the locution by which Hacking referred to the last style of thinking in Crombie’s list. The few comments he made on Crombie’s account of the historico-genetic style are insufficient to reconstruct the physiognomy of this SoT. First of all, we gather that while the postulational SoT systematically employs deductive logic, ‘[the historico-genetic SoT] employs one branch of logic, namely abduction or inference to the best explanation’ (Hacking 2012, p. 5). Hacking’s description of the historico-genetic thinking can be summarized by these steps: 1)
assuming that things stand in a certain way in some domain of interest; 2) devising an explanation of how things got to be that way; 3) inferring from the power of the explanation, and its superiority to any other available candidate, that it tells how things are (Hacking 2012, p. 5). ‘Darwin’s long argument’, mentioned by Hacking in passing, can be taken as an example of historico-genetic thinking. Indeed, the explanation of adaption of living beings is considered the primary fact to be explained by the theory of evolution (Gould 2002, p. 119) (step 1). The ‘long argument’, as Darwin calls his argument in The Origin of Species (1985 [1859]), provided an explanation of this problem (step 2); William Paley (1743-1805) had proposed a different explanation according to which adaptation must have been ‘designed’ by God. As there are several facts that are not explained by Paley’s argument (see Darwin 1985 [1859], chapter XIV) but are explained by the ‘long argument’, one can conclude that Darwin proposed the best explanation (step 3). In the next subsection I shall provide a more detailed characterization of the historico-genetic thinking.

Hacking has not been very clear as to when the historico-genetic SoT emerged. Indeed, although he pointed out that ‘[the historico-genetic] genre of reasoning began long before what we call the sciences’ (Hacking 2012, p. 5), he also noted:

Probability crystallizes in the time of Pascal. Then there is historico-genetic explanation, whose triumph is Darwin’s explanation of the species by natural selection (Hacking 2012, p. 5).

In this passage he seems to suggest that Darwin’s time represents the point in
which the historico-genetic explanation became the standard method of scientific
disciplines such as the life sciences. For my part, I shall argue that the historico-
genetic SoT, a way of tapping abduction, a human form of reasoning, emerged in
the eighteenth century.

3.4.1 Historico-genetic Thinking versus Laboratory Thinking

After this brief presentation of Hacking's claims, I am going to better characterize
the historico-genetic thinking. The best way to do this is to compare and contrast it
with laboratory thinking.  

In his book What Makes Biology Unique? (Mayr 2004) Ernst Mayr explains that
'biology consists of rather two different fields, mechanistic functional biology and
historical biology' (Mayr 2004, p. 24). The former deals with the physiology of
living organisms '[which] can be explained mechanistically by chemistry and
physics' and does not need any historical analysis. The latter deals with 'all
aspects of the living world that involve the dimension of historical time' (Mayr
2004, p. 24). Whereas in functional biology the most frequently asked question is
'how?', in historical biology scientists ask 'why?' most of the time (Mayr 2004, p.
25).

This distinction can be extended to disciplines such as geology, cosmology
and palaeontology: these disciplines are 'historical', although they can ask 'how-
questions' like in chemistry and physics. For instance, geology has provided
insights into the structure of the Earth by applying physics methods to the study

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18 In my analysis I shall only deal with the general features of the historical and the experimental
sciences. For a review of the methods deployed by Darwin see (Gould 2002, pp. 103-116). A
compendium of the methods of geology can be found in (Laudan 1987, pp. 7-16).
of seismic waves, i.e. by asking questions about how waves propagate ('how-questions'); on the other hand, thanks to Alfred Wegener (1880-1930), geology has offered a historical explanation of continental drift, explaining a large number of features of the surface of the Earth, e.g. why continents’ shapes fit together like a jigsaw ('why-questions').

In subsection 2.5.2, I have explained that one of the cognitive steps involved in laboratory thinking consists of establishing what is a matter of fact and what is a speculative hypothesis, which theory has to be embraced and which has to be rejected. Laboratory sciences postulate statistical or causal regularities among types of events; from the laws supposed to describe these regularities, predictions are inferred and tested in laboratory; and, importantly, often in the face of a failed prediction ad hoc hypotheses are added and separately tested in order to protect the hypothesized laws from false negatives (see also Cleland 2002, pp. 476-480). Furthermore, the essence of explanations in physics is to show that a phenomenon ‘has to be so’ given certain physical laws and a few additional facts. This is what Isaac Newton (1642-1727) did when he proved that Kepler’s three laws of planetary motion could be deduced from his laws of motion and universal gravitation.

Conversely, as Mayr notes, laws have a non-essential role in historical biology (Mayr 2004, p. 27) given the contingent character of many phenomena. In biology a result R we expect could fail to come about simply because our hypothesis K admits exceptions. Indeed, what really characterizes historical sciences is the postulation of a common cause for explaining different puzzling effects of past events. Often this common cause must be connected with its effects
through a long chain of events, some of which are contingent: for example, on
Gould's model of history of life the Cambrian explosion produced a wide range of
phyla, of which many disappeared rapidly; for him, it was a matter of luck, and
not of being 'fitter', if a worm-like organism called Pikaia gracilens, from which we
probably descend, did not go extinct (Gould 2000b). This example shows the
absence of regularities in biology: according to Gould, if one could somehow turn
the clock back and allow life to evolve again, very likely our species would not
exist. It is for this reason that *historical explanations* have the character of a story
that cannot be tested in a laboratory: there is no way to test experimentally a thesis
about human origins.

Historical scientists try to find 'the best explanation', i.e. the particular story
that unifies under a single explanation the largest number of facts to be explained.
As often many explanations compete, scientists must hunt for what Carol Cleland
called a 'smoking gun' (Cleland 2002, p. 480), namely a trace or a body of traces
that establish whether an explanation is more unifying than another. The search
for a smoking gun plays a key role in historico-genetic research, as Cleland
explained. A case in point is the hunting for a smoking gun that discriminates
between the different explanations of the extinction of the dinosaurs. According to
some of the first explanatory stories offered before the 1980s, dinosaurs went
extinct about 65 millions of years ago because of the lack of immunity to a very
infectious disease or because of a rapid change of climate. Then, following the
discovery of unusual high concentrations of iridium in a layer of Earth's crust in
1980 (iridium is rare on our planet and abundant in meteorites) another
explanation gained support: an asteroid impact determined a mass extinction.
Finally, during the late 1980s, geologists found the Chicxulub Crater underneath the Yucatán peninsula: the smoking gun, as it were.

It is a fact that since the beginnings of the modern historical sciences there has always been a clear awareness of the distinction between how-questions and why-questions. So much so that when M.J.S. Rudwick outlined the historical setting that lies behind the Great Devonian controversy (about the dating of certain puzzling rock strata) in the 1830s, he wrote:

Causal analysis was central to dynamical geology, whereas for stratigraphical geology it was peripheral [...] Most geologists in the 1830s were not centrally concerned with causal questions at all [...] The problems of geological dynamics were perceived as neither more nor less fundamental than those of stratigraphical geology: they were just different [...] Geologists [...] treated them as essentially separate realms (Rudwick 1985, p. 45).

In sum, my points so far provide an illustration, although idealized, of two distinct ways of acquiring knowledge: laboratory scientists infer predictions from laws, test them and control auxiliary hypotheses; historico-genetic scientists suggest explanations of puzzling traces of the past searching for smoking guns that rule out alternative explanations. We can say that the historico-genetic way of thinking is well portrayed by the following three mental operations. The first one consists of focusing on the historical aspect of the world. It is a change of perspective that is well described by Darwin:
When we regard every production of nature as one which has had a history; [...] when we thus view each organic being, how far more interesting, I speak from experience, will the study of natural history become! (Darwin 1985 [1859], p. 456)

Darwin’s perspective is radically different from that in which the world is investigated as it is: compare a question about the orbit of a planet with that concerning its history from its formation to its orbital capture. The second mental operation consists of postulating causal histories to explain the effect of long-past events. Finally, the third mental operation is the search for the smoking gun, the evidence that discriminates between two or more historical explanations.

3.4.2 Doing

It must be added that, by hunting high and low for the traces of past-events, historical scientists do not only support their conjectures or discriminate between conflicting explanations. They also build their stories just as if they pieced together a jigsaw: sometimes they have no clear vision of how the puzzle image will look like; their final product is often unexpected and results from an interchange of fieldwork and theoretical research. A case in point is the reconstruction of the evolutionary history between Homo sapiens and the ancestral species we share with chimpanzees and bonobos. The hypothesis of a new species, now gone extinct, Homo rudolfensis, was put forward in the 1970s on the basis of a collection of early Homo fossils from Koobi Fora (Kenya). However, this hypothesis remained controversial because of the incomplete preservation of the remains. Only with the discovery of other fossils in 2012 by the paleoanthropologist Meave Leakey did it
gain support (Leakey et al. 2012). Another example comes from geology: the Scottish geologist James Hutton (1726-1797), searching for a proof of the idea that internal and external forces of Earth mould rocks and mountains, went by boat to Siccar Point, a Scottish promontory in which he found overlapping strata of different geological eras. That was the evidence that the Earth was considerably older than previously thought and the beginning of modern geology. As a final example, by removing ice cores from the polar ice, climate scientists have been able to examine the air trapped inside. The data revealed information about the atmosphere over the millennia and helped the reconstruction of the history of the climate of our planet.

Fossil hunting, geological digging, ice cores removing represent examples of fieldwork in historical sciences. M.J.S. Rudwick pointed out that, in early nineteenth-century, working on foot, wearing outdoor clothing, being frequently soaked to the skin was not thought unusual by the geologists. This was the indispensable part of their activity that Deborah Cadbury has recently recreated in her scientific novel *The Dinosaurs Hunters*, a story of the rivalry between the palaeontologists Gideon Mantell (1790-1852) and Richard Owen (1804-1892). A hammer for collecting samples of rock and finding fossils, a magnetic compass for measuring the tilt of strata and other few items were ‘all that the most eminent geologist required in the field’ (Rudwick 1985, p. 40). Fieldwork was an activity fundamentally distinct from the activity in laboratory. As Rudwick put it

[the] major issues in early nineteenth-century geology,

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19 More details on this example can be found in (Eldredge 1998, chapter II)
were argued over in such public arenas as the Geological Society and the British Association. But all parties [...] acknowledged publicly that fieldwork was the primary locus of encounter between the geologist and the phenomena of his science (Rudwick 1985, p. 37).

Fieldwork marks the way of doing that is peculiar to historico-genetic sciences: without this activity no stories can be told about the portion of world under study. The primacy of fieldwork in the historical sciences parallels the primacy of laboratory in the experimental sciences or the primacy of the use of diagrams in geometry: if a practitioner of geology did not have acted ‘in the field’ she would not have been accepted as part of the community of the earliest members of the first geological societies.

3.4.3 Discontinuity

The historian of science Paolo Rossi wrote that

In the one hundred of years that separate in time the Discourse on Earthquakes (1668) of Hooke from the Universal Natural History and Theory of Heaven (1755) of Kant, the discussions about the history of the Earth and of cosmos radically change[d] (Rossi 2000, p. 256 my translation).

In the seventeenth century the minds of the natural philosophers were still impregnated with the origin myths of Genesis and the alleged brief history of our
planet. A century later the dichotomy between a prescriptive science of the unchangeable laws of the universe and a descriptive science of the evolution of phenomena had already emerged. Historical sciences such as cosmology, biology, palaeontology and geology constituted themselves around a set of practices that shared the historico-genetic way of thinking. As Antonello La Vergata pointed out, ‘only in the second half of the eighteenth century did the geological practice consolidate and did a consensus about terms, concepts and data emerge’ (La Vergata 1988, p. 165 my translation). The first geological societies were founded at the beginning of the nineteenth century: in the case of the Geological Society, ‘[its] membership more than doubled between 1825 and 1845’ (Rudwick 1985, p. 22).

When the legitimacy of a ‘historical’ consideration about nature was established, several alternative theories as well as various models of history of Earth and universe emerged.

The more [...] naturalists looked at the earth’s structure, the more complex it seemed [...]. Soon theories based on extensive transformations of the earth’s surface became to appear [...] [and] geology and palaeontology would eventually emerge (Bowler 2003 p. 28).

On the basis of these considerations, it is possible to say that the emergence of the historico-genetic way of thinking and doing I have described so far was not gradual. In their book Cartographies of Time (Rosenberg and Grafton 2010) the historians Daniel Rosenberg and Anthony Grafton looked into how humans have drawn time in the course of history. I think that their conclusions corroborate my
Today the timeline with a single axis is a ubiquitous figure in the representation of time and one of the most inescapable metaphors for representing history by a drawing. Yet, Rosenberg and Grafton showed that the timeline is not even 250 years old. It is difficult to imagine a sort of rudimentary idea of the timeline that grows by micro-accumulation as if the first forms of such line had continuously developed into our timeline. Rosenberg and Grafton’s account of ‘the birth of modern historical thinking traces a path from the enumerated but not yet narrated medieval date lists called annals, through the narrated but not narrative accounts called chronicles to fully narrative forms of historiography that emerge with modernity itself’ (Rosenberg and Grafton 2010, p. 11). In their research they have not observed such a micro-accumulation. Until the mid-eighteenth century, what Rosenberg and Grafton call the Eusebian model - a simple matrix with kingdoms listed across the top of the page and years listed down the left or right hand columns - was dominant. Only in the middle of the eighteenth century did scholars start to communicate the idea of historical development. For Rosenberg and Grafton, the crucial event was the publication in 1765 of the Chart of Biography and in 1769 The New Chart of History by the scientist Joseph Priestley (1733-1804). The reader could carry her eye vertically and see the contemporary state of all the empires subsisting in the world, at any particular time; or, she could carry her eye horizontally and note empires rise, flourishing and decline as time goes by.

Priestley’s chart [...] provided an intuitive visual analogue for concepts of historical progress that were becoming popular [...]. In Priestley chart, historical thought and new forms of graphic expression came
Priestley's charts became an essential part of a gentleman's library: for example Rosenberg and Grafton say that the novelist Maria Edgeworth (1768-1849) and the physician Erasmus Darwin (1731-1802) recommended the new ways of visualizing history as aids in the education of children. In his book *The Life and the Opinions of Tristram Shandy, Gentleman*, the novelist Laurence Sterne (1713-1768) drew lines to represent the narrative patterns and the digressions from the main story. In conclusion, just when the historico-genetic thinking became central in the history of thought the timeline emerged. Its sudden coming to the fore suggests that the historico-genetic way of thinking and doing, with the features I am going to describe, had sharp beginnings.

### 3.4.4 A New Kind of Evidence

Thinking in the historico-genetic way involved a 'new kind of evidence'. This expression must be understood in two senses, as I am going to explain. First of all, let us compare and contrast how the experimental and the historical sciences provide pieces of evidence and how they use them. Experimental scientists test a target hypothesis not so much in order to reject it on the basis of a failed prediction (as Popper thought) but to save it from misleading confirmations. For example, the astronomers Urbain Le Verrier (1811-1877) and John Couch Adams (1819-1892) 'saved' the gravitational law. When it was found that the orbit of Uranus differed from that predicted by the gravitational law, they suggested the existence of another planet (Neptune) whose position was theoretically calculated
and confirmed by telescopic observation. Two points must be noted: firstly, experimental sciences' use of evidence is limited to a single hypothesis (Cleland 2001, p. 988); secondly, the analysis of the hypothesis is often carried out by man-made machines such as Boyle's air pump that bring about the test conditions specified by the hypothesis.

Conversely, as Cleland pointed out, in the historical sciences most of times evidence plays a different role since it is used to discriminate multiple competing hypotheses about past events (Cleland 2001, p. 989). The new kind of evidence that comes about with the emergence of the historical thinking is the 'smoking gun'. The 'smoking gun' is a kind of evidence entirely distinct from the 'man-made evidence' of the laboratory SoT: the former discriminates between different historical accounts, the latter concerns a single hypothesis that must be falsified or 'saved' and it is, most of the time, elicited by apparatuses. For example, the Chicxulub Crater represents the evidence that an asteroid impact determined a mass extinction. So, it discriminates between different historical accounts of the evolution of dinosaurs. On the other hand the death of a bird in the air-pump is a piece of evidence produced by a man-made machine in order to test the single hypothesis of the existence of void.

I am not claiming that there are no examples in which historical scientists use apparatuses in order to sharp a particular trace or to reveal a smoking gun. Nor am I claiming that 'the smoking gun' is a kind of evidence that was not in use before the emergence of the historical sciences. My point is that when the Geological Society invited its members to display on a table set the relevant specimens after a paper was read (Rudwick 1985, p. 23), those pieces of evidence
were being used in a way of which the entire community had become aware – they were used to discriminate between different historical accounts. The existence of a ‘smoking gun’ had become the new standard of truth within a new way of thinking and doing.

There is a more obvious sense by which it is possible to say that a new kind of evidence emerged: historico-genetic scientists turned their eyes to a previously unnoticed portion of the world, to objects such as fossils, minerals, rocks and geological layers. Before the emergence of the historico-genetic way of thinking this portion of the world had no role as evidence. In his book *I segni del tempo* (Rossi, 2002 [1979], p. 21-45), Paolo Rossi explained that, until the seventeenth century and with the exception of Leonardo Da Vinci (1452-1519) and Bernard Palissy (1510-1590), Platonic and Aristotelian interpretations of the origins of fossils were prevalent: Platonic thinkers thought that fossils grew inside the earth like organisms thanks to particular virtues (*virtus plastica, lapidifica, vegetabilis*); Aristotelian thinkers ascribed the existence of fossils to the action of solar heat and winds that blew in the bowels of the earth. Only when fossils were thought of in terms of their origin could thinkers interpret those puzzling objects as remains of old organisms: nature became the reign of history, of which fossils represented the documents (Rossi 2000, p. 258). Consequently, the emergence of the historical vision of nature is closely tied to the coming to the fore of a new portion of the world, which for the first time came to play the role of evidence.

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20 An account of Leonardo’s theories about fossils can be found in (Gould 1998)
3.4.5 Techniques of Self-Vindication

The evidence on which historico-genetic scientists rely in order to infer the best explanation may not be determined independently from the historico-genetic way of thinking itself. For example, the evidence for the extinction of the dinosaurs is provided by the claim that the Chicxulub Crater was caused by an asteroid-impact dating from the end of the Cretaceous period, when the dinosaurs went extinct. In turn, this claim is an inference to the best explanation of different traces of past events such as: clay with quantities of iridium much higher than normal and concentration of iridium in asteroids. In other words, the very evidence that supports the historical account of how dinosaurs went extinct is still another historical account obtained by using the historico-genetic way of thinking and doing.

Consider these questions and answers: why is the historico-genetic way of thinking and doing reliable? The reason is that it provides the best explanation. And why is what it provides the best explanation? The reason is that there is a smoking gun. And why is just that a smoking gun? Because it follows from an inference to the best explanation obtained by using the historico-genetic way of thinking. These questions and answers form a *circulus in probando*: the historico-genetic way of thinking is self-authenticating.

This point can be viewed from a different perspective:

- science advances our understanding of nature by showing us how to derive descriptions of many phenomena using the same patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as
That is: humans apply the abductive method again and again in order to build up a unifying picture of all the disparate phenomena. For example, the extinction of dinosaurs and the crater in the Yucatan peninsula were ‘brute facts’ until they were put in relation by applying twice the inference to the best explanation: an inference to the claim that the crater was caused by an asteroid-impact and an inference to the hypothesis of the extinction of the dinosaurs. By doing so those two brute facts are fitted together. In this sense to be self-authenticating for the historico-genetic thinking corresponds to the fact that we apply again and again the abductive method in order to provide a coherent historical account of our world; and we cannot ‘escape’ from abduction.

3.4.6 New Sentences

In their book Rosenberg and Grafton reproduce a section of a famous mediaeval manuscript called the *Annals of St. Gall*, which records events in the Frank kingdoms during the eight, ninth and tenth century:

710. Hard year and deficient in crops.
711.
712. Flood everywhere
713.
714. Pippin, mayor of the palace died
715.
716.

717.

718. Charles devastated the Saxon with great destruction

719.

720. Charles fought against the Saxons

721. Theudo drove the Saracens out of Aquitaine.

722. Great crops

Rosenberg and Grafton say that although ‘these annals breathe with the life of the Middle Ages’ they appear ‘strange and antic to a modern eye, beginning and ending seemingly without reason, mashing up categories helter-skelter like the famous Chinese encyclopaedia conjured by Borges’ (Rosenberg and Grafton 2010, p. 11). Indeed, the *Annals of St. Gall*

make no distinction between natural occurrences and human acts; they
give no indication of cause and effect; no entry is given more priority
than another. [...] there is no distinction among periods, and lists begin
and end as nameless chroniclers pick up and put down their pens.
(Rosenberg and Grafton 2010, p. 12).

Let us focus on a single sentence of *Annals of St. Gall*:

712. Flood everywhere

There is no history behind that sentence, no real sense of a cause such as an alleged cyclone that brought about the flood. The sentence could be contrasted
with sentences such as:

The flood was a consequence of a warming of ocean waters and the formation of a cyclone that rotated counterclockwise and reached France in 712.

The meaning of the latter sentence hinges on scientific practices unknown in 712: no sense can be attributed to it until one comes to consider floods as having a history and grasps how different explanations are selected and how and where the evidence can be found and used. What is necessary to make sense of the latter sentence is a way of thinking and doing that was absent during those centuries. The same arguments could be applied to sentences such as:

Modifications to the lower parts of the limbs of the horse shows that evolution proceeds in the direction of increasing specialization.

The order of formation of rocks can be read from the observed sequence.

For us these sentences could be true or false, but in the ancient times no truth-value could be attributed to them. Myriads of sentences such as these two above, i.e. sentences that are candidates for truth or falsehood only within the historico-genetic way of thinking, have appeared after the eighteenth century.

To conclude, although humans have always used the abductive method, it was only in the seventeenth century that a way of thinking with all the features described in this section emerged. These features characterize this way of thinking as a SoT, 'the historico-genetic SoT'.
3.5 The Taxonomic Style of Thinking and Doing

By ‘taxonomy’ it is generally meant a branch of science concerned with the classification of organisms. As Ernst W. Mayr and Walter J. Bock explained, classifications are attempts to arrange a diversity of entities into sets of classes according to a criterion of similarity (Mayr and Bock 2002, p. 170): each class is constituted by individual entities that are considered similar. Classifications are a subset of ‘ordering systems’, a term that alludes to any attempt to arrange objects into particular categorizations. For example, the ‘cladistic method’, introduced by Willi Hennig (1913-1976) in the late twentieth century, is not considered a classification but an ordering system in that it groups species not according to their morphological similarity but according to their degree of evolutionary relatedness. ‘The entities ordered in a cladification are not classes, but clades’ (Mayr and Bock 2002, p. 182), which may be so heterogeneous to include, e.g., mammals and primitive reptiles.

Rather than depicting our ancestral ability to perform the dullest forms of cataloguing, I want to characterize the way scientists think when they classify the living world. Hacking explicitly said that Crombie’s taxonomic style has to be counted as a SoT, but he never provided his own characterization. However, it is possible to say that he pointed out the possibilities that this way of thinking opens up for human action and described the way it affects the people classified (e.g. see Hacking 2002, chapter six) (Hacking 1999b, chapters four and five). For example, Hacking reflected on the fact that, although certain perversions may have always existed, the ‘pervert, as a diseased person, [was] created in the late nineteenth
century’ by classifications based on a new functional understanding of disease (Hacking 2002, p. 100)(Hacking 1999). This is only one of the ways explored by Hacking in which human beings and their perceptions of themselves are affected by our classifications. The way of framing these issues, which are beyond the purpose of this work, was influenced by Foucault, as Hacking acknowledged (Hacking 2002, p. 100).

Another fact to be noted is that Hacking has been attracted by a thesis put forward by the American anthropologist Scott Atran in the book *Cognitive Foundations of Natural History* (Atran 1990). The latter argued that there are universal principles of biological classification and nomenclature employed by peoples everywhere. In particular, Atran maintained that the beginnings of modern biosystematics are to be traced to folk classifications of living beings: the classification of plants and animals made by Aristotle exhibit the same constraints as the classification of other coeval peoples in different ecosystems.

According to Atran plants and animals may change from a geographical region to another but every people recognizes the same classes in its environment and eventually comes to use the same taxonomic structure. For example, peoples who live in two different ecosystems will give to the same plant or to two distinct varieties of the same plant the same place in their classification21.

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21 There is no consensus about Atran’s theses. Developmental psychology does not offer much support to them: for example, Spelke maintains that young infants appear to have initial domain-specific knowledge only in four domains: physics, psychology, number and geometry (Spelke 1994, p. 433) (see also Carey 1985). Lloyd pointed out some difficulties with Atran’s studies (Lloyd 2007, p. 47-53). Also, by comparing the taxonomies of animals in ancient China with Aristotle’s, Lloyd argued that there is no universal common sense by which to compare and contrast the solutions to some questions about the classification of animals (Lloyd 2007, p. 54) (Lloyd 2004).
What attracted Hacking in Atran's work was the fact that it provides a basis for claiming that the taxonomic SoT is grounded in the humans' innate dispositions: in fact Atran proposed the existence of a module for the naïve classification of living beings. However, for Hacking, Atran's theses only represent the first step towards an understanding of the taxonomic SoT. What he would like to see is a theory of the contingent social and institutional conditions that have made this SoT possible (Hacking 1991b, p. 18).

3.5.1 Taxonomic Thinking

It is a fact that in the extant literature before Aristotle there is no comprehensive explicit classification of animals and plants. Aristotle introduced a major distinction between blooded and bloodless animals that roughly corresponds to our division between vertebrates and invertebrates. He subdivided the former class into live-bearing and egg-laying animals (and a third categories of animals that share properties with both groups); then, he subdivided the latter into four groups. Aristotle's aim was not to carry out a taxonomic project in the real sense of the word (Lovejoy 1960 [1936], p. 56) (Louis 1986, p. 7-8) (Gohau and Duris 1997, p. 8): he did not invoke a single principle to classify animals and switched from anatomical to physiological or behavioural criteria (see Lloyd 1961). In spite of that, it was Aristotle who suggested to later thinkers the idea of a graded scala naturae in which the animals are arranged according to their degree of 'perfection' (see Lovejoy 1960 [1936]) (Bynum 1975). This suggestion was based on a criterion of rank: the grade of development reached by the offspring. On the basis of this criterion he conceived eleven grades, with man, the ens perfectissimum at the top.
and at the bottom the zoophytes, those animals that look like plants. For example, ‘bloodless’ animals were ranked below animals that possessed blood; animals such as birds were ranked over fish; and animals were ranked over plants for their ability to move and sense.

Grouping (forming taxa) and ranking (placement of those taxa in a hierarchy) represent two distinct mental actions that can be recognized in Aristotle’s way of thinking about the natural sciences (Winther 2012, p. 634) (Stevens 1994, p. 10). For example, when Aristotle divided animals into two classes by using as a criterion the presence of blood in the body he was grouping, an action that remains at an organizational level and is distinct from that of ranking. On the other hand, when he suggested a criterion of rank to arrange all the classes in a single ladder he was ranking.

The taxonomist Graham Griffiths distinguished ‘classification’ from ‘systematization’: to classify means to order into classes, which in turn are constructed from similarities; to systematize means to arrange classes according to certain relations (Griffiths 1974, p. 85) such as developmental or philogenetic. Griffiths’ concept of systematization implies something more than just ranking: to put forward an explanation in order to interpret the ranking (Winther 2012, p. 7). Systematizing makes the work of a taxonomist distinct from that of stamp-collector. To make the concept clearer let us come back to Aristotle. When he applied the categories ‘high’ and ‘low’ throughout the animal kingdom he just ranked different classes of living beings, e.g. according to his classification dogs are more advanced creatures than fishes, and fishes are more advanced than any invertebrate. This mental action is to be considered distinct from that of
systematizing, i.e. interpreting the classification. For example, Aristotle claimed that the latter shows the static plan of nature: each species has always existed and will never become extinct. This was a systematization of the classification.

Later on, from the Middle Ages until the late eighteenth century, the result of Aristotle's interpretation was a conception of the universe as a 'Great Chain of Being', which was made, for the principle of continuity, of an infinite number of links in hierarchical order. The naturalist Charles Bonnet (1720-1793) took seriously this classification and put forward an anti-evolutionist systematization, i.e. an explanation according to which 'each species was an eternal element in the divine plan' (Bowler 2003 p. 64). In fact, Bonnet provided a causal explanation to interpret a classification whose basic principles had been introduced by Aristotle. He did not group, nor did he ranked, he just systematized, i.e. he provided an explanation of the classification by denying evolution. On the other hand Lamarck reinterpreted the chain of being by 'temporalizing' it, to use Lovejoy's term, i.e. by abandoning the fixity of species and proposing the theory of a linear evolution.

To suggest a causal explanation of the classification is often an action strongly interconnected with those of ranking and systematizing.

Whether with Linnaeus we attend to form [...] [or] function, whether [...] we arrange the taxa by counting similarities, or whether we construct a taxonomy that is homomorphic to an evolutionary tree, we are in every case invoking a conception of living beings (Hacking 1993a, p. 288).

Yet my point is that it is theoretically possible to disentangle these three mental
actions, which are peculiar to the taxonomic thinking.

When from Aristotle we turn to the modern taxonomists, the mental actions of grouping, ranking and systematizing are even more clearly distinguishable. In order to group plants, the naturalist John Ray (1627-1705) focused on the distinguishing features that perpetuate themselves in propagation from seed and established as a principle that ‘a species never springs from the seed of another’ (quoted in Gribbin and Gribbin 2008, p. 24); in so doing he provided a first version of the definition of species. By taking into account every visible character of every species Ray tried to introduce a ranking: he grouped species infimae (our species) into species subalternae (our genera) and the latter into genera (our orders). Eventually he concluded that there is no way of knowing which is the most relevant character for a classification. Carl Linnaeus (1707-1778) opposed Ray’s conclusion and focused on the reproductive organs of plants. He determined classes and orders by counting stamens and pistils of every flower; then he determined the species by a closer inspection of the structures of these plants. By doing so he introduced taxonomic ranks into the living world: species were grouped into a higher category called genus and the genera were grouped into classes of more basic resemblances. Linnaeus thought that his scheme of mapping biological relationships as smaller classes within successive large classes mirrored the God’s permanent order, which ought to be discernible by the human mind. This can be viewed as a systematization, that is an interpretation of his classification, but also as an assumption that guided his research of a comprehensive classification of the living world – at any rate, Linnaeus’ creationist account is to be distinguished from his mental actions of grouping and ranking.
The interpretation in creationist terms given by Linnaeus was not the only possible. Linnaeus’ classification is open to another systematization in terms of evolutionary theory, as Stephen Jay Gould explained: ‘Linnaeus’s scheme happens to be compatible with the interpretation that the interrelationships among organisms derive from a genealogical hierarchy built by evolutionary branching (Gould 2000a, p. 22). Ultimately, like Aristotle, Linnaeus carried out three independent mental actions: grouping, ranking and systematizing. His systematization was only one of the possible ones.

To sum up, the way of thinking of taxonomists from Aristotle to the modern taxonomists is characterized by three mental actions: grouping, ranking and systematizing. Grouping and ranking require criteria such as those based on similarities in Aristotle’s and Linnaeus’ classifications. Systematizing consists of a causal explanation to interpret the classification: for example, the fixity of species for the great chain of being or the evolutionary theory for the Linnean classification.

Another point to note is that taxonomists themselves consider their methods as ways of finding out. This is evident not only in Aristotle, for example, who wanted to explain the nature of kinds by identifying their place in Nature (Hacking 1991b, p. 18), but also in the thought of later students of classification. In the mid-seventeenth, ‘to name means to know’ was the idée fixe of taxonomists (Rossi 2000, p. 280): for Linnaeus denominatio was one of the aims of botanic and classifying constituted a way for finding out the structure of the divine thought. In the mid-eighteenth century the idea that a classification should mirror the principle of common descent caught on. Classification helped to find out about the
history of life: for example, to distinguish, within vertebrates, morphological types such as fishes, amphibians, reptiles, birds and mammals suggested that each of them descended from a common ancestor which possessed the same body plan.

3.5.2 Emergence and Discontinuity

For Hacking Linnaeus represents the point in time in which the taxonomic thinking suddenly came to dominate the scientific milieu. Perhaps it is not by coincidence that a similar thesis can be drawn from Foucault’s *The Order of Things*. In *The Order of Things* (Foucault 1994 [1966]) Foucault told of three major epochs in Western thought: the Renaissance, the Classical Age (from 1650 to 1800), and Modernity. Each epoch was marked by an epistemic system, i.e. by particular conditions of possibility of knowledge and its discourse that Foucault tried to describe. In particular, until the mid-seventeenth century ‘the historian’s task was to establish the great compilation of documents and signs – of everything, throughout the world, that might form a mark’ (Foucault 1994 [1966], p. 130). It was only in the Classical Age that ‘the sciences carr[ied] within themselves the project […] of an exhaustive ordering of the world’ (Foucault 1994 [1966], p. 74). This entirely new way of connecting things is for Foucault ‘the hidden network’ (Foucault 1994 [1966], p. XX) that determines, *inter alia*, how we deal with the things classified. For Foucault, the *épistème* of the Renaissance is the period in which ‘the naturalist [becomes] the man concerned with the structure of the visible world and its denomination according to characters’ (Foucault 1994 [1966], p. 161). As I am going to show, Foucault’s account not only supports the claim that the taxonomic style of thinking crystallized in Linnaeus’ time but also that it
suddenly opened up a conceptual space for new candidates for truth and falsehood.

To start with, as Lloyd noted, no thinker in pagan Greek antiquity attempted a zoological classification on the scale of Aristotle (Lloyd 2007, p. 54). Theophrastus, Aristotle’s disciple, focused only on plants and rocks and Roman naturalists such as Pliny the Elder (23-79 AD) did not really add anything scientifically important to Aristotle’s achievements (Gohau and Duris 1997, p. 9). The important naturalistic works of Albertus Magnus (1193/1206-1280) faded soon into oblivion and in the Renaissance animals were not studied as such but for the signs that could be read in them (Foucault 1994 [1966], chapter 5). Furthermore, no new systematizations were proposed until the end of the seventeenth century when the idea of chain of being finally lost support.

It is a fact that, between the mid-sixteenth and the mid-seventeenth century the amount of data increased exponentially: as Paolo Rossi reports, in the *Herbarum Vivae Eicones*, written by the botanist Otto Brunfels (1439-1534) and illustrated by Hans Weiditz (1495-1537), we find a list of 258 species of plants. Less than one hundred of years later, in 1623, the naturalist Gaspar Bahuin, in the *Pinax theatre botanici*, listed six thousands of species. In 1686, Ray published the first volume of his *Historia plantarum* in which he described 18,000 species (Rossi, 2000).

An interesting point made by Atran is that Aristotle dealt with a local fauna of just six hundred or so local species, a number that does not strain our innate capacities to sort; by the time of Linnaeus and Georges Cuvier (1769-1832) the complexity of the known world of nature increased so much that our built-in capacity to sort plants became insufficient. Linnaeus is therefore the point of
history - argues Atran - in which a 'scientific breakaway' took place, i.e. a sort of gradual disengagement of science from common sense. What triggered this explosion of discoveries? Why did people become obsessed with classification?

Foucault's analysis suggests the following answer to these questions: for the taxonomic thinking to crystallize in Linnaeus' time it was necessary for the students of 'History' (Foucault's terminology) to become observers of the natural world. This has not always been the case:

 [...] though it is true that the historian, for the Greeks, was indeed the individual who sees and who recounts from the starting-point of his sight, it has not been so in our culture. Indeed, it was at relatively late date, on the threshold of the Classical age, that he assumed – or resumed – this role (Foucault 1994 [1966], p. 130).

The year 1657, date in which the naturalist John Jonston (1603-1675) published a Natural history of quadrupeds, is considered by Foucault the threshold of the Classical Age. For him, until that moment all that existed was just histories: a History of the nature of birds by Pierre Belon (1517-1564) an Admirable history of plants by Claude Duret (1570-1611), a History of serpents and dragons by Ulisse Aldrovandi (1522-1605). In their works, these authors gave room to allegoric interpretations, myths and legends about a given animal or a plant. Their classifications included mythological animals, literary considerations, possible uses of a certain animal and its edibility. Rather than coming from a direct observation of nature, Renaissance scholars usually depended for their reference material on information obtained from previous Classical authors. The naturalist
Conrad Gesner (1516-1565), whose *Historiae animalium* was widely read in the Renaissance, is another case in point: in that work he showed the animals’ places in history, literature and art and included mythical creatures and imaginary beasts, intermixed with animals brought back from the New World. Foucault explained his attitude with these words:

> Until the time of Aldobrandi, History was the inextricable and completely unitary fabric of all that was visible of things and of the signs that had been discovered and lodged in them [...] (Foucault 1994 [1966], p. 129).

The reason why, as I have just said, Foucault considers Jonston as the threshold of the classical age is that in his *Natural history of quadrupeds* the latter eliminated almost completely the literary considerations still present in Aldobrandi. For Foucault, in the Classical Age naturalists eliminated fables and documents, began to take close examinations of the living beings and reported what they see in the most neutralized way. The ancient texts ceased to be the documents of history, replaced by herbariums, collections and gardens. The taxonomic thinking as we know it, described above by the mental actions of grouping, ranking and systematizing on the basis of the characters of animals and plants, was fully emerged.

When Rossi (Rossi 2000) says that in the Classical age scholars got rid of the superfluous, he must be interpreted as saying not only that they eliminated legends, hearsay and literary considerations from their classifications but also that they *focused* their attention on a particular portion of the world: certain aspects of
plants and animals up to that moment ‘unknown’. The stamens and pistils of flowers began to be enumerated, the sizes of corollas measured, the shapes of the flowers recorded. It was at that point that to group on the basis of the many emerging differences and similarities between animals and plants, to rank the many species just discovered in order to arrange them and, finally to systematize, became crucial.

3.5.3 A New Kind of Evidence

Foucault points out a sentence of Linnaeus that established what should be taken into account as fundamental in the study of a living being.

As Linnaeus said [...] ‘every note should be a product of number, of form, of proportion, of situation’. For example when one studies the reproductive organs of a plant it is sufficient but indispensable to enumerate the stamens and pistils [...], to define the form they assume, according to what geometrical figure they are distributed in the flower [...] and what their size is in relations to organs (Foucault 1994 [1966], p. 134).

As a matter of fact, number, form, proportion and situations were the standards of evidence shared by the new community of naturalist historians in the Classical Age. This is a standard of evidence entirely new: as we have seen in section 2.2 in the Renaissance an opinio was supported by signs. Different scholars could well disagree on which characters to consider in order to group and rank different classes of living beings: Joseph Pitton de Tournefort (1656-1708) looked at corollas
and the features of fruits whereas Linnaeus focused on sexual characters of plants. But they all agreed that number, form, of proportion and situation must represent the features necessary for reconstructing the place of a natural object in the natural world; they all agreed on trusting only their eyes and disregarding other organs of sense. In other words, they all agreed on certain standards of evidence for justifying what can be considered a sound classification. In doing so they established that to classify means to group and rank on the basis of visible similarities. The emergence of this way of thinking with these standards of evidence is a fact that characterizes Linnaeus’ epoch.

Botanic gardens and zoological collections represented the institutions that reinforced the ‘rules’ shared by the naturalist historians. What came into being was a community of people that shared common standards for describing the living world. In order to classify the members of this community had to perform measurements and make comparisons, to carry out experiments and observe the capacity of interbreeding of animals and plants. More recently, measures and comparisons involve genetic sequencing. These physical actions necessary for comparing and contrasting characterize the taxonomic way of thinking.

3.5.4 Meaningful and Meaningless Classifications

The change between the way of thinking of the Renaissance scholars and that of the seventeenth century taxonomists was so radical that the classifications made by the former appear alien to us. To give an example, Gesner grouped unicorns with rhinoceroses; Aldovrandi described snakes under headings such as: various meanings of the word ‘serpent’, synonyms and etymologies, differences,
form and description, nature and habits, temperament as well as prodigies and presages, monsters, mythology, fables, allegories and mysteries, hieroglyphics, emblems and symbols, proverbs, heraldic signs (Foucault 1994 [1966]). The juxtaposition of monsters and snakes, mythical creatures and rhinoceroses as well as that of habits of animals and proverbs and hieroglyphic is impossible for us to make sense of -- it is a way of classifying and describing living creatures that seems to cross the boundaries of our rationality.

Indeed, Aldovrandi’s and Gesner’s lists are reminiscent of the essay *The analytical language of John Wilkins* by Jorge Borges cited by Foucault in a well known passage of the preface to *The Order of Things*. Borges described a Chinese encyclopaedia in which it is written that animals are divided into:

1. Those that belong to the Emperor,
2. Embalmed ones,
3. Those that are trained,
4. Suckling pigs,
5. Mermaids,
6. Fabulous ones,
7. Stray dogs,
8. Those included in the present classification,
9. Those that tremble as if they were mad,
10. Innumerable ones,
11. Those drawn with a very fine camelhair brush,
12. Others,
13. Those that have just broken a flower vase,
14. Those that from a long way off look like flies.

Foucault commented:

It is not the ‘fabulous’ animals that are impossible, since they are designated as such, but the narrowness of the distance separating them (and juxtaposing them to) the stray dogs, or the animals that from a long way off look like flies. What transgresses the boundaries of all imagination, of all possible thoughts, is simply that alphabetic series (a,b,c,d) which links each of those categories to all the others (Foucault 1994 [1966], p. XVI).

For the same reasons, the juxtapositions of Aldovrandi and Gesner live in a space that is unthinkable for the student of classifications who lived some dozens of years later: the mode in which animals and plants are grouped make their writings fantastic, irrational, meaningless.

3.5.5 Techniques of Self-Vindication

Hacking followed William James (1842-1910) in considering classification as ‘the fourth’ branch of logic together with deduction, induction and abduction (Hacking 2006a, p. 8-9). Hacking’s point can be developed in order to explain in which sense the taxonomic way of thinking is self-authenticating. When a classification does not satisfy us (e.g. because it does not happen to mirror our current theories) we have still to rely on taxonomic thinking in order to provide more and more suitable classifications. It is never the case that the taxonomic way of thinking and
doing is dismissed as inadequate or conducive to falsehood. What might be refused is the systematization of a classification, that is, the theory we use to interpret the classification. But whether or not we keep on using the same classification or we provide a more suitable one it is taxonomic thinking that we use. In other words, the taxonomic way of thinking and doing itself is immune to refutation, as deduction, induction and abduction are.

This point can be illustrated by historical considerations. As I have explained, Aristotle suggested that zoological forms could be arranged on a hierarchical scale, which reflected the grades of perfection of living beings. The organizing schema of a single linear chain remained substantially identical in the following centuries although it was enriched of always new forms of living beings. When the evidence that the world is forever changing became overwhelming, it was thought that the static linear chain could be converted into a sort of biological escalator, leading from the lowest organisms to man. Lamarck thought that, due to the pressures of adaptation to changing environment, the scala naturae had to make room for a certain number of branches. In other words: he kept on using the taxonomic way of thinking although he changed his theories. Eventually, the theories that interpreted the schema of a single linear chain were refused and Darwin’s theory triumphed. It was the hierarchical order based on continuous branching and no subsequent joining of branches erected by Linnaeus that was compatible with the theory of evolution. Linnaeus, as it were, had changed the organizing device behind the classification but, again, he had still relied on the taxonomic way of thinking. He proposed not a linear chain but a logical tree in which we can reach a species by making successive twofold divisions (vertebrate
and invertebrate, mammals and non-mammals within the vertebrates, carnivores and non-carnivores within the mammals and so forth). This type of logical tree that represented Linnaeus’ new classification was not a novelty: it was familiar at least since Plato and Aristotle and Ray had already used it before Linnaeus (Gould 2000a, p. 23). However Linnaeus had interpreted his classification in creationist terms. His way of grouping and ranking was neither true or false: it just happened to mirror the topology of evolutionary systems (Gould 2000a). It was his systematization that was wrong. Ultimately, while our systematizations can be refused, taxonomic thinking is inescapable: we have still to rely on it again and again when we want to know about the living world. It is in this sense that one can consider taxonomic thinking self-authenticating.

To summarize, to define the taxonomic way of thinking as a ‘SoT’ is appropriate because it satisfies all the characterizing properties of SoTs. The taxonomic style of thinking and doing has followed a specific trajectory: it ‘surfaced’ in the writings of Western thinkers since Aristotle but it flourished only in Linnaeus’ epoch, when a community of people that relied on certain standards of evidence came about.

3.6 Conclusions

There exist six ways of thinking and doing (SoTs) that share a set of common characterizing features. I have described two of them in the previous chapter, following some of Hacking’s suggestions. In this chapter I have argued for the existence of other four SoTs by outlining their features. All the differences notwithstanding, striking parallels can be drawn between different SoTs: they
emerged suddenly at certain points in time, crystallized around communities that shared certain standards of evidence and represent basic and inescapable forms of reasoning in the sense I have discussed above.
Chapter Four: The Theory of Styles of Thinking: Further Developments

4.1 Introduction

For the theory I am developing to be complete it is necessary to address three more issues. First of all, the characterization of scientific thought is still partial: in the next section I shall examine a few ways of thinking and explain that they differ from the SoTs I have presented. One of the upshots of my discussion will be that SoTs possess particular techniques of self-vindication that make them unique in the variety of ways of thinking. Secondly, up to now I have isolated single SoTs in scientific research in order to describe and analyse them. However, very rarely do SoTs operate in isolation: in section 4.3 I shall explain how they intertwine with one another in scientific research. Finally, in the last section I shall spell out the claim that a style-dependent sentence has no truth-value for a community that adopts a different SoT.

4.2 Spurious Styles of Thinking

According to my account in the previous sections, there is a set of ways of thinking and doing that share a few precise features, although they differ in questions, methods and trajectories of development. I have reserved the
expression ‘style of thinking’, abbreviated to ‘SoT’, for these particular ways of thinking. I make no claim that they exhaust the set of all the SoTs, the actual ones or those that might come about in the future. I only claim that, should we exclude one of the SoTs I have presented, we would inevitably end up with a partial understanding of scientific thinking.

SoTs are distinct from other ways of thinking that do not possess all the characterizing properties and that, therefore, are spurious SoTs, as I shall call them -- species of a different taxonomic category. Ever since Hacking introduced his notion, some scholars have argued that some particular ways of thinking should be counted as SoTs. I shall show that their conclusions are fallacious for reasons that differ case by case. However, the fact that those ways of thinking are spurious SoTs does not prevent some of them from having played a crucial role in the history of scientific thought.

4.2.1 Are Religions Styles of Thinking?

In the course of his argument for the thesis that ‘Hacking has failed to make progress as far as criteria are concerned’ (Kusch 2010, p. 170) Martin Kusch made the point that Christian-Catholic reasoning satisfies the characterizing properties of SoTs. For him this way of thinking introduces:

- new types of objects (God and angels that I can directly experience),
- evidence (that God appears to me is *prima facie* evidence of his existence), sentences ('God told me that I should do his will from day to day in humility and poverty') modalities and possibilities (it is possible for God to be perceived by me) (Kusch 2010, p. 170).
Furthermore, according to Kusch, Christian-Catholic thinking is self-vindicating for two different kinds of reasons. Firstly,

Theorists of mystical perception have developed techniques of self-stabilisation. That God seems to talk to us is not good enough; for the experience to pass muster it must cause 'interior peace' rather than perturbation, trust in God rather than despair, or calm rather than impatience (Kusch 2010, p. 170).

Secondly,

The authority principle and the principle of infallibility of the Pope in fundamental theological questions are further stabilization techniques (Kusch 2010, p. 170).

Now, notice that in principle his argument could be extended to any religion. One could say that, in general, religious thinking 'introduces' a variety of 'objects' (ghouls, ghosts, spirits), new truth-candidates ('God made the angels endowed with one, two, three and also four wings' (Koran 35:1)), new evidence (miracles, signs, states of mind), possibilities (it is possible for spirits to inhabit a wood); and possesses techniques of stabilization (e.g. the Koran proves the prophethood of Mahomet, then the Koran is the revelation of a prophet). However, that religions can be counted as SoTs is a result difficult to accept: the notion of SoT has been conceived to describe scientific ways of thinking and one
would expect that not all the characterizing properties of SoTs are satisfied by non-scientific ways of thinking.

Hacking has never considered the problem of marking a difference between SoTs and ways of reasoning that are non-scientific. Before 2012, the only claim germane to this point he made is that what distinguishes scientific from non-scientific discourse is that there are no self-authenticating techniques for the larger part of morality and humanistic thought (Hacking 1996, p. 74). However he never substantiated this claim, which is unfounded in the case of religions in the light of Kusch’s example: Christian-Catholic thought – argues Kusch - does not belong to the scientific discourse and is self-authenticating. As I shall argue in the next subsection it is necessary to spell out what a self-authenticating technique is in order to highlight the difference between certain ways of thinking and the SoTs I have presented.

However, in 2012, two years after the publication of Kusch’s paper Hacking made another claim (the characterizing feature 1) already mentioned) that, this time, does help to identify a difference between religious ways of thinking and genuine SoTs, as I am going to argue. He clarified that SoTs are ‘ways of finding out in the sciences’, that is: ‘finding out that “so and so”’ and ‘finding out what’s true, and how to change things’ (Hacking 2012, p. 3). Furthermore, by claiming that the emergence of SoTs is to be understood in an ‘ecological way’ he left no doubt about the fact that a SoT should not only be understood as a way of finding out but also as a way of altering our physical world in order to make it our ‘home’, in the Greek sense of the word ‘ecological’. These suggestions can be summarized by saying that to adopt a certain SoT involves the use of our bodies for fulfilling
one or more of these aims: to know why a given fact happens; to predict certain events; to find out how to elicit or describe a phenomenon; to reveal how facts, entities or phenomena are connected with one another.

Now, my point is that religious ways of thinking may well involve ways of doing but never for fulfilling the aims just mentioned. Indeed, compare the ways of doing of religions with those of the SoTs I have described. The monastic life certainly involves particular activities such as praying or introspecting. However, whether or not these activities can cause effects on our world, it is never the case that believers systematically act and intervene 'hands-on' in the world in order to reveal patterns of relations between facts or entities that appear *prima facie* unrelated or in order to predict certain facts. On the other hand, to think and act in the algorithmic SoT allows us to discriminate, compare or associate objects and events on the basis of their numerosity. Physicists use the visual resources involved in the postulational SoT in order to reveal the relations between different phenomena, e.g. they use geometry to study the phenomena of light. Laboratory scientists build machines that induce certain effects in order to find out about the existence of theoretical entities, connect their properties with one another and predict their behaviour. Statisticians perform several actions, such as selecting random samples of voters, in order to find out how the probability of the election of certain candidates are related to certain events. The taxonomists of the seventeenth century conducted field research in order to find out about the structure of divine thought: do different morphological types descend from a common ancestor? Finally, by hunting high and low for the traces of past-events,
historical scientists build patterns of relations between different facts, as when they reconstruct the evolutionary history of *Homo sapiens*.

In brief, by thinking and doing, scientists reveal patterns of relations and give explanations of how and why natural phenomena occur. On the contrary, religions do not systematically intervene in the world in order to put in relation different physical phenomena; they are rather concerned about questions of ultimate meaning and moral value. My conclusion is that Kusch’s paper does not take into due consideration a property of SoTs: that they are ways of doing in order to find out about phenomena. The wrong conclusion that, according to the claims of the SoTs project, religions should be counted as SoTs follows from having disregarded this fundamental property of SoTs deeply rooted in Hacking’s philosophy of science.

### 4.2.2 Internal and External Techniques of Self-vindication

Hacking wrote that

> [E]ach style has its own self-stabilizing techniques, but [...] some are more effective than others. [...] in the case of statistics there is an almost too-evident version of self-authentication (the use of probabilities to assess probabilities). But that is only part of the story, for I emphasize the material, institutional requirements for the stability of statistical reasoning (Hacking 1996, pp. 73-74).

This passage implies that there are different self-stabilizing techniques and that, for example, in the case of the statistical SoT, material and institutional factors
contribute to the stability of a SoT. So Hacking seems to treat these very factors and the use of probabilities to assess probabilities alike: he counts both of them as 'techniques of stabilization'. It seems to me that the two things (social factors and use of probabilities to assess probability) must be kept distinct. To take as an example the postulational SoT, one thing is to attribute its stability to the argumentative character of the Greek society and other historical and social circumstances, another thing is to explain its stability by noting that there is no way of assessing the demonstration of a theorem apart from that of using another demonstration (the use of proof to assess proof). In the first case, what provides stability to the postulational SoT is something external to its cognitive methods: social factors. In the second case, as for other SoTs I have described so far, to be self-authenticating means that the postulational SoT does not answer to any external canon of truth independent of itself: the truth of a proposition whose sense hinges on the postulational SoT can only be assessed by using that very SoT. This very fact makes the postulational SoT immune to refutation and therefore stable, as Hacking says. Ultimately, in the first case the techniques are external to the SoT, in the second they are intrinsic to the SoT.

I shall distinguish 'internal techniques of self-vindication' from 'external techniques of self-vindication'. An internal technique refers to cases in which stability is provided by the characteristics of the way of thinking itself, which is self-reinforcing: in order to assess what has been found out by using a certain form of reasoning (e.g. abduction for the historico-genetic SoT, deduction for the postulational SoT, computation for the algorithmic SoT) one has to use that very form of reasoning. SoTs are inescapable, e.g., it is not possible to say that counting
is a ‘wrong’ method of thinking and doing on which the algorithmic SoT relies: we can only assess a calculation by using another kind of calculation. On the other hand, an external technique refers to cases in which social, cultural and historical factors make a SoT stable. As I have argued in the previous chapter, all the SoTs I have described possess internal self-vindication techniques in addition to external: both internal and external techniques concur to giving stability to a given SoT.

Hacking wrote:

> The apparent circularity in the self-authenticating styles [SoTs] is to be welcomed. It helps explain why, although styles may evolve or be abandoned, they are curiously immune to anything akin to refutation (Hacking 2002 [1992], p. 192).

I would correct this passage by making it more precise: as SoTs possess internal techniques of self-vindication they are immune to refutation; its style-dependent sentences cannot be refuted, neither in principle nor in fact. Of course, SoTs might be abandoned should we find better ways of knowing or should we stop craving for knowledge, a circumstance impossible to imagine. Conversely, if a way of thinking possesses only external techniques of self-stabilization in principle it could be refuted, e.g. when the social context changes. I am not saying that this has to happen; I am saying that whereas a given SoT cannot be assessed by other SoTs, in principle it would be possible to use one or more SoTs to assess a way of thinking that does not possess internal techniques of self-vindication. For example, it would be possible to use the SoTs I have presented to assess other ways of thinking, but it would be impossible to assess the postulational SoT by another SoT.
The styles project as formulated by Hacking does not provide an answer to the question as to why SoTs have endured. I suggest that a possible answer is that as they rely on internal techniques of self-vindication they can never be refuted (I will say more about this issue in section 9.3). The examples I am going to give in the next subsections will clarify the points of this subsection (see also my parallel between Wittgenstein and Hacking in chapter eight).

4.2.3 A Scholastic-inquisitorial Style of Thinking?

Barry Allen argued that the reasoning of the inquisitors of the early modern period (1490-1690) possesses the features of a SoT (Allen 1993). When examining cases of witchcraft, two works were often consulted: the *Malleus Maleficarum* by Heinrich Kramer (1430-1505) and James Sprenger (1436-1495), admired theologians and Dominican inquisitors, and the *Daemonolatria* by Nicolas Rémy (1530-1616), a secular magistrate who condemned nine hundred witches. For Allen, both these works proposed a debate about the existence of witches (the 'new objects'), contain candidates for truth or falsehood ('children can be generated by incubi and succubi' or 'witches are more common than in the past') (Allen 1993, p. 115). Furthermore, according to Allen, the reasoning of demonologists is self-authenticating because of their attitude towards textual authority: 'each writer cited his predecessors and became an authority worthy of citation' (Allen 1993, p. 112). Lastly, inquisitorial reasoning involves a new kind of evidence: confessions under torture, special signs in the body, taciturnity, and incapacity to recite the Lord's Prayer (Allen 1993, p. 111). Allen concluded that the men who persecuted witches were not misogynistic monks with narrow minds.
but secular magistrates and highly educated lawyers who ‘shared a style of reasoning [(SoT)] which was [...] productive of reasoned discourse and knowledge’ (Allen 1993, p. 105). He called this way of thinking ‘scholastic-inquisitorial’.

Notice that the attitude of appealing to the authority of predecessors, which provides stability of the scholastic-inquisitorial way of thinking, is an external technique of self-vindication. By no means can this technique of self-vindication be comparable to that of SoTs: whereas the propositions of Malleus Meleficarum could well be assessed now by, e.g., the laboratory SoT, which would reject its kind of evidence, SoTs are immune to refutation just because they possess an internal technique of self-vindication. Attributions of witchery might be mutually supporting but, as it is possible in principle to use another SoT, e.g. the laboratory SoT, to assess them, it cannot be said that the scholastic-inquisitorial way of thinking possesses internal techniques of self-authentication. As I have said, Hacking is reluctant to state rules. However, it is important to note that if one requires that any SoT must possess internal self-stabilization techniques then Allen’s candidate is not a SoT. For sure, its techniques of self-vindication make it dissimilar to the SoTs I have presented.

Another obvious difference between Allen’s candidate and SoTs is that there is no relevant sense in which the former can be considered a way of finding out: demonologists used methods such as torture, interrogation and physical inspection in order to assess whether a person was a minion of Satan. These methods have a very narrow range of application: they only help to find out whether or not innocent human beings are witches.
4.2.4 The Chemists’ Way of Thinking

Bernadette Bensaude-Vincent argued that both Crombie and Hacking wrongly located the emergence of laboratory style in Boyle’s air pump (Bensaude-Vincent 2009). Indeed, in their History of Chemistry (Bensaude-Vincent and Stengers 1996) Bensaude-Vincent and Stengers had explained that the laboratory as a physical place was invented by the alchemists and then was adopted first by the chemists and only afterwards by the physicists. Chemists had to fight to secure the identity of chemistry against other scientists, until it became an autonomous discipline. The conquest of this autonomy, they argue, was determined by the combined and changing action of three factors: professions, institutions and techniques of laboratory. For example, for Bensaude-Vincent the laboratory of chemistry must be conceived as distinct from the laboratory of physics (Bensaude-Vincent 2009, p. 370).

In Bensaude-Vincent’s view the chemists’ way of thinking possesses all the characterizing properties of SoTs. For example, she noted that by altering materials chemists have been able to know the features of the substances. For her, chemistry is a way of knowing by making -- only through technological processes are the features of the chemical elements revealed. She also considered the chemical atoms introduced by John Dalton (1766-1844) as the epitome of the ‘new objects’ introduced by chemists’ style and the chemical formulas as a perfect example of ‘new candidates for truth and falsehood’.

Hacking replied that ‘the chemical laboratory is different from, and older than, the physical laboratory’ and that it is better not to multiply the number of
styles in Crombie’s list beyond necessity, ‘and instead to see the invention of the laboratory as making experimental exploration more ‘systematic’, to use Kant’s word’ (Hacking 2012, p. 6). That is: Hacking proposed to exclude Bensaude-Vincent’s candidate on the ground that it was with the physical laboratory that experimental practice raised to the rank of science.

As I interpret Bensaude-Vincent, different reasons explain why, despite ‘all sorts of enterprises of reduction or refutation throughout their history, chemistry remain[ed] nevertheless relatively stable’ (Bensaude-Vincent 2009, p. 376). Firstly, there is what I have called external techniques of self-stability: the combined role of institution and professions. Secondly, Bensaude-Vincent maintained that ‘[chemists] do not claim to provide a causal explanation, and their theory is closer to being a narrative than a fundamental explanation’ (Bensaude-Vincent 2009, p. 372). For her chemists’ practice is a sort of ‘cookery’ that does not involve theory books but recipes for manipulating substances in laboratory or mnemonic rules for dealing with chemical formulas (Bensaude-Vincent 2009, p. 371). Rather than representing the real world, chemical formulas are useful fictions to be manipulated in order to predict and characterize compounds. For Bensaude-Vincent there is no real effort for unveiling a deeper structure of the world: chemists mainly want to understand the principles behind particular classes of reactions, the changes undergone by matter under certain processes, the structure and the properties of certain compounds.

It is not clear whether by likening chemistry to cookery Bensaude-Vincent wanted to suggest that chemistry is self-authenticating. At any rate, on the basis of her claims it would be possible to argue for this thesis by noting that a chemical
formula is correct because it follows certain rules, which are not really discussed and questioned by the chemists. For example, the reduction-oxidation reactions are worked out in reference to 'rules' imposed by the conservation of charge and mass. Chemists simply need to apply definite procedures in which only certain possible oxidation states of the elements are possible. And how can they double-check the correctness of a reduction-oxidation reduction? By looking at other chemical reactions in which it is found that a given element does get the same oxidation state number as in the reaction under study. In other words, the conclusion of the argument would be that chemistry could be thought as a set of procedures that are self-reinforcing. That is, according to this argument, chemistry would possess an internal self-authenticating technique (and therefore it would be comparable to other SoTs).

If this is the essence of Bensaude-Vincent's point, it might be prone to the following objection. Suppose that a very large part of chemistry had been reduced to physics in the original sense of Ernst Nagel (1901-1985): a secondary science can be reduced to a primary science when the laws and theories of the secondary science are a logical consequence of those of the primary science (condition of derivability) and when the terms of the secondary science can be redefined in terms of the primary science (condition of connectability) (Nagel 1961, p. 336-397). Roughly speaking, that a science is reduced to another means that a set of propositions is derived from another set. In that case, there would be a sharp difference between the chemical way of thinking and the SoTs I have presented. Compare the postulational SoT with the chemical thinking. The former is somehow irreducible in the sense that the truth-value of the propositions of
geometry hinges on the very postulational SoT: only demonstration can be used to check another demonstration. Conversely, in the hypotheses of reducibility, the propositions of chemistry would depend on those of physics and therefore the chemists' style would not be really autonomous from the Laboratory SoT. Otherwise stated, although chemistry is stable and autonomous as a discipline, its propositions would have an external justification – and therefore the chemical way of thinking would not be self-authenticating. In parenthesis, notice the difference between the chemists' way of thinking and the historico-genetic SoT: as I have already argued, the latter has an internal self-stabilizing technique, and indeed biology, palaeontology and geology cannot be reduced to physics.

In conclusion, if chemistry can be reduced to physics it does not possess internal self-stabilizing techniques. Though, it must be said that it is still a res controversa whether or not chemistry can be reduced to physics. For different reasons Popper, Kuhn and Feyerabend were critics of reduction, while Paul Dirac thought that the difficulty of reducing chemistry to physics was only technical (cited in Scerri 1994, p. 162). Today it is widely recognized that the valence of an element can be calculated and explained by putting it in relation with the number of electrons in the outer orbit of atoms. However, according to Roald Hoffmann there are still in chemistry concepts such as PH or reactivity that resist reductionism (Hoffmann 1995). More recently the philosopher of chemistry E.R. Scerri argued that the hope of any strict or exact reduction has been abandoned and that all that remains is the possibility of approximate reduction, a notion still vague because no criteria have been put forward (Scerri 1994, p. 168).

Ultimately, the chemical way of thinking has played a fundamental role in
the history of science and it is still crucial in many areas of scientific research. Since whether or not it is reducible to physics is a question for determination it is impossible to conclude that it satisfies all the characterizing properties: it might not possess internal techniques of self-vindication. We can only conjecture that if chemistry is in principle reducible to physics the chemists’ mechanisms of reasoning are not so basic, fundamental, and inescapable as those of the SoTs I have presented.

4.2.5 The Astrological Way of Thinking

As we have seen in section 2.2, Hacking presented a sentence of Paracelsus as an example of style-dependent proposition. For him the way Paracelsus reasoned is entirely different from ours and exemplifies a ‘Northern European Renaissance tradition of a bundle of hermetic interests: medicine, physiology, alchemy, herbals, astrology, divination’ (Hacking 1983b, p. 70-71). I shall briefly discuss here only the astrological way of thinking, as similar points can be made about alchemy and divination.

Ancient peoples such as the Babylonians practised astrology and believed that many aspects of the skies had some importance in divination. However, it was with the conquests of Alexander the Great that astrology crystallized out of an amalgam of Babylonian, Greek and Egyptian elements. I shall focus on its elementary grammar, although astrology has greatly evolved beyond his apparent roots in Babylonian culture becoming a variegated group of doctrines.

To put it simply, astrological thinking is purely deductive: to think in the
astrological way amounts to drawing some consequences from precise facts in the skies according to certain rules. For example:

When Venus appears in Dilgan (Virgo), rains in heaven, floods on [earth] the crops of Aharru will prosper.

a sentence from a collection Assyrian omens called 'Enuma Anu Enlil' (Bobrick 2005, p. 16).

However, the astrological way of thinking is more accurately described by three steps. The first step is an assumption: it is postulated that celestial bodies have an influence upon human affairs. To all intent and purposes, this is the postulate of astrology: ‘astrology extrapolates factual influences by postulating that the positions of the Sun, Moon and 8 planets other than Earth with respect to the sky background, as well as with respect to each other, influence terrestrial events and human psychology and destiny’ (Zarka 2011, p. 420). I shall call this postulate ‘the astrological correspondence principle’ in that it is a principle that establishes a correspondence between mundane facts and cosmos. The second step consists of casting a horoscope, which is a map of the positions of Sun, Moon and 8 planets drawn for a particular moment of time and read according to established rules. In many traditions this map is round: the circle is divided in twelve sectors, which are called the ‘houses’; in the outer portion are drawn the signs of Zodiac, that is, twelve stations or ‘signs’ along the ecliptic, the apparent path of the Sun around the earth. Once the astrologer knows the date of birth, time and place of someone, he can draw up a horoscope. First, he calculates the ascendant; the point where the ecliptic intersects the horizon in the east at the place of birth; from that point, he
draws the twelve sectors (houses) of the circle. Then he writes the signs along the round and, consulting an ephemeris, marks the location of the sun, moon and planets and their relative angles for that particular date and time. The third step consists of interpreting the horoscope, that is of drawing certain conclusions according to the rules of a certain tradition of astrology.

'Houses', 'signs', 'zodiac', and other conventions and reference frames can be considered the 'new objects' introduced by the astrological way of thinking. The horoscope represents a form of evidence for the believers in astrology: an astrologer will consider each of the possible relative angles between pairs of planets (i.e. Conjunction (0°), Opposition (180°), Square (90°), Trine (120°)), or how much an angle made by two points differs from the exactness, as evidence of the truth of certain sentences.

I think that two different external techniques of self-vindication made the astrological way of thinking endure. First of all, institutions, authorities and mass media have always been consistently woven into the discourse of astrology and astrologers cite their predecessors as authorities: the Tetrabiblos, Ptolemy's treatise on astrology, was considered the most authoritative astrological work of antiquity not only in the Medieval Latin West but also in the Islamic world. Finally there is the astrological correspondence principle, which is never questioned. Its role can be likened to the infallibility of Pope in Christian-Catholic reasoning.

The astrological way of thinking does not belong to the same taxonomic category as the SoTs. Indeed, it does not possess internal techniques of self-vindication because the astrological correspondence principle as well as sentences such as
His condition quickly worsened because of the rapid motion of the moon (found in Bobrick 2005, p. 137),

(written by the mathematician Gerolamo Cardano (1501-1576) in 1553) are obviously not potentially immune to refutation. The astrological thinking is not inescapable: one can reason in a different way to show that it is not a valid form of reasoning. But one cannot reason in a different way from the algorithmic SoT to show that a calculation is wrong or in a different way from the historico-genetic SoT to show that there is a better explanation for the extinction of dinosaurs.

It is also possible to argue that the astrological way of thinking is not a SoT by noting that the evidence for any astrological claim is provided through the horoscope by the positions of planets. No further investigations are carried out in order to know whether or not the positions of the planets exert an influence upon terrestrial events. This point lies behind Kuhn complain that ‘astrology has rules to apply but no puzzles to solve and therefore no science to practice’ (Kuhn 1970, pp. 8-9) and Feyerabend’s claim that ‘[astrology does not] attempt to proceed into new domains and to enlarge our knowledge of extra-terrestrial influence’ (Feyerabend 1978, p. 96). Therefore, the astrological way of thinking is a spurious SoT: it cannot be likened to the SoTs I have presented because it is much interested in making predictions and very little in finding out why and how.
4.3 Styles of Thinking in Action

Let us now put aside the spurious SoTs. I now want to make it clear that sometimes the same scientific problem has been tackled in two distinct ways based on different SoTs. Consider this example. In the third century BCE, the Greek astronomer Aristarchus of Samos calculated the distances of the Sun and the Moon from the Earth in units of the Earth's radius. His style of thinking was postulational: Len Berggren and Nathan Sidoli liken his treatise *On the Sizes and Distances of the Sun and the Moon* to mathematical texts of the early Hellenistic period, such as Euclid's works (Berggren and Sidoli 2007). In fact, in order to work out the distance of the Moon and Sun, Aristarchus drew geometric diagrams of the Earth's, the Sun's and the Moon's respective positions during a half moon and a lunar eclipse, made few observations, formulated geometrical and computational hypotheses, used known geometrical theorems and finally derived the distance of these celestial bodies (Berggren and Sidoli 2007, p. 215). However, in principle, the distance to the Moon can also be determined by adopting the laboratory SoT through these steps: determination of acceleration \( g \) by measuring the period of a simple pendulum; measurement of the sidereal period of the Moon; calculation of the Earth's radius by measuring the gravitational constant \( G \) and applying the law of universal gravitation; finally, by equating the force between the Moon and the Earth to the Moon's centripetal force, the distance to the Moon can be expressed and calculated in terms of the acceleration \( g \), the sidereal period of the Moon and the Earth's radius. In the case of Aristarchus the result is obtained by thinking in a postulational way: no laboratory thinking is
involved, although Aristarchus had to make a few astronomical observations. In the second case, the laboratory thinking is necessary to reason in terms of physical laws and experimental apparatuses.

4.3.1 The Accumulation of Styles of Thinking

Another point to note is that the project of SoTs implies that 'styles of scientific reasoning [SoTs] tend to accumulate' (Hacking 1983b, p. 56). Unlike Kuhn's paradigms, SoTs do not replace one another. As SoTs have emerged they have often been adopted together in order to solve single scientific problems: the algorithmic, the postulational, the statistical, the historico-genetic, the taxonomic, and the laboratory SoTs are not mutually exclusive. Today scientists switch from the ways of thinking and doing, the methods, the objects, the type of evidence of a SoT to another while they do scientific research. Algorithms, hypothetic deductive methods, distributions, inferences to the best explanation, taxonomies, analogical models and measurements as well as distinct ways of thinking and doing, take your pick: whatever scientific problem one considers, one can recognize the features of different SoTs.

The simplest example of simultaneous use of different SoTs that can be given has to do with the use of vectors to represent physical magnitudes such as forces and velocities. As vectors are often represented in the three-dimensional space as arrow-like objects, which can be multiplied by real numbers or added together, the simple rules that define their use rely on the postulational SoT. Therefore, we can expect that in many problems of physics both the postulational SoT and the laboratory SoT play a crucial role. Consider for example three positive
charges Q1, Q2 and Q3 arranged in a right triangle ABC. Suppose that one has to determine the force on Q2 on the point B, the vertex of the right angle. The solution of this problem is worked out by summing up two vectors: that one that represents the Coulomb force exerted by Q1 and that pushes Q2 away from it, and that one that represents the Coulomb force exerted by Q3 and that pushes Q2 away from it. By applying Pythagoras’ theorem to the triangle formed by the two force vectors on Q2 and by their sum it is possible to calculate the magnitude of the resultant force on Q2. In principle, three SoTs, the laboratory, the postulational and the algorithmic one, are necessary for working out the result. First of all, thinking and doing in the laboratory SoT is necessary to put forward and test the Coulomb law; secondly, thinking and doing in the postulational SoT is necessary to represent the forces in a plane and apply geometrical considerations: finally, the algorithmic SoT is necessary to perform multiplications and divisions and obtain a number, the magnitude of the force.

4.4 Truth-value, intelligibility, pure nonsense

In order to arrive at a complete theory of SoTs I want to address a last question concerning the semantics of sentences whose truth-value hinges on their SoT (style-dependent sentences). I shall elaborate on the claim that members of a community cannot make sense of a style-dependent sentence uttered by another community that adopts a different SoT. More precisely, I shall define and examine
the type of intelligibility that can be achieved for different kinds of sentences by communities that adopt different SoTs.

True-or-false sentences are sentences that express true-or-false propositions. Henceforth, as I have done so far, I shall use the term 'sentence' rather than terms such as 'propositions', 'statements' or whatever the logicians use in order to designate assertive acts of thought: I want to call attention to the fact that I am referring to grammatically correct strings of words that come to be uttered by members of certain communities of people at specific points in time.

Suppose that a SoT crystallizes around a community of people. Its members will communicate to each other by expressing style-dependent and style-independent sentences. In that case, they will have three kinds of intentions: to utter those sentences, to confer meaning to them and to communicate them. The first intention is satisfied by using parts of the body to produce a sound; the second intention by expressing the sentences correctly and imposing truth-conditions on them. Once these two conditions are satisfied, a hearer could in principle recognize them and understand the sentence so to satisfy the third intention of the speaker. Ultimately, putting aside the intention of uttering the sentence, the hearer must recognize the speaker's intention of 1) formulating the sentence correctly, i.e. according to the rules of the language and of 2) providing truth-conditions for that sentence. Points 1) and 2) express two different concepts: point 1) concerns the correct syntactical arrangement of the words that, by their meaning, will determine the sentence meaning, that is, the conventional meaning of the sentence; point 2) concerns the speaker meaning, that is, what the speaker means by expressing the sentence. This distinction between these two concepts has been
brought to the fore by the philosopher Paul Grice (1913-1988): the notion of meaning involved when someone says that a *sentence (or a word) means* so-and-so is to be distinguished from the notion involved when someone says that by saying such-and-such *she means* so-and-so (Grice 1968).

To give an example, if the sentence were ‘the dog is barking’, the hearer could recognize the intention of expressing a sentence correctly in English so to attribute to it a conventional meaning. Then, recognized this intention, the hearer could also recognize the second intention of providing truth-conditions: the sentence is true if, as a matter of fact, the dog is barking. Such being the case, the hearer could grasp the meaning of the sentence and the speaker could succeed in communicating it.

Now, for the argument’s sake, I shall imagine different cases in which speaker and hearer belong to different communities, even separated in time. If the hearer does not belong to the community that adopts the same SoT of the speaker the message conveyed from the latter may not get through. Take for example, this sentence in Middle English:

I ne have heer a fruyte

If the hearer (or the reader) belonged to the community of the Renaissance alchemists, she might not be able to recognize some words of that sentence and their syntactical arrangement – the conventional meaning of the sentence would be obscure. However, the hearer could investigate about the meanings of the words and the grammar rules employed by the modern scientists. She would find out that the translation is:
I do not have here a fruit

This sentence would not only have a conventional meaning but also a truth-value straightforwardly recognizable by the hearer — its conditions of truth do not differ from the conditions of truth she employs in her SoT. In other words, after investigation, the hearer would be able to recognize that:

1) the sentence ‘I ne have heer a fruyte’ means ‘I do not have a fruit (sentence meaning)

2) by saying ‘I ne have heer a fruyte’ the speaker means that her sentence is true if and only if the speaker does not have a fruit (speaker meaning).

In these examples above, once the hearer finds out the ‘sentence meaning’ by virtue of the correct translation of the sentence, she has a straightforward access to the speaker meaning. However this is not always the case. Take for example, this passage of Paracelsus:

The Adamant is a black crystal called Adamant or else Evax on account of the joy which it is effectual in impressing on those who carry it. It is of an obscure and transparent blackness, the colour of iron. It is the hardest of all; but is dissolved in the blood of a goat (Paracelsus 2007, p. 90)
Suppose that the hearer belongs to a community of the Eighteenth century that adopts the statistical SoT. She would not find it difficult to recognize the correct arrangement of the words and therefore the conventional meaning, but that would not be of much help – the access to the speaker’s meaning would not be straightforward anymore. To the hearer it would not be clear whether or not the speaker wants to attribute truth-conditions to that sentence and which ones they could be. The historical discontinuity between the way of thinking of the Renaissance thinkers and the statistical SoT has not really turned words such as ‘black, ‘transparent’, ‘goat’ into something else; but it has concealed from sight the conditions of truth given by the speaker. The hearer might want to ask how Paracelsus’ sentences could be proven true or false; or, alternatively, she might decide that no condition of truth has been attributed to it and that the sentence has to be deemed nonsensical.

If the hearer opted for the former alternative she would need to investigate about the truth-value of the sentences in the passage. She could find out that ‘the body of the Emerald is derived from a kind of petrine Mercury’ and that ‘it receives from the same its colour, coagulated with spirit of Salt’ (Paracelsus 2007, p. 90), but still struggle to understand the passage above. She could then turn to a study of the system of relations of similarity or equivalence between things posited by the Renaissance thinkers and become aware of the existence of criteria that provide a justification for sentences such as Paracelsus’. She could also learn that metals have virtues and essences and that planets sign metals, understand why crystals can be black and dissolve in animals’ blood, acquire skills in following the logic chain of Paracelsus’ reasoning. But still she would need an
awareness of the ways of doing that enable one to bring to light the signs and the virtues or an awareness of the laws that connect them with other signs and lead the speaker to certain conclusions.

All these circumstances, from the belief in analogies and sympathies between heavenly bodies and earthly things to Paracelsus' chain of reasoning, from his criteria of evidence to his way of doing constitute presuppositions that were unknown to the hearer. As it is against these presuppositions that Paracelsus' sentence acquires a truth-value for the Renaissance community of thinkers, the hearer would need to be aware of them in order to be able to attribute to the speaker the intention of conveying a message. In other words, only when the hearer has gained an insight into all those presuppositions can she recognize the intentions of the speaker, that is, can she recognize that the speaker has imposed conditions of truth on the sentence. When this is the case, the sentence becomes intelligible: by this term I mean that the hearer can recognize both the conventional meaning and the conditions of truth of the sentence -- how it would be validated in a cognitive frame different from her own.

By the same token, one can consider the following style-dependent sentence in the postulational SoT:

In any triangle the sum of any two sides is greater than the remaining one (Euclid, Book I, prop. 20)

Suppose that this sentence were translated from the Greek spoken by Euclid in around 300 BC into Archaic Egyptian. Although, one can imagine, the translated sentence would have a conventional meaning because of the correct syntactical
arrangement of the words, its truth-value, imposed by Euclid, would be concealed from a hypothetical ancient Egyptian. Indeed, according to the theory of SoTs, it was only with the abrupt emergence of the postulational style that the methods of reasoning and standards of evidence necessary for validating that sentence came about. An ancient Egyptian would find it impossible to examine any triangle and establish that the sum of two sides is greater than the remaining one. In order to recognize the intention of imposing a truth-value to that sentence an ancient Egyptian would need to come to see the presuppositions necessary to validate the sentence: among them, the existence of the postulational method and the different steps of the postulational way of thinking. This point is better expressed in terms of a verificationist account of meaning: whether or not Euclid’s sentence above is a candidate for truth or falsehood for a community of people depends on whether or not that community possesses the methods necessary to assess the truth of the sentence. Since the ancient Egyptians did not know the methods of postulational SoT they could not assess Euclid’s sentence. It is in this sense that this sentence would not have been meaningful for an ancient Egyptian. Notice that whereas we cannot understand what Paracelsus was saying, an Egyptian would arguably have understood Euclid’s sentence, although she would not have had the tools to verify it. To conclude, only when the hearer comes to see the presuppositions of the postulational SoT, and in particular its methods, does Euclid’s sentence above become intelligible to her. It is also important to note that the hearer would need to know the kind of questions at the root of the Greek inquiries. The questions asked by the Greeks were radically different from those asked by the ancient Egyptians: the latter asked questions about single geometrical figures; the former
were interested in underlying principles beside practical problems. Ultimately, the hearer cannot recognize the speaker’s meaning without being aware of another kind of presupposition: the kind of questions that were at the root of the postulational SoT.

4.4.1 Questions and Candidates for Truth or Falsehood

The points I have just made can be developed by considering what the philosopher Robin George Collingwood (1889-1943) wrote in the fifth and seventh chapters of his autobiography:

You cannot find out what a man means by simply studying his spoken or written statements [...]. In order to find out his meaning you must also know what the question was [...] to which the thing he has said or written was meant as an answer (Collingwood 1979 [1939], p. 31)

According to Collingwood, truth and falsehood do not belong to sentences in their own right but to the sentences as answers to questions. In this sense, what the ancient Egyptian would lack is to have a bearing on the kinds of abstract questions that are asked in the community of Euclid. Questions about the first principles were not ‘real’ for the ancient Egyptian society, to use a term introduced by the philosopher Nicholas Jardine in his book *The Scenes of Inquiry* (1991). For Jardine, a question is *real* in a community if there is a disposition to acknowledge the relevance of evidential considerations to that question. Evidential considerations are considerations that are potentially evident under appropriate circumstances: for example, they might be empirical but also
theological, aesthetic or self-evident. A consideration is relevant just in case it is taken by the community to favour an answer to the question over the others (Jardine 1991, chapter 3). For example, for Jardine a question that is not real in any community is:

Why is there something rather than nothing? (Jardine 1991, p.56)

On the other hand, he gives examples of questions that were real only in a given community:

Is the organic the image of the planet? (Jardine 1991, p. 56)

This is a question that the naturalist Lorenz Oken (1779-1851) addressed himself when he set out on his classificatory enterprise. Oken’s aim was to rethink the natural history by following the programme of Friedrich Schelling (1775-1854), who thought that nature realizes an ‘original ideal’ through the entire sequence of types of organization. There is no consideration that one can take in order to argue for a possible answer. By the same token, from Hacking’s account it follows that Euclid’s question about the entire class of prisms would be unreal for an ancient Egyptian: there is no consideration that she could have taken to answer questions about the properties of entire classes of geometrical figures.

In *The Social Construction of What?* (Hacking 1999b) Hacking espoused Jardine’s ideas: ‘[Jardine] shows, by means of compelling historical examples, how questions that make sense in one scientific framework are unintelligible in another’ (Hacking 1999b, p. 165). Moreover, Jardine gave examples of sentences to
which the comments I have made in the case of Paracelsus' sentence apply. For example, he quoted Oken's sentences such as:

The nose is the thorax repeated in the head grown (Jardine 1991, p. 64)

The organic must be a vesicle because it is the image of the planet
(Jardine 1991, p. 51)

Jardine explained that much of the rationale necessary to understand those sentences is not explicit in Oken's work. A modern reader would need to put Oken in the context of Schelling's views and then learn how the latter explained the relations between different forms of living beings in the light of an interplay of natural forces. Only then would Oken's ways of defending his claims come to light and the sentences above become intelligible.

4.4.2 Intelligibility versus Understanding

Both Hacking and Jardine use the term 'intelligibility' rather than other terms such as understanding. I think that this choice deserves a close examination. When the hearer comes to see the presuppositions of another community that has a different SoT she can recognize the truth-values imposed by the speaker. However, those presuppositions are not shared by her community, are not her presuppositions. As a consequence, (in Hacking's terminology) the style-dependent sentences of a different SoT have no truth-value, although they are in principle intelligible. Or, alternatively, in Jardine's terminology, one may say that the questions of different
cognitive standpoints can become intelligible, yet they remain unreal. So, what should we reserve the term 'understanding' for? Suppose that a speaker uttered Herschel's sentence:

The heat which has the refrangibility of the red rays is occasioned by the light of those rays

Hacking claims that, if the hearer were Archimedes, he would not be able to assign a truth-value to this sentence, even if she had learned Herschel's physics from the ground up. However, not any hearer who belongs to a community that has the laboratory SoT would grasp the meaning of this sentence straightforwardly. Yet, any hearer in the SoT of the Laboratory, the same as Herschel's, could well understand the sentence, provided that she has learned some rudiments of physics. Indeed, she does not need to perform any hermeneutic work in order to come to see presuppositions such as questions, methods, and standards of truth: these presuppositions are peculiar of her community and are 'real' for her– and I have used the verb 'to understand' to underline this very circumstance. To be clearer, understanding does not require an effort for gaining an insight into the presuppositions of a different SoT. Whereas Archimedes can only achieve intelligibility, any member of the SoT of the Laboratory can achieve understanding of Herschel's sentence.

It is important to bear in mind that, for us, understanding is in principle possible for all the style-dependent sentences of the six SoTs that I have presented in chapter two and three. Indeed, as I have explained, they have emerged and still
endure. That is to say that the presuppositions of those six SoTs, i.e. their criteria of evidence, methods, ways of reasoning and questions, are our presuppositions.

In his *Representing and Intervening*, Hacking wrote:

> There is no way to match what Paracelsus wanted to say against anything we want to say [...] we cannot assert or deny what is being said (Hacking 1983b, p. 71)

What Hacking means can be interpreted in the light of my discussion so far: Paracelsus’ sentences only ‘live’ in his community, that is, only when there exist the presuppositions by which they can be validated. By virtue of a hermeneutic attitude Paracelsus’ sentences do become intelligible in our community but, if we want to use them in order to argue, think and do we need to give up our own SoTs, i.e. our presuppositions: either we become members of Paracelsus’ community or we cannot say what we want to say by using Paracelsus’ sentences.

By the same token, it is not possible, for example, to defend a style-dependent sentence of the statistical SoT without relying on its presuppositions, e.g. the evidence of things and, say, the way of selecting a sample or dealing with errors. In other words, if a sentence such as

> The adult height for one sex in an ethnic group follows a normal distribution (section 2.2)

has to be used to express claims of knowledge it cannot be separated from the presuppositions of the statistical SoT by which it can be validated. This claim implicitly relies on a verificationist thesis: the sentence is not meaningful for a
community that has no methods to assess its truth. The methods and criteria of
evidence of, say, the taxonomic SoT cannot validate the sentence above – either we
switch from the taxonomic SoT to the statistical SoT or we are not able to make use
of the sentence in order to know and find out.

4.4.3 Projectibility

This point can be expressed in a different way. One of Kuhn’s ideas was that after
a change in paradigm scientists ‘live’ or ‘work’ – he used both of these verbs – in a
different world. In his paper *Working in a New World* (1993) Hacking used the term
‘projectible’ (introduced by the philosopher Nelson Goodman (1906-1998)) to
restate Kuhn’s thesis of untranslatability between different paradigms. He made it
clear that the scientific terms of an earlier paradigm might well be intelligible to
the members of the new paradigm, but the latter could no longer use these terms
projectibly. By this term he means:

a class of terms is [projectible when it is] used by a community for
making law-like statements, forming general conjectures, picking and
making things with expectations about what they will do and how they
will work (Hacking 1993b, p. 295).

For Hacking scientific names refer to scientific kinds in use by a paradigm and the
notion of projectibility express their untranslatability into those of another
paradigm. By analogy with Hacking’s use of the term ‘projectible’ I say that style-
dependent sentences are *not* projectible: they cannot be used to work and think in
a different SoT. Very importantly, while scientific terms are not projectible because of an issue of untranslatability between paradigms that use different scientific kinds, style-dependent sentences are not projectible because of a difference in presuppositions between different SoTs. A style-dependent sentence is not projectible into a different SoT because it needs the presuppositions of its own SoT to be meaningful: it would be impossible to form conjectures, think and know. Although in the case of Kuhn, projectibility is referred to untranslatability, Hacking’s use of the term ‘projectible’ for scientific terms is compatible with my extension to style-dependent sentences:

To call a term projectible is not to say that generalizations made with it [...] can be justified in the community. Projectibility defines the class of possibilities envisioned or capable of being taken seriously by a science at a time (Hacking 1993b, p. 296).

In other words, projectible terms are terms that can be used for forming statements that can be justified in a given community. Therefore, to study the projectibility of terms also includes the study of the possibility of justifying statements in a given community - just the possibility I have been discussing.

To sum up, what Hacking maintains can be couched in these terms: there are a number of presuppositions that must be known by a hearer in order to come to see the intention of the speaker of attributing truth-values to certain style-dependent sentences. The concept of intelligibility concerns the circumstance in which the speaker and the hearer have different presuppositions. The presuppositions of style-dependent sentences in the SoT of the speaker are
recognized only thanks to the hermeneutic attitude of the hearer, who has a
different SoT. The concept of understanding relates to the situation in which the
speaker and the hearer share the same presuppositions but the hearer needs to
acquire some background knowledge in order to grasp the meaning of the style-
dependent sentence. The society we live in adopts all the six SoTs I have presented
so that style-dependent sentences in those SoTs are in principle understandable by
us. The term presuppositions refers to criteria of evidence, methods, ways of
reasoning, questions. In other words, the presuppositions that need to be known
in order to attribute truth-values to a style-dependent sentences coincide with
some of the features of the SoTs they belong to. Ultimately, understanding has to
do with sentences that have truth-value in a given SoT; intelligibility has to do
with style-dependent sentences that lack truth-value according to the
presuppositions of a given community that has a certain SoT.

Hacking's SoTs' project also implies the impossibility of projecting style-
dependent sentences into another SoT. For example, certain sentences of
Paracelsus are not projectible into our SoTs: there is no way of justifying them
according to our standards; there is no possibility of doing things as he does
because that would be to hold Paracelsus' criteria of evidence. It would mean to
drop out of our community.

On the other hand, the style-independent sentences have truth-value in any
SoT. They are always meaningful because they rely on the evidence of senses,
which is not a presupposition of only one given SoT. Suppose that the following
declarative sentences were translated from Greek to ancient Egyptian or vice
versa:
Two sides of this trapezoid are parallel.

Any hearer would recognize the truth-value that the speaker has imposed on these sentences straightforwardly: their being true-or-false depends on the evidence of senses.

### 4.4.4 Hypothetical Styles of Thinking and Doing

In section 3.5 we have seen that an essay of Borges is mentioned by Foucault as an example of transgression of the boundaries of our rationality. The short stories of Borges present other sentences that are seemingly absurd. Take for example:

> No book can be a ladder, although no doubt there are books which discuss and negate and demonstrate this possibility (Borges 1997 [1941], p. 686).

This sentence appears in a note of the tale *The Library of Babel* in which Borges imagines a building composed of an indefinite number of hexagonal galleries. In each wall of each hexagon there are 32 books of identical size made of 410 pages; each page contains 40 lines composed of 80 letters. As each book is a possible combination of the 25 orthographic symbols, every conceivable book must exist in the Borges’ library. For example, Borges writes that the detailed history of the future or the autobiographies of the archangels can be found in the library.
Now, Borges' sentence just above suggests the existence of a book in which is argued the existence of another book that is a ladder. For example, the book might contain, beside a sentence such as 'a book can be a ladder', the sentence:

Sometimes a ladder behaves like a book

How should we classify this sentence? It is natural to say that it is just nonsense. Notice, however, that in the future there might emerge a SoT whose presuppositions give truth-value to it; or there might have existed an unknown SoT in which that sentence was understandable. We cannot put forward any hypothesis about the presuppositions that would attribute a sense to the sentence in an unknown way of thinking, that is, about the presuppositions that would make that sentence true-or-false. More importantly, it is difficult to imagine this hypothetical SoT as self-authenticating, i.e. to imagine that the sentence above is immune to refutation by our SoTs. However, in principle the theory of styles of thinking is open to this possibility. In other words, Hacking's account of SoTs implies that certain sentences that seem nonsense in our SoT can be in principle intelligible. The reason is that, for Hacking, when a SoT comes about it objectivizes new candidates for truth or falsehood. A new SoT might provide sense to some of all the possible sentences whose syntactical arrangement is correct.

In his fictional text Borges says that 'it was possible for a librarian of genius to discover the fundamental law of the library' (Borges 1997 [1941], p. 682) so that it was possible to decipher every book. A possible suggestion of this idea is that it is possible to make sense of every possible combination of the
orthographical symbols written in correct grammatical form in the books of the Library. If so, as a metaphor, the suggestion would reminiscent of the Kantian idea of rules intrinsic to the mind that structure the entire reality. The possibility of a universal law that makes sense of all the possible sentences is compatible with Kant’s philosophy. As a matter of fact he believed in ‘transcendental conditions’ that can be stated independently of history or other constraints imposed on us from outside. On the contrary, Hacking would reject Borges’ metaphor: for him there is no universal rule; the sentences in the books of Borges’ Library are affected by the arguments and the methods of inquiry that people have deployed in a certain historical context. This difference suggests that the project of SoTs consists of historicizing Kant, a point that I shall develop in the next chapter.
4.4.5 Taxonomy of Declarative Sentences: a Summary

In this section I have deduced a classification of declarative sentences in correct grammatical form. The rules by which I have built this taxonomy are entirely derived by Hacking’s claims in his SoTs project. From the point of view of a given SoT there are sentences that are understandable (in Hacking terminology: meaningful or sentences that have a truth-value), sentences that are intelligible and sentences that are nonsensical. Understandable sentences are either style-dependent sentences, that is, sentences that have a truth-value only within the SoT or style-independent sentences, which have a truth-value that does not hinge on a particular SoT. Intelligible sentences have a truth-value only by virtue of the presuppositions tacitly assumed by a community that has a different SoT. Nonsensical sentences are sentence that do not have truth-value in any SoTs -- they are nonsense. Given a nonsensical sentence, we could well find out that a past SoT has made it objective.

By the term intelligible, I mean that, in principle, it is possible to come to see the presuppositions on which the truth-value of the style-dependent sentences is based on. Methods for finding out, standards of evidence, ways of thinking, questions constitute the presuppositions that need to be known in order to recognize the conditions of truth that have been imposed on the sentence.

Presuppositions of a certain SoT are constituted by some of its features. In other words, an appreciation of the fundamental features of the SoT is necessary to obtain an insight into why a given style-dependent sentences is meaningful for a community of people. When this appreciation is gained, the style-dependent sentence become intelligible outside its SoT.
Intelligible sentences are not projectible: even if certain style-dependent sentences have become intelligible, one cannot use them in one’s own SoT in order to know – they cannot appear as part of a body of sentences that express claims of knowledge, conjectures, hypotheses or law-like statements of another community that has another SoT. Style-dependent sentences receive their sense, their being true-or-false only in their own SoT by virtue of the presuppositions of that very SoT. To use them in order to know and find out in another SoT would mean to give up being part of one’s own community and think, act and justify as the members of another community do. In my examples, this means that Laplace could have made Paracelsus’ sentences intelligible by studying his thought but he could not have acted, thought or justified as Paracelsus did, and vice versa. Similarly, an ancient Egyptian could have learned about the postulational methods and the standards of truth of geometry but she could have not thought and acted as Euclid did when he proved the propositions in his *Elements*.

4.5 Conclusions

In this chapter I have completed my characterization of scientific thought: I have argued that there are ways of thinking that differ from SoTs. What I consider the most relevant difference is that only SoTs have strong techniques of self-authentication, techniques that are intrinsic to their forms of reasoning. I have also discussed the claim that certain style-dependent sentences are not meaningful for a community that adopts a different SoT. The main upshot of this discussion has been that a style-dependent sentence is intelligible outside the SoT but it is not projectible.
Chapter five: The Concept of Style of Thinking and Doing

5.1 Introduction

The previous chapters provide a characterization of SoTs as well as examples of their use in different branches of scientific research. We are now ready to pursue the two main objectives of this chapter: to place the SoTs project in its philosophical perspective and to grasp fully the philosophical import of the concept of SoT. It will be a salient point of the first half of this chapter that the concept of SoT should be analysed within the context of an area of research called ‘historical epistemology’, which studies epistemological concepts as objects that evolve.

Since its introduction in the 1980s, the concept of SoT has gone through a process of evolution as Hacking reacted to the ideas of coeval thinkers such as Lorraine Daston and Peter Galison, who have been involved in similar projects in their careers, and Bernard Williams (1929-2003), who came from an entirely different area of philosophy. The ideas of these thinkers will be considered in the second part of this chapter, where I shall revisit the concept of SoT by considering different perspectives, suggesting analogies and drawing parallels.
More in detail, in the next three sections I shall present historical epistemology as an attempt to historicize Kant: while the latter found the conditions of experience in the universal structures of mind, historical epistemologists find them in certain historical conditions for possible discourse. In the next section, in particular, I shall explain that while for Hacking there have been distinct SoTs at different times, for Kant there is a fixed universal rationality common to human beings at all times, to be understood in terms of pure concepts. I shall also present historical epistemology and argue that the SoTs project can be understood as belonging to this kind of study. In section 5.4 I shall draw an ideal line linking the SoTs project, the project of historical epistemology and a tradition of philosophy of science born in France at the beginning of the twentieth-century.

In section 5.5 I shall present *Objectivity* (Daston and Galison 2007) as a work in historical epistemology. As Daston and Galison's analysis is mainly *historical*, it will serve as a test against which Hacking's conclusions can be assessed. Finally, a point that will be clear in the last section is that, on the basis of some ideas of Williams, each SoT can be viewed as a way of representing a single aspect of a certain portion of the world.

5.2 Hacking's Historical Epistemology

Few passages of Hacking can be more illuminating than the following one for those who want to understand the philosophical perspective of the SoTs project:
my study is a continuation of Kant's project of explaining why objectivity is possible. [...] Kant did not think of scientific reason as a historical and collective product. We do (Hacking 2002 [1992], p. 181).

In the first sentence of this passage Hacking espouses the Kantian idea that objectivity is made possible by certain *a priori* conditions, which he wants to identify and study, continuing Kant's project. In the second sentence, though, he makes a fundamental departure from Kantian philosophy by suggesting that what is objective is the result of historical and social circumstances.

In order to elaborate on this passage, I wish to remind the reader that the concept of objectivity occupies a pivotal role in the SoTs project: in Hacking's terminology, when in a community that adopts a given SoT certain sentences come to be 'up for grabs as true or false' (Hacking 2002 [1982] -a, p. 160), they become *objective*; and when concepts, questions, conjectures, problems, solutions come to be shared and discussed, in short, when they become 'possible', they become *objective*. Indeed, in his styles project Hacking often uses interchangeably qualifiers such as 'meaningful', 'true-or-false', 'objective'.

One of Kant's problems was to understand how judgements become possible: to determine what he called the 'transcendental conditions', i.e. the 'conditions of possibility' of our concepts. He believed he could state them independently of history or necessary constraints imposed on us from outside. Indeed, the Kantian transcendental conditions are related only to the structure of human thought - it is reason that imposes them on the raw sense data by making a set of sensations objective experience. In particular, the spatial and temporal
forms of intuition represent the conditions within which we perceive objects as existing in space and time and within which the propositions describing these objects have a truth-value. Since transcendental conditions are prior to experience, Kant referred to them as *a priori* conditions.

For Hacking, Kant's solution cannot be satisfying because he thinks that the truth-value of a sentence is fixed by contingent historical circumstances. He rejects the picture according to which certain propositions are not affected by the methods of inquiry that we deploy in a certain historical context: although the statements of the theory of probability might be true at any time, for Hacking they could not be uttered until the mid-seventeenth century, when certain conditions for their emergence came into being, e.g. the new 'evidence of things', a new way of reading signs in things. Therefore, for Hacking, Kant's claim that the conditions of experience can be found in the structure of human mind is to be rejected because it jars with the results of historical analysis -- if anything, these conditions must be impermanent. It is the very existence of a SoT that provides the conditions for the objectivity of statements, theories and debates.

It must be emphasized that Kant was engaged in a particular stage of scientific process, Newtonian science\(^{22}\), which for him explained the world of appearances. In order to justify the kind of necessary universal laws proposed by Newton he defended the intellectual structures underlying Newtonian mechanics: Euclidean geometry, continuous classical time, absolute space and the concepts of causality and substance. By contrast, Hacking set out to solve the problem of why objectivity is possible in the aftermath of the upheavals of quantum theory and the

\(^{22}\) For a comprehensive account of Kant's scientific sources see (Calinger 1979)
theories of relativity, which had resulted in a non-Newtonian conception of space, time and motion\textsuperscript{23}.

In the rest of this section, I shall argue that the context of the SoTs project is a field of research that Hacking has called 'historical meta-epistemology'. As many other scholars denote it as 'historical epistemology', without the prefix 'meta', henceforth I shall use the same label (or the acronym HE). In turn, in the next section I shall identify the philosophical roots of HE in a French approach to the history of the sciences that emerged at the beginning of the twentieth century.

The expression ‘historical meta-epistemology’ appeared in 1999 as title of a paper (Hacking 1999a) prepared for a conference in honour of the philosopher Lorenz Krüger. In that paper Hacking explained the meaning of that expression with these words:

\begin{quote}
[Historical meta-epistemology] is, first of all, ‘meta’ epistemology in that it talks about very general or organizing concepts that we use today, and which have to do with knowledge, belief, opinion, objectivity, detachment, proof, probability, argument, reason, rationality, evidence. It is not directly epistemology, in the sense of a theory of knowledge (etc.), but a study of ideas about or uses of knowledge (Hacking 1999a, p. 53).
\end{quote}

I am going to analyse this passage in order to explain what HE is. I shall start by clarifying what organizing concepts are.

\textsuperscript{23} The first chapters of \textit{Causality and Chance in Modern Physics} (Bohm 1984 [1957]) provide a clear exposition of why quantum mechanics brought about a renunciation of causality.
5.2.1 From Kantian Categories to Organising Concepts

A good start for a discussion about organizing concepts might be thinking of Kant’s categories, as Hacking expressly said (Hacking 1999a, p. 66). Indeed, Kant’s categories (and the two innate intuitions of space and time) lie at the top level of our conceptual organization. To such a degree that according to the *Critique of Pure Reason* (Kant 1996 [1781]), there could not be empirical concepts without organizing concepts such as a priori categories: the former are forged from the raw material of experience by applying the categories to intuitions.

Like the Kantian categories, the organizing concepts studied by HE (e.g. objectivity, knowledge and probability, mentioned in the passage above) ‘govern and control ground level concepts’ (Hacking 1999a, p. 65). For example, the concept of objectivity organizes a whole collection of other concepts, practices and values. Concepts such as standard of evidence, reliable method and valid argument, and the way in which they are related one to another, presuppose the meta-concept of objectivity. For example, in order to establish that a method is reliable it is necessary a standard of evidence, which in turn necessitates a cognition of what to be objective means. Ultimately, here objectivity plays a similar role to a Kantian pure concept: it is a priori because it is necessary for the existence and organization of other concepts. Similar considerations apply to other organizing concepts, generally absent in traditional epistemology, e.g. the concept of child development. According to Hacking, in the last 150 years ‘[the concept of child development] has come to determine in the most minute details how we organise our thinking about children’ (Hacking 1999a, p. 68). Notice that, as Kantian categories appear inescapable because timeless and universal, so organizing concepts seem inescapable because, as Hacking says,
they are [...] essential to the very functioning of our society. We are stuck with them, which is not to say [...] that they are not changing as I speak (Hacking 1999a, p. 65).

However, despite these analogies, Hacking points out a difference between organizing concepts and Kantian categories: the former are changeable, the latter are timeless. In particular:

Organising concepts [...] do not exist as a timeless built-in resource for all human beings. They are [...] situated. They change, evolve, undergo mutations, emerge out of new practices or radical transformations of old ones (Hacking 1999a)

For instance, the (organising) concept of probability emerged in the mid-nineteenth century and has changed – Hacking claims - in the way described in the central chapters of *The Emergence of Probability*. As an example of non-organizing concept Hacking considers the concept of horse: it is a concept ‘we can understand totally outside the history of any idea’ (Hacking 1999a, p. 59).

### 5.2.2 Historical Epistemology versus Traditional Epistemology

In the first passage of this section Hacking also says that HE is not a theory of knowledge. Indeed, traditional epistemology studies the conditions, the limits and the sources of knowledge and inquires into what makes knowledge scientific. It considers concepts such as proof, rationality, objectivity, evidence and the like as
permanent and, to understand them, it analyses the timeless structure of logic and consults human understanding.

HE starts from an opposite presupposition: even though organizing concepts are necessary for our thinking, they get their meaning from the uses we make of them in a certain period of history and the way in which they are connected with other concepts. They are ‘situated’: they ‘have no constitution other than tradition and use’ (Hacking 1999a, p. 56). Furthermore, HE cannot be assimilated to history of ideas either: the latter is generally centred on the subject and tells how key concepts are transmitted from a thinker to another. Historical epistemology, on the other hand, is an inquiry into the very general structures in which human beings think. In conclusion, unlike traditional epistemology, HE does not formulate or defend theories of knowledge; rather, it reflects on the historical conditions under which we know, that is on organising concepts.

In nuce, historical epistemologists make a double claim: 1) at a particular stage of history there will be a set of organising concepts that play a role similar to Kantian pure concepts in that they allow us to make judgements; 2) organising concepts are not permanent. By bearing in mind these two points historical epistemology can be viewed as the historicization of Kant. In other words, the qualifier ‘historical’ in the expression ‘historical epistemology’ alludes to a radical departure from Kant, to the historicization of his ‘pure concepts’.
5.2.3 The Styles Project as a Study in Historical Epistemology

Another important conclusion concerns the question as to whether the SoTs project can be viewed as a specific study in HE. Surprisingly, Hacking’s answer is in the negative:

Anyone who has come across some published sketches of ideas in progress [the SoTs project] might think this was an example of historical meta-epistemology: absolutely not! (Hacking 1999a, p. 72)

Rather, he interprets the SoTs project as ‘a way of saying something about language, truth and reason’ (Hacking 1999a, p. 72). On his part, Kusch’s answer is in the positive because ‘Hacking’s analysis historicises reason, historicises what counts as a scientific proposition, and historicises what passes for a scientific entity’ (Kusch 2010, p. 158). My answer is in the positive too for the following reason: both methods and results of the SoTs project are part of the methodological and conceptual apparatus of HE. Indeed, to start with, I recall from the previous section that Hacking declared that his ‘styles of reasoning [...] are part of what we need to understand what we mean by objectivity’ (Hacking 1992d, p. 181). In this sense, the scope of the theory of SoTs is identical to one of the objectives of HE – to understand a concept, objectivity, which undergoes mutations and evolves along with the changes of our practices. However, as I have tried to highlight in the previous section, it is the mutation of a ‘way of being objective’, e.g. the evidence of signs, which can make it possible the emergence of

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24 In general, Hacking combines the tradition of historical epistemology with the Anglo-American tradition of analytical philosophy (see for example Brenner 2006). However, the former is particularly relevant for the SoTs project as it will be clear in the next sections.
a new SoT. Therefore, the reverse of Hacking's sentence is also true: what we mean by objectivity, its study, is necessary to explain what a style of reasoning is. But for Hacking objectivity is to be studied using the methods of HE, so the latter is necessary for elaborating the theory of SoTs.

5.2.4 Other Projects in Historical Epistemology

Over the past few years HE has been used as a label for a wide variety of programs (for an overview see Feest and Sturm 2011). Among them it should be mentioned the work of Daston and Galison, who characterized epistemological categories out of knowledge practices. Daston characterized HE as 'the history of the categories that structure our thought, pattern our arguments and proofs, and certify our standards of explanation' (Daston 1994, p. 282) and cited Hacking and Arnold Davidson as the leading practitioners of HE (Daston 1994, p. 283). The latter, conducted investigations into the conceptual formation of new disciplines and mentioned historical epistemology in relation to a kind of study that attempts to show how novel forms of experience are linked to the emergence of new structures of knowledge (Davidson 2001, p. XIII).

Today HE has expanded beyond the study of organizing concepts.25 Historical epistemologists are also studying the objects of scientific practice and their history. Some of them are focused on the development and the dynamics of scientific knowledge. Another sub-area of research of HE aims to historicize epistemology by trying to answer to questions such as: 'does the historicity of

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25 The introduction to the conference entitled “What (good) is historical epistemology?” and held at the Max Plank Institute for the History of Science in Berlin, provides further details about aims and programs of historical epistemology (Sturm 2008)
some epistemic values force us to accept a relativistic outlook of science and history? (see for instance Kusch 2011c) ‘Are certain questions of traditional epistemology well posed?’.

5.3 The Roots of Hacking’s Historical Epistemology

In the previous section I have argued that the SoTs project fits into an approach to the study of knowledge called historical epistemology (HE). When the label ‘historical epistemology’ (Historische Epistemologie) appeared for the first time Hacking thought that it was inappropriate. For him that label was too full of reminiscences of a book by Dominique Lecourt on Gaston Bachelard (1884-1962) entitled L’Epistemologie historique de Gaston Bachelard (Lecourt 1969). He meant that the expression ‘historical epistemology’ already indicated a tradition in philosophy of science whose main protagonists were Gaston Bachelard and Georges Canguilhem (1904-1995) (Hacking 1999a, p. 54). The prefix ‘meta’ he added wanted to mark a distinction with that prior use: ‘for Bachelard objects are the sciences in their historical development [...] our objects of study are different, not knowledges, but ideas about knowledge’ (Hacking 1999a, pp. 54-55).

However, other practitioners of HE did not follow Hacking in the use of the prefix ‘meta’ and continued to use the label ‘historical epistemology’.

Consequently, today there is some ambiguity in its use. ‘Historical epistemology’ indicates both the variety of projects mentioned in the previous section and an

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26 The label appeared in a correspondence about the establishment of the Max Planck-Institute in Berlin in the first years of 1990s (Hacking 1999a, p. 54).
older tradition of thought in philosophy of science born in France in the beginning of twentieth-century, when different thinkers started to reflect on the historical conditions of knowledge. To avoid confusion, I shall follow Cristina Chimisso who called the latter ‘classical historical epistemology’ (henceforth CHE) in order to distinguish it from HE (Chimisso, unpublished).

In this section I want to show that the underlying assumptions of the SoTs project can be traced back to classical historical epistemology (CHE). It will be my contention that CHE, which represented a shift of interest in theory of knowledge previously focused on finding the correct method of science, continues until today under various forms.

5.3.1 Origins of Historicization of Epistemology

Thomas Kuhn’s *The Structure of Scientific Revolutions* (Kuhn 1996 [1962]) is widely presented as the book that produced a decisive transformation in the predominant image of science as exhibiting progress and methodological unity. However, already at the beginning of the twentieth century the attempt to identify the credentials of scientific knowledge was set aside and a space for a historicization of epistemology was opened up. It had become clear that the scientists’ practices do not follow a timeless rule but are subject to a historical development and that the way in which human beings reason changes over history. Much later, fuelled by various scientific and philosophical developments, this philosophical attitude led to a historicization of all the categories associated with science.

Recently, several scholars have identified different pivotal points of history in which classical epistemology came to be under attack. Rheinberger emphasized the importance of Emil Du Bois-Reymond (1818-1896), working as an electro-
physiologist in Berlin: by claiming that the science of mechanics could not account for its basic concepts – matter, force and movement - he called into question the mechanistic conception of nature and therefore the unity itself of science (Rheinberger 2010, pp. 5-8). Gutting focused on the French philosophers Léon Brunschvicg and Gaston Bachelard and only mentioned *en passant* that the French philosopher August Comte, and later Pierre Duhem (1861-1916) and Henri Poincaré (1854-1912), started a tradition that insisted on understanding science through its historical development (Gutting 2005). Chimisso, in her *Writing the History of the Mind* (Chimisso 2008) presented a set of projects whose common presupposition was that ‘the mind could not be studied *a priori*, and that ways of thinking were different in different civilizations’ (Chimisso 2008, p. 3). For her, Gaston Bachelard and Georges Canguilhem represent the acme of a French tradition of thought that had already started to historicize epistemology and of which the philosophers and professors at the Sorbonne Lucien Lévy-Bruhl (1857-1939) and Léon Brunschvicg (1869-1944) and the historian Alexandre Koyré (1892-1964) were illustrious exponents (see for example (Lévy-Bruhl, 1992 [1910]) (Brunschvicg, 1912) (Brunschvicg, 1934) (Koyré,1958 [1957])). Although this tradition did not ignore the lesson of previous positivist thinkers such as Comte, it ‘took history more seriously, in some cases extremely more seriously’ than they did, as it is shown by Metzger’s complain that for Comte ‘historical events are chosen and interpreted to illustrate a theory, rather than being a basis for the theory’ (Chimisso 2010, p.44). Moreover, Chimisso considered Lévy-Bruhl and Brunschvicg the ‘fathers’ of CHE: the latter inherited their idea that human mind changes across different times or cultures (Chimisso, unpublished p. 3).
Lévy-Bruhl argued that certain traditional non-literate societies thought in a different way from modern people, e.g., they adopted a form of explanation based on supernatural causes, that is, on primary rather than secondary causes. Brunschvicg, on his part, struck by the philosophical implications of the Relativity Theory, argued that Kant had in fact presented a particular way of thinking, of which Newton’s science was the product. In parenthesis, the belief that categories and certain laws and values are timeless and universal had already been the subject of Hegel’s criticism. But Brunschvicg, like other French historians of philosophy at the Sorbonne, rejected the idea of laws of development in the history of thought (Chimisso 2008, p. 38-42).

We come to see that it was with Lévy-Bruhl and Brunschvicg that that kind of historicization of Kant, of which Hacking presented his own version, started. Lévy-Bruhl had argued that the categories of thought change in different cultures; Brunschvicg had historicized the Kantian categories; Hacking historicizes standards of evidence, way of reasoning and doing and spells out the conditions of possibility of certain sentences.

5.3.2 Origins of the Idea of Discontinuity
In the tradition of Brunschvicg, Gaston Bachelard (1884-1962) thought that scientific knowledge and common sense differ radically because they are produced by two distinct ways of thinking. In order scientific knowledge to emerge, it is necessary an ‘epistemological break’ with immediate experience that is rooted in imagination and emotions. Within scientific thought there have also been epistemological breaks, which have produced new ways of thinking: the
lesson of Quantum Theory and Relativity, which had overthrown the old theories of classical physics, was strongly present to his mind.

With respect to discontinuity, Hacking took Bachelard’s tack by arguing for sharp breaks in the history of science. He himself paid obeisance to Bachelard by saying:

I have a revolutionary temper, perhaps under the excessive influence of Bachelard, Kuhn and Foucault [...] I am interested in ruptures that radically transform our methods of reasoning (Hacking, 2006 -e p.9)

However, the reasons that underlie the two ‘revolutionary’ attitudes are different and not simply due to the distinctive character of Hacking’s notion of SoT. While for Hacking SoTs accumulate, for Bachelard there is no monotonic increase of knowledge: at certain points of history radical revisions of the conceptual foundations of scientific knowledge can happen, although the past can be transformed rather than completely rejected. For example, in the beginning of the twentieth century, the theory of relativity emerged with the consequent rejection of all the basic concepts of classical physics. For him, epistemological breaks like this were the consequence of a new ‘scientific mind’ that had emerged superseding the ‘old Newtonian mind’, which therefore was not eternal and universal.

In Bachelard’s thought ancient ways of thinking now gone extinct such as the alchemic way of thinking are non-scientific – he distinguishes three stages of historical development: prescientific, scientific and the phase of the new scientific mind, which started with the appearance of the Relativity theory. His
philosophical attitude was normative: he used modern science as a standard in order to judge certain ways of thinking as 'non scientific'. Conversely, Hacking's historical epistemology is not normative – Hacking is reluctant to saying that certain ways of thinking are non-scientific, e.g. he did not put forward any criterion to discard Paracelsus' way of thinking as non-scientific. As we shall see below, the kind of discontinuity proposed by Hacking shows a more direct lineage to Foucault.

5.3.3 Origins of the Historicization of Objectivity
In classical epistemology the knower had direct access to nature; Bachelard complicated this subject-object relationship – for him ‘the production of knowledge is in a fundamental sense instrumentally mediated’ (Rheinberger 2010, p. 24). Phenomena are not ‘just there’, waiting for being discovered, but must be isolated, purified and investigated through instruments, projected and calibrated by many people according to certain rules, which must be discussed and negotiated. As a consequence, for Bachelard objectivity becomes not something given once and for all but something that 'is produced in a process of objectification, the result of a double instruction – of phenomena and of minds' (Rheinberger 2010, p. 24). In other words, the phenomena are the result of the interaction between the world, a subject and a context; as a consequence, objectivity is established each time in relation to the growing precision of instruments and the negotiations between experts. By 'put[ting] the accent on the interface between science, technology, social structure of scientific community and the importance of scientific negotiable policies' (Castelão 2010, p. 53 my translation) Bachelard opened up the way for accepting the idea of a concept of
objectivity that is variable in the course of history. Hacking developed his own way this aspect of Bachelard’s philosophy: he put the accent on the instrumentally mediated production of knowledge through his idea that human ways of knowing must be understood as ways of thinking and doing. Besides, he argued that standards of evidence are not only the result of the development of technology but also of the emergence of new SoTs. In this respect, his theory of SoTs is a step further with the respect to Bachelard’s view of objectivity: it is a theory that describes how and why it changes. Canguilhem, who succeeded Bachelard as Sorbonne professor, wrote: ‘the object of historical discourse is, in effect, the historicity of scientific discourse’ (Canguilhem 2005, p. 203) If Bachelard’s view represents the first step for a theory of SoTs, Canguilhem’s represents the second step: to identify particular scientific discourses, examine their criteria of evidence and methods so to draw a lesson about how they change.

5.3.4 Thought styles versus Styles of Thinking

In the 1930s the Polish microbiologist Ludwik Fleck (1896-1961) put forward some ideas that showed a striking similarity to those that were emerging in the same period in France: in one of the first chapters of The Genesis and Development of a Scientific Fact (Fleck 1979 [1935]), he wrote that epistemology without historical investigations is an empty play on words27 (Fleck 1979 [1935], p. 21). His ideas sharply contrasted with the ‘outside time and history’ investigation of scientific activities conducted by the logical empiricist movement (see Fleck 1979 [1935], p. 50). Perhaps for this reason he did not catch the attention of other scholars until

27 Fleck cited and discussed Lévy-Bruhl in his Genesis and Development of a Scientific Fact (Fleck 1979 [1935], pp. 46-51)
Kuhn rediscovered his work and mentioned it in the preface of *The Structure of Scientific Revolutions* (Kuhn 1962 p. VIII-IX).

Fleck introduced the concept of *thought collective* (*Denkkollektiv*), a specific interactive community in which scientific knowledge is produced, and the concept of *thought style* (*Denkstil*), the distinctive mode of thinking of a certain thought collective. In the foreword of 1976 to *Genesis and Development of a Scientific Fact* Kuhn wrote that ‘what the thought collective supplies its members is somehow like the Kantian categories’ (Fleck 1979 [1935], p.XI): a thought style constrains, inhibits, influences the way of thinking and even the modes of perception. Kuhn’s mention of Kant is another fact that suggests that Fleck’s work could be viewed as another project in the current of ideas of CHE. Actually, besides important differences, there are many analogies between the notion of thought style and that of style of thinking, as I am going to show.

For Fleck,

[a thought style] is characterized by common features in the problem of interest to a thought collective, by the judgement which the thought collective considers evident, and by the methods which it applies as a means of cognition (Fleck 1979 [1935], p. 98).

In addition, thought styles are maintained by certain communities whose individuals share the same common knowledge and have the same communicative behaviour and literary style. The SoTs project proposes no equivalent of the notion of thought collective and the idea of a community is rarely explicit. Communicative behaviour and literary styles are important aspects
of the notion of thought collective that have been inherited by social
constructionism and are not present in Hacking: for example, Shapin and Schaffer
describe the literary technologies of Boyle’s community (Shapin and Schaffer 1989
[1985]-a, chapter II). It is also to be noted that SoTs are ways of doing and finding
out whereas practical aspects of cognition are less important in Fleck’s thought.
Furthermore, for Fleck

[there are] professional and semi-professional thought communities in
commerce, military, sports, arts, politics, fashion, science and religion’
(Fleck 1979 [1935], p. 107).

On the contrary, for Hacking the notion of style must capture the idea of
communities of people that share the same standard of evidence and adopt a
method of reasoning that is self-authenticating. Ultimately, the comparison so far
suggests that Fleck’s notion is more sociological, Hacking’s is more
epistemological.

Another interesting contrast is that the formation of a thought style is
ascribable to the change in meaning that emerges from a continuous
communication between the members of a thought collective. This process of
development is continuous\textsuperscript{28}: ‘In the development of ideas, primitive pre-ideas
often lead continuously to modern scientific concepts’ (Fleck 1979 [1935], p. 100).
Contrary to this picture of continuity, Hacking emphasized sudden breaks in
history of thought: rather than the slow and continuous transformations of
thoughts that pass from an individual to another, what might causes the

\textsuperscript{28} Nicola Mößner supports my point (Mößner 2011, p. 7)
emergence of a SoT is, for example, a rapid change in the kind of evidence that people use. In brief, Fleck’s thought styles emerge from communicative interactions, mute continuously and die out in a brief span of time. Hacking’s SoTs have clear beginnings and persist for a long period -- they are a matter of longue-
durée. SoTs inherit from Crombie the function of organizing the history of science; thought styles have a sociological substance. One of the reasons why SoTs are more enduring than thought styles – Hacking himself noted - is that ‘they are built on fundamental cognitive capacities’ (Hacking 2009, p. 27), although these capacities might have been tapped in different ways had the historical circumstances been different.

It is also worth noting that Fleck, before Hacking, had already used Paracelsus’ way of reasoning as an example of thought style. Commenting on a passage of the latter he characterized Paracelsus’ way of thinking in terms identical to Hacking (see Fleck 1979 [1935], p. 126). Compare:

[Paracelsus’] style of reasoning is alien (Hacking 1983b, p. 71)

Comprehending objects and phenomena in a way similar to our own was completely alien to [Paracelsus’s] way of thinking (Fleck 1979 [1935], p. 127).

The fact that Fleck considered Paracelsus’s way of thinking a thought style should make us doubt that it can also be considered a SoT, given the differences between the two notions. Indeed, I had argued in section 4.2.5 that it differs substantially from the six SoTs I have presented.
Fleck asserted that direct communication between the adherents of different thought styles is impossible (Fleck 1979 [1935], p. 36) and justified this point both in terms of impossibility of translating certain terms from a thought style into another and in these terms:

The statement ‘Someone recognizes something’ [...] is no more meaningful as it stands than the statements ‘This book is larger’ [...] . Something is still missing, namely the addition, ‘than that book’, to the statement. Analogously, the statement ‘Someone recognizes something’ demands some such supplements as ‘on the basis of a certain fund of knowledge’ or, better, [...] ‘in a particular thought style’ (Fleck 1979 [1935], p. 38).

I have made a similar point in section 4.4 when I have said that certain sentences are intelligible but not understandable for a member of a community that adopts a different SoT. In the theory of SoTs what we need to understand a sentence are the presuppositions of the SoT; for Fleck what we need is the ‘fund of knowledge’ of the thought style, the shared knowledge of the thought collective. However, Fleck made other strong claims that are absent in the theory of SoTs. He thinks that the perception of an individual changes when it is embedded in a certain thought style (or ‘paradigm’ in Kuhn’s version of the same claim). Even facts are created by thought styles; truth is a function of the particular thought style that has been accepted – there is no objective truth. Not only does Hacking, as we shall see, reject these claims but also insist that sense-datum statements are represented by style-independent propositions.
5.3.5 From Foucault’s Archaeology to Hacking’s Historical Epistemology

As I have explained, there are aspects of the notion of SoT that cannot be put in relation with the notion of thought style. One of the reasons of these differences is ascribable to the fact that Hacking’s way of tackling the problem of identifying the \textit{a priori} conditions of objectivity has a more direct link with the methods of analysis of Foucault. With the latter CHE reached a new stage of its reflection on the history of the sciences: Foucault’s notion of a ‘historical a priori ’ can be viewed as another transient substitute of the timeless Kantian conditions of possible experience: it is constitutive with respect to new kinds of propositions coming into being as objective. In Foucault’s words:

> What I mean by the term [historical a priori] is an \textit{a priori} that is not a condition of validity for judgements, but a condition of reality for statements. It is not a question of rediscovering what might legitimize an assertion, but of freeing the conditions of emergence of statements (Foucault 1969 [1972], p. 127).

As I have already mentioned, in \textit{The Order of Things} Foucault used the term \textit{episteme} to mean the historical \textit{a priori} as referred to scientific forms of knowledge. In a certain epoch, an episteme is a mode of thought whose system of rules operates under the surface of our consciousness and determines the boundaries of what can be thought and therefore made sense of. There is no continuity between
an episteme and another and it is the task of ‘archaeology’ (Foucault’s historiographic method) to dig into the thought of a certain epoch to identify and describe its episteme. In emphasizing discontinuity Foucault linked up with the studies of Bachelard and Canguilhem and their heterogeneous vision of history of the sciences.

I wish to set out three reasons why it is possible to say that Hacking developed the notion of SoT by adopting Foucault’s perspective. First of all, Hacking considered the notion of SoT as a particular element of the episteme, the 

*decisive* element that is necessary to understand the emergence of new true-or-false sentences. As he put it:

> I do not say that ‘style of reasoning’ means Foucaultian ‘episteme’ or whatever. On the contrary, if one could adequately define an episteme, one would surely have to include, as one of its elements, the styles of reason that bear on the positive propositions of that field of knowledge (Hacking 2002, p. 181).

In other words, for Hacking, of all the elements that characterize an episteme, e.g. assumptions, unspoken truths, institutions, knowledges, social activities, the material traces left behind by a particular epoch, what really counts in order to understand the emergence of new true-or-false statements is the style of thinking. Therefore, Hacking works in Foucault’s perspective in that he aims to discover the conditions of validity of statements by using what might be considered an element of the episteme.

Secondly, for Foucault
Archaeology does not seek to rediscover the continuous, insensible transition that relates discourses, on a gentle slope, to what precedes them, surrounds them, or follows them [...] On the contrary, its problem is to define discourses in their specificity; to show in what way the set of rules that they put into operation is irreducible to any another (Foucault 1969 [1972], p. 139)

In a similar way, the SoTs project is 'archaeological' in nature. It is not interested in those transitional zones between the phase that precedes the emergence of a SoT and its crystallization, for example in describing the continuous stages of refinements undergone by certain methods, instruments and practices. Rather, what counts is to discover the standards of truth, the questions, the criteria of evidence, the way of thinking and doing, i.e. the presuppositions that characterize the new SoT; and to do so it might be necessary to set up the presuppositions of a new SoT against those of others.

Finally, James Elwick highlighted the importance of possibility in the works of Hacking: 'Hacking's style of reasoning and Foucault's episteme are both conditions that make possible phenomena such as positive statements, new sciences and concepts' (Elwick 2012, p. 4). Furthermore, Elwick observed that certain sciences, new evidence, objects, sentences are made possible by SoTs and, in turn, SoTs are made possible by certain historical events. Therefore the model of layered conditions of possibility (stratigraphical model) can be applied not only to Foucault but also to Hacking. Following Elwick the SoTs project can be viewed as an analysis of different strata of possibilities: the first stratum made of contingent
I want to conclude by highlighting a difference between the notion of SoT and that of episteme: epistemes come and go, while a SoT never nudges out another SoT. Hacking wrote that complains have been addressed to Foucault because he never explained why epistemes die out (Hacking 2002, p. 195). Unlike the notion of episteme, Hacking’s notion is described, among other things, in terms of the concept of self-vindication. The existence of techniques of self-vindication, especially when they are divided into internal and external, supply part of the answer to the question as to why SoTs do not vanish (see also section 9.3). Why the Renaissance episteme of resemblance expired is a question that has no answer within Foucault’s intellectual framework. On the contrary, why the Renaissance way of thinking of resemblance did not endure is a question that admits partial answers in terms of the absence of internal techniques of self-vindication, as I have explained in subsection 4.2.2.
5.4 Hacking's and Daston and Galison's Conceptions of Objectivity: a Comparison

My brief sketch of classical historical epistemology was intended to shed light on the concept of SoT. This is, however, only part of the analysis we need. It is also essential to examine further the connection between the notion of SoT and the concept of objectivity. I shall pursue this aim by comparing the SoTs project with Daston and Galison's study on objectivity. This choice is natural: on the one hand the SoTs project implies the existence of different 'objectivities', since every SoT generates its own sphere of objectivity, a space in which some sentences are candidates for truth or falsehood; on the other hand Daston and Galison's book *Objectivity* (Daston and Galison 2007) charts the history of nature of new conceptions of objectivity that emerged in the last three centuries. Are the conclusions of these two studies compatible? In order to address this question I shall start by outlining some conclusions of the book *Objectivity* interesting for my purposes.

5.4.1 Various Conceptions of Objectivity

Daston and Galison based their study on the analysis of scientific atlases from different fields of research. Atlases are 'those select collections of images that identify a discipline's most significant objects of inquiry' (Daston and Galison 2007, p. 17). Daston and Galison think that their centrality comes from the fact that they 'set standards for how phenomena are to be seen and depicted' (Daston and
Galison 2007, p. 19). They analysed atlases in order to gain insights into three conceptions of objectivity that came about – they argue - in the last three centuries: truth-to-nature, mechanical objectivity, and trained judgement. Truth-to-nature emerged in the eighteenth century when scientists started to be afraid of being overwhelmed by the chaos of sensations and the intricacy of natural phenomena: too many non-essential details could hinder the knower from describing properly a phenomenon. Lisa Jardine described some technological factors that became catalysts for scientific advance in the seventeenth century: enhanced microscopic observation, chemical substance analysis, autopsy, camera obscura (Jardine 2000, chapter three). What her account reveals is how these new techniques produced a blinding intricacy of details that made it easy to fail to see the whole picture of a phenomenon. As a reaction to all that, Daston and Galison argue, scientists appealed to reason in order to find the hidden order behind the monstrosity of nature; they trained themselves to prune, shape, select and represent things in such a way that only what was typical of a certain class of objects could be recognizable. For example, eighteenth century atlases show images of ideal types that mean to illustrate the fully developed features of objects and be truer to nature than a specimen (Daston and Galison 2007, p. 56).

In the first decades of the nineteenth century, scientists started to criticize the artistic portrayals and praise the techniques of photography that were emerging. They ceased being concerned about being overwhelmed by nature and started to fear to project their theories into nature, deforming it. As a consequence, they tried to repress their predisposition to idealizing, pruning and perfecting. For example, Daston and Galison quoted the Berlin bacteriologist Carl Fraenkel as
saying:

A drawing can only be the expression of a subjective perception and therefore must, from the beginning, renounce the possibility of an objection-free reliability. [...] The photographic plate, by contrast, reflects things with an inflexible objectivity as they really are (Daston and Galison 2007, p. 177).

Daston and Galison considered objectivity as a ‘passionate commitment to suppress the will, a drive to let the visible world emerge on the page without intervention’ (Daston and Galison 2007, p. 143) as the manifestation of another scientific ideal that they called mechanical objectivity.

In the twentieth century mechanical images became complex and needed to be interpreted and compared. According to Daston and Galison many scientists began to question the current way of representing (and knowing) nature: they regarded certain images as unreliable because of the abundance of incidental details and artefacts. As an example of the inadequacy of mechanical objectivity, as perceived by scientists, consider that the radiologist Rudolf Grashey (1876-1950) wanted to classify x-ray images of human organs and bones in order to distinguish the normal, with all its variations, from the pathological. The mechanical registration of images could not be the solution: he was forced to decide which of them represented deviations within normality and choose some of them as a boundary post of the normal (Daston and Galison 2007, p. 344). In short, the insistent drive to repress the wilful intervention of the scientist did not prove adequate to answering the need of interpreting, highlighting, comparing
and handling images. In doing research, the images that any machine could produce were not sufficient to understand phenomena; in communicating and teaching science, some representations uncontaminated by human intervention were of no help to explain what was to be considered as pathological in a body or to distinguish a new particle among the tracks of myriads of decay particles. For example, in the *Atlas of Electroencephalography* (1941), the neurologists Frederic A. Gibbs and Erna L. Gibbs declared that their book had been written ‘to train the [reader’s] eye so that he can arrive at diagnoses from subjective criteria’ (Daston and Galison 2007, p. 321). In many cases a subjective decision was a necessary supplement to mechanical images or the mathematical techniques. Daston and Galison called this supplement *training judgement*. The word judgement stands for the ability of the researcher to make decisions without following standard procedures; it is a training judgement because it is a kind of perception one can be trained for.

Lastly, in the twentieth century, fidelity to nature gave way to an engineering-inspired way of knowing in which making, manipulating or simulating, more than depicting, are considered the best method to gain insight into the phenomena. In *Objectivity* Daston and Galison also mentioned another form of objectivity, *structural objectivity*, which counters the hermetic privacy of the self: the visual was to be distrusted and only relationships and structures were to be relied upon (Daston and Galison 2007, pp. 253-307).

On her own Daston also introduced the concept of *communitarian objectivity*: it emerged in the nineteenth century and ‘demanded the equally severe curtailment of individual and/or local autonomy in choice of instruments,
methods, and even research topics, in the name of solidarity’ (Daston 1999, p. 86).
We can conjecture that there are also other forms of objectivity, which imply the restraint of emotions or the belief that underlying the probabilistic nature of the universe there is a deeper reality independent of the observer.

5.4.2 Styles of Thinking, Epistemic Virtues, Objectivity

A way of looking at the work of both Hacking and Daston and Galison is to note that, like other previous thinkers of classical historical epistemology, they address a particular question, i.e. the question as to what changes in the course of the history of the sciences. So, for them, what is it that changes? A partial answer would be that for all of them what changes are the concepts of objectivity that human beings possess, i.e. what to be objective means at a certain point in time. However, the question requires a less superficial answer. Addressing it will lead us to appreciate both the substantial differences and similarities between Hacking’s and Daston and Galison’s historical epistemologies. Let us focus on Hacking first. He maintains that SoTs are a priori conditions, i.e. they are ‘prior to the concept of objectivity’: each SoT defines its own norms and in so doing makes certain sentences objective. Therefore, the concept of objectivity Hacking has in mind concerns the meaningfulness of sentences, their being uttered and shared. In other words, in Hacking ‘objectivity’ refers to the fact that a community of people share the presuppositions of its SoTs (in particular, the standards of evidence). However, it is a consequence of the fact that there are diverse SoTs (and different
communities of people that may not share the same SoTs) that there are also
different ways of understanding objectivity: for a community being objective
might mean to use the algorithmic method; for another using it in addition to the
method of mathematical proof. In sum, in Hacking there are different
‘objectivities’ because there are different SoTs, which are ‘prior to them’.
Moreover, Hacking also maintains that objectivity is an organising concept.
Therefore we can conclude that, once a certain community of people adopts one or
more SoTs, a certain form of objectivity is in place, and this form of objectivity is a
concept that organises the thinking and the system of values of the members of
that very community. Now, since SoTs are ‘prior to the concept of objectivity’, can
we say that the concept of SoT is an organizing concept too? I would refrain from
using this qualifier. An organising concept – in Hacking’s characterization – is an
inescapable concept, a concept that is necessary for the functioning of our
community and our life (Hacking 1999a, p. 65). In this sense the concept of SoT is
not inescapable, although in chapter three and four I have used it to organise our
scientific thinking and the different forms of objectivity that have emerged in the
history of the sciences.

Let us now turn to Daston and Galison. At first it would seem that they
prise the concept of objectivity away from any other notion. According to this
misreading, they would limit themselves to identifying different ontological,
epistemological, methodological or moral senses of objectivity and to tracing their
genesis and development in scientific practices. Actually, as I am going to explain,
each conception of objectivity is rooted in an epistemic virtue, as Daston and
Galison call the scientific ideal to which scientists are committed in a particular period or circumstance. Indeed, consider first that for Daston and Galison

scientific objectivity always counters some aspect of the self, but not always the same one. This is why the genus objectivity embraces the species of both mechanical and structural objectivity—and no doubt potentially others as well. Because the subjectivity is multifarious, objectivity must be too (Dear et al. 2012, p. 31).

For example, mechanical objectivity is the expression of the fear of one’s subjectivity (Daston and Galison 1992, p. 83): it opposes the post-Kantian unified self organized around the will (Daston and Galison 2007, p. 199). On the other hand, the enemies of truth-to-nature and trained judgment are external, Daston and Galison argue: the overwhelming wealth of irrelevant variations of nature for the former and the clutter of incidental details and artefacts introduced by mechanical objectivity for the latter. The battle is fought by what Daston and Galison depict as different prototypical knowers of nature, which in turn correspond to the different epistemic virtues: the ‘insightful sage’ (truth-to-nature), the ‘diligent worker’ (mechanical objectivity), the ‘trained expert’ (trained judgement) (Daston 1999, pp. 98-100) (Daston and Galison 2007, pp. 216-233).

These scientific personas cultivate particular traits of the self and pursue different values at the expense of others. Epistemic virtue is therefore the term with ethical overtones that Daston and Galison use to describe the values pursued by scientists in the battle for knowledge against powerful enemies such as those just mentioned. In this sense truth to nature, mechanical objectivity, trained judgement
can be seen either as epistemic virtues or as different forms of objectivity.

Although the epistemic virtues oppose different sorts of subjectivities, they all serve a common ideal, that of being faithful to nature. So, one can say that Daston and Galison present a history of the ways of being faithful to nature, a point that I will develop in the next section from a different perspective. In conclusion, there is a plurality of epistemic virtues or, alternatively, different forms of objectivity, which oppose different sorts of subjectivities and serve the common ideal of being faithful to nature.

I wish to insist on the fact that in Daston and Galison the concept of epistemic virtue is inseparable from the concept of objectivity: there is an epistemic virtue for each form of objectivity. It cannot be said that the concept of epistemic virtue is prior to the concept of objectivity, i.e. that it makes objectivity possible just as the concept of SoT makes objectivity possible. Indeed, the concept of epistemic virtue is simply the other side of the concept of objectivity or, rectius, each epistemic virtue represents the other side of a conception of objectivity: as Daston and Galison say each conception of objectivity is ‘rooted’ in an epistemic virtue or, equally, in an epistemic fear (Daston and Galison 2008, p. 671).

Consequently, unlike the concept of SoT, the concept of epistemic virtue is an organizing concept: each epistemic virtue (i.e. each conception of objectivity) organises the thinking of scientists concerning their system of values in scientific research or how to perform experiments and interpret their result. In general, by creating relations of significance, epistemic values direct research into certain directions, as Martin Carrier illustrated (Carrier 2012).

It is unfortunate that Daston and Galison’s study is limited to scientif
atlases and the SoTs introduced by Hacking have not been examined in order to identify the epistemic virtues involved. Although Hacking, Daston and Galison agree on the existence of a plurality of 'objectivities', it is still an open question whether or not all the SoTs represent forms of objectivities that Daston and Galison would trace back to epistemic virtues.

5.4.3 Styles of Thinking and Epistemic Virtues: Some Substantial Convergences

As a further point, according to Daston and Galison, the discovery of photography was crucial for the emergence of truth-to-nature. Epistemic virtues, therefore, like SoTs, are made possible by contingent reasons; then they can conflict each other or intertwine, and, in principle, vanish; when a new epistemic virtue comes into being, the old ones do not necessarily pass away. Here, the resemblances between the notion of epistemic virtue and that of SoT are striking. Furthermore, Daston and Galison say that

the atlas is always - and fundamentally- an exemplary form of collective empiricism: [...] Atlas users become the people of a book, which teaches them how to make sense of their sliver-world and how to communicate with one another about it (Daston and Galison 2007, p. 27).

It is a sliver-world because, as matter of fact, atlases select what is important to ask, see, depict, teach and learn so that they exclude portions of the world, questions
and perspectives. This may mean that for the members of a community who pursue certain epistemic virtues some aspects of phenomena (those that have been excluded) cannot play any role in the evidential considerations that bear on their knowledge. For example, the physicist Arthur Worthington (1852-1916), by relying only on his eyes, made symmetrical drawings of the impact of drops of liquids falling vertically on a surface of water (Daston and Galison 2007, pp. 11-16). In other words, Worthington discarded some features of the phenomenon in the name of the ideal of symmetry. Later on, after having succeeded in stopping a droplet’s splash with a photograph, which showed that the phenomenon was anything but symmetrical, he converted to the ‘objective view’, i.e. to the epistemic virtue of mechanical objectivity. Arguably, under the new possibilities of a ‘mechanical objective vision’, those variations that had been discarded under the ‘truth-to-nature vision’, could have become the evidence for more accurate theories that describe the splash of a drop. Ultimately, epistemic virtues select end exclude portions of the world and shape what can and what cannot be considered as ‘evidence’. Similarly, SoTs select and exclude, opening a specific space of possibilities and relegating to the oblivion questions, concepts and kinds of evidence. A case in point is the historico-genetic SoT: the community of scientists that adopted it turned their eyes to a previously unnoticed portion of the world that became the evidence for their new theories: fossils, minerals, rocks and geological layers.

Nevertheless, the role played by scientific atlases is not only limited to selecting and excluding. By reminding her readers that Foucault described the museum in terms of the difference between objects and concepts, Beth Lord
argued that museums do not only display objects but also systems of representations (Lord 2006). What is true of museums is much truer of atlases: they are representations of phenomena. More importantly, atlases reveal the contingences and the discontinuities of human conceptual schemes. As spaces of representations, atlases are the tangible historicization of Kant’s idea of a fixed system of representations that applies to all phenomena. From the characterization of what has to be considered as the ‘ideal image’ in representing organic systems to the descriptions of manipulations of objects, what Daston and Galison do is a Foucauldian analysis of different ‘orders of things’.

5.5 Reinterpreting the Concept of Style of Thinking: Williams and Hacking

Hacking’s writings that date from after 2000 present a interesting rereading of the concept of SoT inspired by the philosopher Bernard Williams. In 2002, the latter published *Truth and Truthfulness* (Williams 2002) an essay that explores the value of truth and the meaning of truthfulness. For the benefit of his argument, Williams reflected on different commitments to truthfulness that originated in certain historical periods, took over one from another or coexisted. Hacking found this set of reflections on truth-telling very interesting (Hacking 2004, p. 137) and re-described his notion of SoT from Williams’s perspective. In this and the next section, I shall compare Williams’s account of truth and truthfulness with
Hacking’s views in order to explore the philosophical import of the notion of SoT.

I do not see this analysis as conceptually separated from the content of the previous section: once I shall have explained Hacking’s point that SoTs are different ‘ways of being truthful’, SoTs and epistemic virtues will look much more tightly related concepts.

In his book William points out that some philosophers, called ‘deniers’ (Williams 2002, p. 5) are suspicious of the fact that any historical account can aim at truth: the stories that are told about the past, e.g. that of European anti-Semitism or the French Revolution, are always biased or ideological. Therefore, they say, we should admit that the concept of ‘pursuit of truth’ is empty and truth or truthfulness cannot be considered as values. Williams defends the opposite thesis that any human society needs to possess truth and truthfulness as values. His move is to look at truthfulness from a historical perspective in order to provide a narrative that reveals the role of truthfulness in different societies, the way it came about and developed. Indeed, according to Williams, truthfulness has a history and telling it will show that all the societies need dispositions of truthfulness and the sense of the value of truth, otherwise they ‘lose everything’ (Williams 2002, p. 7). I shall not examine the whole of Williams’s reply to the deniers; instead, I shall focus on those parts of his argument that are interesting for my purposes. In particular, I shall make it clear that both Williams and Hacking start from a common premise; then, discussing an example of Williams about how truthfulness changes, I shall make some more points about the concept of SoT.
5.5.1 Truth and Truthfulness

The premise of Williams's account of truthfulness and truth is that they are radically different categories - truthfulness has a history, truth does not. For him, truthfulness essentially means to tell stories respecting truth, i.e. to tell the truth about something. In turn this means to be accurate and sincere: *accuracy* implies doing one's best to acquire true beliefs and *sincerity* implies telling exactly these beliefs to other people. These two dispositions have a history because the way of being sincere or accurate has changed throughout different cultural situations and ways of thinking: for example, Williams argues that ' [...] a set of ideas, which associate Sincerity with personal authenticity, [...] came into distinct existence in the eighteenth century' (Williams 2002, p. 172) and, as we shall see in the next subsection, Denis Diderot (1713-1784) and Jean-Jacques Rousseau (1712-1778) had two distinct ideas of sincerity and authenticity.

While sincerity and accuracy, and thus truthfulness, are historically changeable, the concept of truth has never changed. By saying this, Williams means that the idea of what truth is, the role that this idea plays in talking, thinking, arguing and so on has always been the same in history: the concept of truth for an Egyptian or a Greek is absolutely the same of that for a European in the Middle Ages or for us; even if, telling their stories, they all have been answering to different standards or values.

This view is entirely shared by Hacking. Although in his review of *Truth and Truthfulness* he points out that Williams's claim that truth is a human universal is 'more of an assertion than an argued statement' (Hacking 2004, p. 140) it remains the key premise of the SoTs project: 'I accept as a convention that truth is
a formal concept, without content and history' (Hacking 2006f, p. 3) I invite the reader to bear this important claim in mind as my argument proceeds in the next two chapters. For the time being I just want to make it more precise: by saying that styles of reasoning settle what to be objective means, Hacking does not want to say that logic and the concept of truth change through time. Indeed, one’s exercise of rationality involves logical argument and evidence (in different forms); it is only what is considered as amenable to public evidence, i.e. what is objective, which changes switching from a style into another, not the types of inferences (deduction, induction and abduction) used in arguments and the concept of what truth is.

5.5.2 Shifts in the Way of Telling the Truth

Having discussed Williams’s premise, I shall now examine more closely what he means by saying that truthfulness can change. In Truth and Truthfulness he illustrates the different conceptions of the self associated with Rousseau and Diderot. Rousseau’s project in his Confessions was to reveal himself to his readers. One of his main questions was to understand how to bring about a concord between what one is for oneself and what one is for others (Williams 2002, p. 175). A passage of Williams explains what Rousseau’s answer was:

Sincere, spontaneous, non-deceitful declaration, the product of his presence to himself, will guarantee a true understanding of [one’s] motives. Moreover, what is revealed and understood in this way will represent a character, a whole person, and this implies that it will be coherent, or, as one might say, steady (Williams 2002, p. 178)
A precise conception of sincerity underlies this answer: sincerity means to declare what is immediately evident to oneself. In turn this conception implies an idea of the self as a set of constant unchanging features ('steadiness') that can be revealed once and for all.

Williams contrasts Rousseau with Diderot. The latter wrote *Rameau’s Nephew* (published in 1805), an imaginary conversation between ‘moi’, the narrator, and ‘lui’. Diderot wanted to understand how the interaction with other people could stabilize the whimsical inconstant mental constitution of people. Indeed, Rameau is described as a person who does not know who he is, ‘[who] expresses different things at different times’ (Williams 2002, p. 190). As Williams explains:

> Diderot was always attracted to a picture of the self as something constantly shifting and reacting and altering [...] for him the declaration at a given instant of self can be only a declaration of self at that instant (Williams 2002, p. 190).

To conclude, while for Diderot sincerity meant uninhibited expression, for Rousseau it meant to report the finding of self-examination.

### 5.5.3 Style of Thinking as Ways of Telling The Truth

While Williams, given his aims, limited his analysis to truthfulness about domains such as the past or the self, Hacking tried to generalize it to other domains of knowledge: ‘I interpret [Williams’ account] as a genealogy of the possibility of
telling the truth in a domain or another (Hacking 2006f, p. 5). He proposed this schema:

the emergence of a new SoT is represented by a change in conceptions of what it is to tell the truth about X (Hacking 2004, p. 142).

In the case of Williams’s case studies, X was ‘the self’ or ‘the past’, but for Hacking X extends to many other domains of knowledge: e.g. the geometrical relations in the case of the geometrical style. For example, following Hacking suggestion we can say that the style of geometry represents a shift of the conception of what means to tell the truth about the geometrical relations: whereas the Egyptians used algorithmic methods, the Greeks introduced geometry, their new method for telling the truth. Ultimately, the schema above, rather than being a proper definition of SoT, represents a different perspective suggested by Williams from which SoTs are viewed as ways of telling the truth, distinct modes of being truthful.

Let us now consider Diderot’s and Rousseau’s ideas of the self: Diderot was committed to the uninhibited expression of himself, whereas Rousseau was committed to the non-deceitful declaration of feelings. By paraphrasing Daston and Galison it is possible to say that just as in telling the truth about the self Diderot and Rousseau were committed to two different ‘non epistemic virtues’, two different conceptions of both authenticity and sincerity, so in telling the truth
about nature scientists are committed to different epistemic virtues. Williams's account in ethics parallels Daston and Galison's in epistemology. Moreover, according to Williams' schema, just as Diderot's and Rousseau's conceptions of the self represent two different ways of being truthful to it so SoTs represent 'different ways of being truthful to nature'. Since there is no significant change of sense resulting from the substitution of the adjective 'truthful' with the adjective 'faithful' it is possible to say that according to Williams' schema SoTs are 'ways of being faithful to nature' – and this is exactly one of the ways in which Daston and Galison have characterized the commitment to epistemic virtues. In short, SoTs as well as the commitments to epistemic virtues can be considered as different ways of telling the truth about nature or, likewise, as different ways of being objective.

5.5.4 Style of Thinking as Ways of Looking at the World

The comparison between SoTs, epistemic virtues and ways of being truthful can be pushed further. Let us reconsider Diderot's and Rousseau's pictures of the self instead than their conceptions of sincerity. For Diderot the self was 'disintegrated' (Williams 2002, p. 189), the result of spontaneity, its manifestation, it consisted of all that can be 'seen' and is 'external' in a given moment: e.g. jokes, lies and expressions. For Rousseau, it was an unchanging unity, all that is 'internal' and in need of being expressed: e.g. feeling, emotions and states of mind. In other words, both Diderot and Rousseau selected a particular aspect of the self and excluded others: the former directed his attention to the exterior product of spontaneity; the latter directed his attention to interior states of the self. In the previous section I
had pointed out that also SoTs and epistemic virtues *select* and *exclude*: each SoT has its own questions and each epistemic virtue teaches how to make sense of a certain sliver-world. Ultimately, SoTs and epistemic virtues can be viewed not only as ways of being truthful but also as particular ways of looking at the portion of the world under study, which shed light on particular aspects of it and put others in the shade. This may well be the consequence of human inability of encompassing all the aspects of things.

5.6 Conclusions

I have argued that the SoTs project can be better understood within the context of historical epistemology, which I have described. A salient point of my analysis is that Hacking’s is one of the projects of historical epistemology that historicize Kant. In this respect, a profound insight into the notion of SoT can be gained by an analysis of *classical* historical epistemology to which I have traced back many ideas of the SoTs project. There are also substantial convergences between the SoTs project and Daston and Galison’s study on objectivity, although the notion of SoT, as a concept, is different from the notion of epistemic virtue. Finally, Williams’ account of truth and truthfulness helps to show that SoTs are ways of telling the truth and ways of looking at the world.
Chapter Six: Knowledge, Styles of Thinking and the Relativism Issue

6.1 Introduction

In this chapter I shall discuss a philosophical issue concerning the notion of SoT. This issue boils down to the question as to whether the SoTs project implies epistemic relativism. My aim is to bring it to the fore as well as examine and frame it. Only in the next chapter will I tackle this issue (henceforth ‘relativism issue’) by arguing that Hacking’s claims do have relativistic implications.

The relativism issue is best described by comparing historical epistemology and Edward Craig’s approach to epistemology, the so-called ‘state-of-nature approach’. Craig explored the needs of cooperative human beings in an imaginary state-of-nature society in order to explain the role and the features of the concept of knowledge. The state-of-nature scenario provides a logical rather than a historical model that I shall apply to the SoTs project in order to elaborate a new view of it and clarify some points that are still unclear. The most important conclusion that I will reach is that the SoTs project does not imply alethic relativism although, as it will be clear, it is still an open question whether or not it implies epistemic relativism. I shall conclude that Hacking has not provided
sufficient reasons to exorcise epistemic relativism, despite his insistence on anti-relativism.

6.2 State-of-Nature Approach versus Historical Epistemology: the Concept of Knowledge

As I have explained in the previous chapter historical epistemologists think that organizing concepts can be historical. Specifically, if we focus on the (organizing) concept of knowledge we have every reason to think that Hacking considers it as historical and situated. Indeed, in ‘Historical Meta-Epistemology’ first he listed this concept among the organizing concepts:

Historical meta-epistemology [HE] is concerned with very general organizing concepts that we use today: knowledge, belief, evidence, good reason, objectivity, probability (Hacking 1999a, p. 58 my emphasis)

and then, in the following paragraph he stated that

‘[historical epistemologists] begin with the common place that organizing concepts [...] change, evolve and undergo mutations (Hacking 1999a, p. 59).
But, does the concept of knowledge really have a history? As it will be clear in the next section, there is nothing in the SoTs project that implies this conclusion. If anything, by relying on methods different from those of HE, the philosopher Edward Craig in the book *Knowledge and the State of Nature* (Craig 1990) provided an indirect answer to this question that contrasts with Hacking’s claims above. And if we look at the notion of SoT from the perspective of Craig’s approach to epistemology we are forced to conclude that the concept of knowledge does not change over history. In order to argue for these theses I shall start by comparing Craig’s approach with HE.

### 6.2.1 State-of-Nature Approach versus Historical Epistemology:

**Same Questions Different Methods**

Central to Craig’s reflection on epistemology is the question as to why, in addition to the concept of true belief, we have a concept of knowledge at all. In asking this question Craig distances himself from traditional epistemology that pays no attention to the social value and the function of knowledge. Like HE, Craig’s approach to epistemology is not normative in that, rather than investigating the necessary and sufficient conditions for knowledge, he wants to present an account of the reasons why, to paraphrase Hacking, the concept of knowledge became ‘ours, present and inescapable’ (Hacking 1999a, p. 65). Craig’s approach is similar to that of HE also in another respect: he actually asks why the organizing concept of knowledge came about just as Hacking asks how the organizing concept of probability emerged. However, Craig adopts a method of analysis that radically differs from that of HE: he tells a fictional story about an epistemic state of nature.
This approach, called ‘state-of-nature approach’, takes as his model the tradition of naturalism in which philosophers such as Hobbes, Locke, Montesquieu and Rousseau saw concepts, practices and institutions as the outcome of natural causes (Craig 1990, pp 8-9). Just as in *Leviathan* (1651) Hobbes looked at the human behaviour in a state of mere nature in order to establish the rational principles for the construction of a civil polity, so Craig gives a full account of the concept of knowledge by exploring the needs of cooperative human beings in an imaginary state-of-nature society.

6.2.2 The Concept of Trustworthy Informant

In presenting his fictional story Craig takes as a solid point to start from this one:

Any community may be presumed to have an interest in evaluating sources of information [...] [T]he concept of knowledge is used to flag approved sources of information (Craig 1990, p. 11)

Craig posits that the concept of knowledge came about in our early societies to satisfy fundamental epistemic needs: the need of acquiring true beliefs, of exploiting the information possessed by others and of signalling *trustworthy informants*. By saying that a teacher *knows* when Napoleon was born it is meant that the teacher is a good informant about Napoleon’s date of birth. Here, the role of the term ‘knows’ is that of signalling that the teacher is a good informant. Notice that Craig’s claim strikingly contrasts with that of the historical epistemologists: for Craig, given our epistemic needs, it is *inevitable* that we have
this concept whereas for the historical epistemologists organizing concepts emerge for contingent reasons, which are revealed by studying their history.

If knowledge is a concept that inquirers used to flag an approved informant K in the state of nature, there must be certain features possessed by K that make us think that she can be taken into account as a trustworthy informant. As Craig puts it:

What I have in mind is that if the informant possesses any condition which correlates well - as we believe - with telling the truth about p, he will be regarded as a good source (Craig 1990, p. 13).

According to Craig, in the state-of-nature society for being trustworthy an informant had to: 1) hold a true belief p; 2) be honest and understandable towards the inquirer; 3) be able to make the inquirer believe that p; 4) be accessible to the inquirer; 5) possess indicator-properties $X_1...X_n$ that correlated well with having all these features. For example, 'Fred, who is up a tree, is more likely to tell me the truth as to the whereabouts of the tiger than Mabel, who is in the cave' (Craig 1990, p. 9). Features 1)-5) are not necessary and sufficient conditions; in particular situations of the state of nature one or more of the elements above could well be lacking. Furthermore, the set of properties $X_1...X_n$ has no precise identity: in certain circumstances a property could be that of having arrived at the belief by a reliable method; in other cases, even being willing to offer an opinion correlates with being right.
6.2.3 State-of-Nature Approach versus Historical Epistemology: Different Conclusions

Kusch calls ‘proto-knowledge’ the concept of knowledge that emerges in the state-of-nature in order to flag trustworthy informants. (Kusch unpublished manuscript) (Kusch 2011b). As historical epistemologists want to know how concepts have become present certainties so Craig wants to know how we got from the concept of proto-knowledge to the concept of knowledge. Again, although Craig shares with HE similar objectives, he adopts a different method. Instead of hypothesizing the existence of historical contexts that made it possible the crystallization of the concept of knowledge out of certain practices, he tries to imagine and describe the process, which he calls the process of ‘objectification’, which takes from the concept of proto-knowledge to that of knowledge. Craig illustrates the process of objectification with the imaginary example of the emergence of the concept of chair (Craig 1990, p. 84). In Craig’s example, as I interpret it, the various stages of the process of objectification of the concept of chair parallel those concerning the process of objectification of the concept of knowledge (however, notice that the latter is an organizing concept whereas the former is not). One could be interested in ‘something’ which she can sit on here and now, says Craig; then, in other circumstances, in ‘something’ which one can sit on sometimes in the future and somewhere in the space. Similarly, we can suppose that in the state of nature inquirers started to look for an informant to be trusted in situations that could emerge in the future. One may also imagine, adds Craig, that other people, interested too in sitting at future times, ask to other people whether this sort of objects exists. This stage parallels that in which
informants are recommended from inquirers to other inquirers: being recommended became a property that indicates knowledge. Then, continues Craig, one might wish that anyone should have something suitable to sit on if she wants to. Hence, the emergence of ‘the concept of something which is, in abstraction from what any particular person wants at any particular time and place, or even from whether anyone ever wants to sit down, simply suitable for sitting on’ (Craig 1990, p. 84). At this point, the concept of a chair is objectivized: it emerges to satisfy the need of this abstraction.

It is possible to assume that, in a way similar to the case of the concept of chair, human beings started to call someone a knower even though this very knower was not accessible to them. One does not need to think that truly things went on according to the process of objectification that takes from the concept of proto-knowledge to that of knowledge. But it makes sense that this kind of explanation applies to the transition from an imaginary state-of-nature society to a real human society in which the concept of knowledge crystallized. Indeed, some of the features 1) and 5) of a proto-knower disappear when we think of a knower: for example, an individual possesses knowledge even if we will never have access to it and we will never know that she possesses it. Ultimately:

What happens to the concept at the centre of our investigation, that of good informant, as objectification proceeds? The requirement of true belief remains, and so does that of a property correlating well with truth of belief on the issue in hand [...] (Craig 1990, p. 90).
To sum up, for us a trustworthy informant should possess a true belief and a set of properties $X$ that correlates well with having it. But, importantly, it remains true what I have stressed in the case of the proto-concept of knowledge: the set $X$ cannot be fixed, that is, valid in all the situations.

In conclusion, by using methods different from HE Craig argues that the concept of knowledge cannot be historicized. For him, its role of flagging informants who possess a true belief and a property $X$, no matter which one, which correlates well with it, has not changed over history:

the core of the concept of knowledge is an outcome of certain very general facts about the human situations (Craig, 1990 p.10)

As these facts are so general 'that one cannot imagine their changing whilst anything we can still recognize as social life persists' (Craig, 1990 p.10), he concludes:

'there is not a reason to expect the concept of knowing to change as well' (Craig, 1990 p.10).

This conclusion is at odds with Hacking's general claim that organizing concepts are historical and situated.
6.3 From Craig's State-of-Nature Approach to the Styles Project

It must be emphasized that Hacking's claim that knowledge belongs to the kind of concepts that are historical is unsubstantiated. Indeed, there is nothing in the SoTs project (as well as in Daston and Galison's account of objectivity) that plays in favour of this claim; if anything, the core thesis of his project can be couched in terms of the concept of trustworthy informant discussed above.

In order to support this claim let us choose as a case study the emergence of the laboratory SoT. It is easy to see that its coming to the fore did not involve any change of the concept of trustworthy informant (knower), rather a change of who is to be considered a trustworthy informant. Indeed, as I have argued in section 2.5, the story of Hobbes fighting Boyle (told by Shapin and Schaffer) can be interpreted as a dispute between two men who had two different SoTs (respectively, the postulational and the laboratory SoT). In his *A Social History of Truth* Shapin argued that in the seventeenth century the decision of whether or not to believe in the interpretation of an experiment was largely a question of what kind of person to trust. Shapin claimed that in the community of the Royal Society it was the gentleman who was counted trustworthy because of his moral qualities:

Gentlemen were truth-tellers because nothing could work upon them that would induce them to be otherwise (Shapin 1994, p. 84)
therefore:

If one wished to make experimental knowledge, then here [in the laboratory] were the technical, social and discursive means with which it might be made (Shapin 1994, p. 127 my emphasis).

Conversely, for Hobbes:

The geometer [...] could do better than the experimentalist (Shapin and Schaffer 1989 [1985], p. 151)

in that:

when its methods [are] rightly followed, geometry yield[s] irrefutable and incontestable knowledge (Shapin and Schaffer 1989 [1985], p. 100)

These passages show that Hobbes and Boyle possessed the same concept of knower: it was someone who possessed a true belief and a property X, no matter which one, which correlated well with it. They only disagreed on which property X had to be attributed to the knower: for Boyle it was the property of possessing the methods of the laboratory SoT; for Hobbes it was the property of possessing the methods of the postulational SoT. In other words, they disagreed on the answer to the following question: who were the trustworthy informants? But for both of them 'knowledge' had the same meaning: it was a 'tag', so to speak, that could be attributed to the experimenter (for Boyle) or to the geometer (for Hobbes). It is in
this sense that we can say that the concept of knowledge is invariant. What may change when a new SoT emerges is that the adherents of the new SoT are not viewed as trustworthy informants by the adherents of the old one, e.g. because they adopt certain ways of thinking and doing, methods and standards of evidence. For example, for the members of the Royal Society the private work of alchemists became a marker of bad informant.

It is to be noted that just as the SoTs project does not imply that the concept of knowledge varies (although Hacking includes it among the organizing concepts that can be historicized) so does not Daston and Galison’s account of objectivity. An example may clarify this point. In *Objectivity* Daston and Galison mention the confrontation between embryologist Wilhelm His (1831-1904) and the colleague Ernst Haeckel (1834-1919) about the use of embryological evidence (Daston and Galison 2007, p. 191). It was an opposition between two ways of collecting evidence based respectively on drawing and photographing: Haeckel used his drawings, which show the essentials of embryological phases of mammals, to make his point that ontogeny recapitulates phylogeny. On the other hand, His proposed an elaborate mechanical method of making images and accused Haeckel of introducing subjective elements and prejudices into his drawings. Daston and Galison comment:

> The His-Haeckel confrontation dramatizes the transformation of scientific ideals and practices [...] By the middle decades of the nineteenth century, the epistemology and ethos of truth-to-nature had been supplemented [...] by [...] mechanical objectivity (Daston and Galison 2007, p. 195).
Let us reframe Daston and Galison’s point. In the nineteenth century, what in the eighteenth century community of scientists counted as a set of properties \( X_1 \ldots X_n \) of the trustworthy informant was supplemented, and in some cases substituted, by new properties \( Y_1 \ldots Y_n \). Obviously, both in the eighteenth and in the nineteenth century, for a certain part of the scientific world to possess a true belief and any property correlated with it was a necessary requirement for being a good informant. However, what counted as the set of properties that marked out the knower was different in the two cases: in the nineteenth century, to make use of illustrations that showed only the essentials of an object did not represent anymore a property that characterized a trustworthy informant. When this happened, Haeckel came to be an untrustworthy informant for His. For a certain part of the thinkers in the nineteenth century drawing the essentials of an object did not correlate well with possessing a true belief, whereas in the previous century it did. Ultimately, from Daston and Galison’s account does not emerge that the concept of knowledge changes, although the properties that identify the knower do. No wonder that in the case of Daston and Galison we have reached the same conclusions as for Hacking. When Daston and Galison claim that ‘truth-to-nature, mechanical objectivity and trained judgement all combat genuine dangers to knowledge’ (Daston and Galison 2007, p. 376) they are implicitly assuming that the concept of knowledge is unchanging. They discuss how epistemic virtues emerge in order to contrast the dangers to knowledge but they do not put forward any idea that implies that the concept of knowledge has changed over time. Finally, my analysis suggests that Daston and Galison’s claim
that epistemic virtues are 'a plurality of visions of knowledge' (Daston and Galison 2007, p. 371) can be re-expressed by saying that epistemic virtues correspond to a variety of sets of properties that characterize the knower in different historical circumstances. These sets of properties, we might say, characterize the different 'scientific personas' (committed to different scientific virtues) described in *Objectivity* (Daston and Galison 2007, pp. 216-243) and 'The Moralized Objectivities of Science' (Daston 1999).

In the next subsection I shall elaborate on the points made here by reading the SoTs project in the light of Craig’s concept of knowledge.

6.3.1 State-of-Nature Approach and the Styles of Thinking Project

We have seen how the concept of knowledge can be characterized in the state-of-nature approach. My question now is: how can SoTs be characterized in the same perspective? On the basis of my discussion in the previous subsection I propose the following characterization:

1) the exponents of distinct SoTs can be viewed as possible informants for a hypothetical inquirer.

2) the concept of knowledge, in particular its role of flagging informants who possess a true belief and a set of properties $X_1...X_n$, no matter which one, which correlates well with it, has not changed over history.
3) at different times in the history of science, different sets of properties $X_1 \ldots X_n$ have crystallized and have been claimed to be markers of a good informant.

It can be noted that the crucial factor that explains the existence of different SoTs is what I have called the set of properties $X_1 \ldots X_n$ that correlates well with holding a true belief. There is a close connection between properties $X_1 \ldots X_n$ of a SoT and its presuppositions, which are, as I have explained, the methods, standards of evidence, questions, ways of thinking and doing that characterize a SoT. Indeed, we may expect that the presuppositions of a certain SoT identify certain properties of the informant that are recognizable by the inquirer: for example, to adopt the laboratory way of doing may well correspond to properties such as that of wearing a white coat. In certain situations a presupposition may coincide with a property $X_i$, that is, an inquirer can straightforwardly recognize a presupposition of the SoT of the informant, e.g. her way of doing or her methods of research, but this is not always the case. More often the inquirer will recognize a property $X_i$ that signals the existence of a presupposition. As a simple example one may recall what Craig says:

the property of being a taxi-driver certainly cannot be identified with that of having discovered, by a reliable method, how to find one’s way round the neighbourhood […], although of course it is true of taxi drivers that they have found that out by a reliable method: driving round the place for hours and hours […] so long as [the inquirer]
believes that they do, and can recognize them, he can acquire the best information (Craig 1990, p. 26).

In this case, for an inquirer it is sufficient to recognize the property of driving a taxi since it is expected that in that case the informant has used a reliable method. Being a taxi driver is the property that signals the existence of a method. Similarly we may think that certain properties of the adherent of a certain SoT signal the existence of a method, the method of that very SoT. In conclusion SoTs identify certain presuppositions or, similarly, certain properties $X_1 \ldots X_n$. Presuppositions and properties $X_1 \ldots X_n$ are isomorphic. Consequently, point 3) reduces to the already known claim that in the course of history of science certain SoTs/presuppositions/properties $X_1 \ldots X_n$ have crystallized29.

Ultimately, over history it is possible to recognize the crystallization of several distinct sets of properties $X_1 \ldots X_n$ of what is considered the trustworthy informant. We may think of these properties $X_1 \ldots X_n$ as related to the presuppositions of the new SoT of this informant. However, the role and the essential features of the concept of knowledge have remained intact: at any time an approved informant is someone who possesses a true belief and a set of properties $X_1 \ldots X_n$ that make him distinguishable to any inquirer.

Moreover, we can now say that, although Hobbes and Boyle possessed the same concept of knowledge, they did not agree on what was the set $X_1 \ldots X_n$ correlated with true beliefs of informants. When in the late 1650s the English

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29 Before Hacking, Shapin and Schaffer had used the verb 'crystallize' in the *Leviathan and Air Pump*, e.g. in (Shapin and Schaffer 1989 [1985]-a, p. 14), to refer to the formation of scientific communities. Following all of them I have used the same verb to refer to new SoTs or presuppositions or properties $X_1 \ldots X_n$ that come about and stabilize.
experimental community started to use laboratory apparatuses in order to
generate phenomena, a new way of providing evidence, the new kind of evidence
provided by man-machines, crystallized. In turn, certain properties $X_1 \ldots X_n$
crystallized. To adhere to certain forms of discourse and certain modes of
solidarity, to produce experimental knowledge in a public space, to provide
evidence by using apparatuses were all part of the set of the properties that
distinguished a good from a bad informant. Hobbes did not accept as a marker of
a good informant these properties.

As a last point, I wish to explain that the state-of-nature approach can be
used to address another question that Hacking’s account of SoTs has left open.
Suppose that a biochemist performs a blood test and presents the results to a non-
expert patient in a language appropriate for a lay audience. On the one hand, the
biochemist has carried out the test by adopting the laboratory SoT and the
statistical SoT; on the other hand, the non-expert patient has simply ascribed to the
biochemist knowledge for what she asserted. Can we say that the non-expert
patient is adopting a particular SoT? Or should we say that only the biochemist
adopts the laboratory and the statistical SoT? Do SoTs concerns only groups of
experts within a certain community? One way to answer these questions is that all
the members of a given community share the same presuppositions and can
understand and project (see subsections 4.4.2 and 4.4.3 for the use of these terms)
the style-dependent sentences uttered by the experts of their community. But in
the light of the state-of-nature approach we can say that the non-expert patient
ascribes knowledge to the biochemist because she recognizes certain properties
$X_1 \ldots X_n$ that distinguish the good from the bad informant. To wear a white coat
and work in a hospital could well represent a property \( Xi \) that the patient is looking for: so long as she believes that \( Xi \) correlates well with acquiring the best information about her physical conditions, the patient does not need to know anything about the reliable method used by the biochemist. Therefore, the expert and the non-expert who belong to the same community share the belief that a set of properties \( X1 \ldots Xn \) identifies a trustworthy informant. The expert may have a deeper understanding of why \( X1 \ldots Xn \) is a marker of a true belief but both the expert and the non-expert agree on considering \( X1 \ldots Xn \) the propriety that is required. This identity of views about \( X1 \ldots Xn \), i.e. about the physiognomy of the trustworthy informant, descends from the circumstance that both the expert and the non-expert share the same presuppositions, to which \( X1 \ldots Xn \) point to.

Ultimately, ‘to adopt a certain SoT’ means ‘to belong to a community whose members agree about the properties that define a trustworthy informant’ or, equally, ‘to belong to a community whose members share the same presuppositions’.

### 6.4 The Relativism Issue

Let us return to the Boyle-Hobbes dispute. When the laboratory SoT emerged, it raised the question (to which Hobbes answered in the negative) as to whether to consider the gentlemen of Boyle’s community as trustworthy informants. Since a similar question might have been asked, *mutatis mutandis*, when other SoTs emerged, it is better to give to it a general formulation: ‘which informants should
one accept?' That is: 'which sets of properties $X_1 \ldots X_n$ should one accept? Which presuppositions?

We come to see that an important issue underlies the SoTs project. This issue arises from a double claim, which in the light of my analysis can be couched in these terms: on the one hand the SoTs project does not imply that the concept of knowledge has a history; on the other hand it implies that the properties $X_1 \ldots X_n$ that help to recognize the knower do have a history. Then the question is: how then can we recognize knowledge? That is, which set $X_1 \ldots X_n$ should we single out in order to be justified in choosing a certain informant? This is not an abstract dilemma that vanishes when we come to concrete examples. Suppose that a speaker that belongs to a given community that adopts a certain SoT utters a style-dependent sentence. A listener that belongs to a different community with a different SoT can come to see the presuppositions that make that sentence true-or-false in the speaker's SoT. The sentence would become intelligible to the listener but she could ask: are those presuppositions right or wrong? At that point, the listener would require independent presuppositions to provide an answer to this question. Do these independent presuppositions exist? Is what is true-or-false for a certain community true-or-false for any community? And if so, how are we to justify this claim? In this section I shall give a precise identity to the philosophical issue I am discussing. Then I shall argue that Hacking does not provide any clue about how to address it.
6.4.1 What Kind of Incommensurability?

At first glance, it seems that the issue I am discussing resembles those incommensurability problems in which it is asked whether there is a 'common measure', that is a universal standard for comparing the merits of competing paradigms or theories. In this subsection I shall argue that this intuition is correct, although the kind of incommensurability issue posed by the SoTs project is different in nature from that discussed by previous philosophers of science such as Feyerabend and Kuhn.

The term 'incommensurable' originated in the sixteenth century from Medieval Latin *incommensurabilis* ('not-measurable-together'). It was and it is still used in mathematics and physics to indicate the concept of two entities not measurable by the same standard. Typically, two lengths are *commensurable* if one can express their ratio as that of two integers \(m\) and \(n\).

So far as I am aware, Fleck has been the first to use the word 'incommensurable' in philosophy of science. In his papers in Polish he borrowed from mathematics the term *niewspółmierność* (Sady 2012), which is translated into the German term *inkommensurabel* in his *Genesis and Development of a Scientific Fact*. He wanted to express the idea that certain concepts, which are not replaced with adequate substitutes in an emerging thought style, cannot be brought into the usual logical relations of inclusion, exclusion, overlap with new concepts (see for example Fleck 1979 [1935], p. 62). The SoTs project does not investigate the compatibility of concepts within a given SoT. However, it would be correct to say that, as for Fleck there is no common measure that makes it possible to understand both old and new concepts, so for Hacking there is no common measure that
allows a given community to understand and use all the concepts that have come about and will come about over history. This common measure is to be understood as a common thought style in the case of Fleck and a common SoT in the case of Hacking.

It is a fact that only with Kuhn and Feyerabend did the term 'incommensurable' catch on in philosophy of science. Kuhn matured the notion of incommensurability under the influence of philosophers such as Ludwik Fleck and Michael Polanyi (1891-1976) and Gestalt psychologists such as Wolfgang Köhler (1887-1967). According to Eric Oberheim, Feyerabend developed it more than a decade earlier than Kuhn, drawing it from Pierre Duhem (1861-1916) who, in turn, had developed it already in 1906 (Oberheim 2005, p. 372). Feyerabend used for the first time the term 'incommensurable' to describe the relation between two non instantial-theories, i.e. theories that have ontological implications about the nature of the world. Feyerabend claimed that two such theories are incommensurable because the concepts of one theory can

neither be defined on the basis of the primitive descriptive terms of the other [theory], nor related to them via a correct empirical statement

(Feyerabend 1962, p. 59)

Concepts do not 'keep intact' from a theory into another. Feyerabend illustrated this idea by comparing geometrical and wave optics or physics and relativity. For example, he maintained that the term 'mass', as used in classical physics, denotes

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30 Kuhn read the *Genesis and Development of a Scientific Fact* in 1949-50, as he says in his foreword of 1976 to the English edition of the book.
a constant property of a body, while in theory of relativity it denotes the total quantity of energy divided by the speed of light squared.

To grasp the cause of meaning change it is necessary to consider that for Feyerabend the framework of a theory gives meaning to its theoretical and empirical terms. In turn, these meanings affect our ontology, our conception of what exists. For example our ontological concept of what light is made of has changed from the corpuscular theory to the wave theory; each of the theories proposed has given meaning to its theoretical terms and this, in turn, has determined a different ontological concept of what light is. Since the framework of a theory gives meaning to its empirical terms (as well as its theoretical ones) it is not possible to count on empirical facts in order to find a universal criterion to judge between different theories. Therefore theories are incommensurable: there is no common criterion, to decide which ontological consequences are correct31.

we no longer assume an objective world that remains unaffected by our epistemic activities (Feyerabend 1988 [1975], p. 19)

Like Feyerabend, Kuhn believed in the impossibility of defining the terms of one theory on the basis of the terms of the other. Moreover, under the influence of Fleck, he maintained that different paradigms use concepts that are incompatible. However, as he refined his ideas, Kuhn developed a concept of incommensurability not only limited to the idea of translation failure. He argued that incommensurability was not only caused by linguistic factors but also by

31 Beside conceptual inconsistence, Feyerabend also argued for the logical inconsistence between theories. I will ignore this point.
differences in methods for setting up research and evaluating its results. Furthermore, under the influence of the Gestalt psychologists, he also spoke of differences of perceptions between adherents of different paradigms. It is from the collaboration of all these elements that incommensurability follows. For example, in *The Structure of Scientific Revolutions* he argued that in the seventeenth century different people might have viewed in a different way the same stone swinging from a string. An Aristotelian would have seen the stone as constrained in its downward motion; an adherent of the new Galilean physics would have seen a pendulum that repeats the same motion forever. Kuhn claimed that these two conceptualizations are incommensurable because there is no neutral way to adjudicate between them:

>This is the] most fundamental aspect of the incommensurability [...] The proponents of different paradigms practice their trades in different worlds. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. [...] Before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift (Kuhn 1996 [1962], p. 150)

As a consequence, paradigm change is like a Gestalt switch in perception. To sum up, in both Kuhn’s and Feyerabend’s versions of incommensurability I have sketched out it can be recognized a *semantic aspect*: two paradigms or theories are incommensurable when there is no neutral language into which the content of both theories can be translated. In this sense by a lack of
a 'common measure' it is meant a lack of a 'common language'. However, Kuhn's version of the concept of incommensurability also contains an epistemological and methodological aspect concerning how scientific claims have to be justified: there is not universal standard that serves to justify a paradigm against its rivals.

Finally, it is important to bear in mind that Kuhn and Feyerabend believed that observational propositions are theory laden. This point is crucial for their claim of incommensurability: for example, in Kuhn the replacement process of paradigms cannot be a falsification process à la Popper in which certain observational data falsify a paradigm. The two kinds of incommensurability I have distinguished in the works of Kuhn and Feyerabend have been progressively recognized in the contemporary debates as different in principle. In 2004 Léna Soler distinguished two aspects of incommensurability (Soler 2011):

1) The semantic incommensurability, which concerns semantical or descriptive changes about what rival theories, paradigms, thought-styles.

2) The methodological incommensurability which concerns changes in the conceptions of how a scientific claim can be justified.

By comparing Hacking's claims with Feyerabend's and Kuhn's I am going to argue that the set of questions I have asked in the previous section can be identified, in Soler's terminology, with a methodological incommensurability issue or, better, with an 'epistemic incommensurability issue'.

First of all, nothing in the SoTs project leads to semantic incommensurability. It is true that by saying that certain style-dependent sentences have no meaning within another SoT Hacking often assumes verificationism, a thesis about meaning. But to assume that only statements about
the world that are verifiable by a certain method are meaningful does not imply that the meaning of a sentence changes from a SoT to another. That is: within the SoTs project verificationism is a semantic thesis that does not imply semantic incommensurability as in the case of Kuhn and Feyerabend. Hacking’s point is that there are sentences that are meaningful in certain SoTs (e.g. because there are methods to assess their truth) and meaningless in other SoTs -- the meaning of sentences does not change from a SoT to another.

Although style-dependent sentences are intelligible, i.e. their presuppositions can be identified and understood, they cannot be projected into a different SoT. Nor it is always possible to switch from a SoT to another as when, as Kuhn imagines, one switches from the Keplerian system to the Ptolemaic. For Hacking, a Renaissance scientist could well learn how Boyle justified his sentences but in order to use all his sentences she would have to reject the presuppositions of her community and espouse Boyle’s. Intelligibility is not enough. The Renaissance scientist would have to think, do and justify her conjectures as Boyle did -- and she would not be able to perform these mental and physical actions while preserving the presuppositions of her community. Of course it is true that, for example, today it is possible to switch from the probabilistic style to the taxonomic style: their presuppositions are our presuppositions. So, the suspicion of incommensurability of SoTs comes from the fact that there seems to be no common measure, i.e. no universal set of presuppositions, valid for all the theoretical sentences expressed by communities that have different SoTs.

Similarly, the SoTs project does not posit that advocates of different SoTs have different perceptions. According to Kuhn, the idea of incommensurability
sprung to his mind in 1940 when, studying Aristotle's physics he found some passages unsound and senseless (Kuhn 1987). Kuhn realized that by changing his way of reading the text, by changing some concepts and the meaning of some words, he was able to understand what Aristotle said. Later he described it as a *Gestalt switch* (Kuhn 1996 [1962], p. 111-135). Conversely, in reference to earlier scientific conceptions such as Paracelsus', Hacking said:

> [...] understanding is not enough. We cannot use, project, work in a former body of organized thought while we preserve our own

(Hacking 1993b, p. 298).

So there is a crucial difference between Kuhn and Hacking: for the former a Gestalt switch leads to understanding an ancient system of thought, for the latter even if we understand its presuppositions we cannot project its sentences.

At this stage of my argument I cannot conclude that Hacking's characterization of SoTs implies epistemic incommensurability. Indeed, although *some* style-dependent sentences of different SoTs hinge on distinct presuppositions, it is also true that all the SoTs share a criterion for assessing the truth or the falsehood of style-independent sentences. This circumstance could make the scales tip in favour of the possibility of a universal criterion for assessing style-dependent sentences. Furthermore, Hacking could claim that certain scientific claims can be expressed in terms of style-independent sentences, which can be assessed by using the evidence of senses. To get convinced of the fact that we cannot conclude yet that the SoTs project implies incommensurability, one should look at the role of theory-ladeness in Kuhn's and Feyerabend's
epistemologies. Whereas Hacking claims the existence of 'sense-datum statements', which are basic, foundational and out of history, and that do not require any reasoning to be assessed or false, Fleck, Kuhn and Feyerabend believe that all the observational propositions are theory laden. Thanks to this difference the latter thinkers are able to defend incommensurability. To sum up, the SoTs project leaves open what I have called the incommensurability issue.
6.4.2 What Kind of Relativism?

Is the incommensurability issue a relativism issue as well? Chris Swoyer has classified relativism by using the following schema:

Y is relative to X.

Each form of relativism is provided by: replacing Y by different concepts such as rationality, truth, standards of rationality, justification etc.; replacing X by what leads to the differences in the value of Y, e.g. cultures, paradigms, styles of thinking, languages etc.; and stating what ‘relative’ amounts to (Swoyer 2008).

In order to obtain an economical classification, Maria Baghramian has proposed to classify relativism by simply considering that Y can refer to cognitive, moral and aesthetic norms (Baghramian 2004, p. 6). For my purposes, I just want to point out that within the category of cognitive relativism it is possible to distinguish alethic relativism, which claims that the truth of statements is relative to an individual or social group and epistemic relativism, e.g. the claim that justification is relative to SoTs.

Epistemic incommensurability between SoTs implies epistemic relativism for, if there is no universal and independent justification, justifications can only be relative to different SoTs. In particular, a proposition might be justified in a SoT and meaningless in another SoT, which would amount to saying that facts about justification are not absolute. Obviously, epistemic relativism implies epistemic incommensurability so we can conclude that the incommensurability issue is a relativism issue.

Notice that an alethic relativist claim about SoTs would consist of this double claim:
1) different SoTs lead to different claims, sometimes incompatible, about the world
2) the justification for each of these claims is relative to the style of thinking in which they are embedded.

The epistemic incommensurability of SoTs I am discussing has nothing to do with points 1) and 2). Indeed, think of two SoTs S1 and S2. For Hacking, what might happen is: that there exists a proposition C such that C is claimed true in S1 and is neither true nor false in S2; and that C is justified in S1. This is different from saying that C can be claimed true in S1 and false in S2, which would entail alethic relativism.

6.4.3 What Hacking says

My contention in this section will be that Hacking has not offered any conclusive argument that helps to solve the incommensurability issue. Let us examine the passages relevant to this issue. In his first paper on SoTs he wrote:

Consider Hamlet's maxim, that nothing is either good or bad but thinking makes it so. If we transfer this to truth and falsehood, it is ambiguous between (a) nothing which is true is true, and nothing which is false is false, but thinking makes it so, and (b) nothing is either true-or-false but thinking makes it so. It is (b) that preoccupies me. My relativist worry is that the sense of a proposition p, the way in which it points to truth and falsehood, hinges on the style of reasoning appropriate to p (Hacking 2002 [1992], p. 180).
In this passage Hacking highlights that the key distinction to bear in mind is the difference between truth-and-falsehood and truth: point b) would imply alethic relativism; point a) only states that the truth-or-falsehood of style-dependent sentences is relative to SoTs. I think that Hacking is correct when he says that the SoTs project does not imply alethic relativism. Indeed, if the SoTs project implied that truth has a history then the concept of knowledge would have a history too, in contrast with what I have concluded. Ultimately, Hacking only manages to dissociate himself from alethic relativism – point b) is still compatible with epistemic relativism. The incommensurability issue remains open.

In his paper of 1992 Hacking touched on another important point: what is the reason of the disappearance of a SoT? Arguably if there were a universal and atemporal criterion to assess style-dependent sentences it might allow us to give up a SoT because it does not get at the truth. Hacking made no mention of such a criterion claiming that, although particular theories might be refuted a SoT, thanks to its self-authenticating techniques, endures. In his words:

It is our knowledges that are subject to revolutions, to mutation, and to several kinds of oblivion; it is the content of what we find out, not how we find out, that is refuted (Hacking 2002 [1992], p. 192).

In more recent writings Hacking distanced himself from constructionism (Hacking 1999b, 2000a). He distinguished form from content of knowledge, a distinction that is relevant to the SoTs project. By form of a branch of scientific knowledge Hacking means ‘sentences that can be true or false, together with techniques for finding out which ones are true and which ones are false’ (Hacking
In the terminology of the SoTs project: style-dependent sentences as well as methods and criteria of evidence, which are used to determine the truth-values of those very style-dependent sentences, constitute the form of a given SoT. The content of a SoT is what is found out, e.g. the Maxwell equations.

After having distinguished form from content, Hacking replied to constructionist thinkers:

the answer to a clear question about some aspects of the world is determined by how the world is. [...] when the question is a live one, and there is a context in which there are ways of addressing the question, or even methods of verification for possible answers, then aspects of the world determine what the answer is (Hacking 2000a, p. S 69).

In the terminology of the SoTs project ‘live questions’ are the questions that a community that adopts certain SoTs asks in a certain historical period. Hacking’s point is that once a SoT emerges, with its presuppositions and questions,

the answers to live questions about the natural world have nothing to do with us. (Hacking 2000a, p. S70).

I want to highlight two of Hacking’s central claims: 1) there are correct answers to live questions 2) these correct answers are found when there are ways of addressing the question. Now, think of an answer expressed in terms of style-dependent sentences found by a community that adopts a certain SoT. The problem is that we
might not be able to establish that this answer is correct until we find an atemporal and universal criterion to justify style-dependent sentences. If the ways of addressing the questions and the methods of verification are changeable how could one ever know whether the answers provided by a certain SoT are to be trusted? Hacking arguments can only oppose alethic relativism but are a blunt spear against epistemic relativism. I believe that, if Hacking wished to offer such a justification or, more in general, give an example of a 'correct answer' that can be expressed in terms of style-independent sentences and therefore justified independently of the SoTs in which is found, he could bring up his arguments in favour of realism about entities. In the next chapter I shall argue that there cannot be such an atemporal and independent justification.

6.5 Conclusions

I have compared and contrasted Craig's state-of-nature approach with historical epistemology. Despite historical epistemologists stress that organizing concepts are historical, the SoTs project does not imply that the concept of knowledge changes over history: the SoTs project and Daston and Galison's account of objectivity are consistent with the ahistoricity of the concept of knowledge. By developing and applying the state-of-nature approach to the SoTs project I have brought to the fore a philosophical problem, which I have called 'the incommensurability issue' (or 'the relativism issue'). I have shown that this issue is still open because Hacking does not provide a coherent and convincing answer.
Chapter Seven:

The Incommensurability of Styles of Thinking and Doing: the Case of the Existence of Theoretical Entities

7.1 Introduction

In the previous chapter I have argued that an important issue underlies the SoTs project. I have called it 'the incommensurability issue': the question as to whether there exists a universal and atemporal justification for the claims made by a community that adopts a particular SoT. In this chapter, I shall present a case study in which a claim made in the laboratory SoT has no universal and atemporal justification. As I shall explain, Hacking justifies his belief that unobservable entities exist on the ground that they can be regularly manipulated by experimenters in order to find out and produce various phenomena (experimental realism); he also maintains that no belief that our theories are true is required in order to be realist about unobservable entities. I shall argue that his justification is relative to the laboratory SoT. In particular, my point will be that, if Hacking's characterization of SoTs is correct, his justification strategy based on experimental
realism is no viable option for a member of a community that does not adopt the laboratory SoT – the existence of a particle would be justified in the laboratory SoT and unjustified outside it. Ultimately, from this case study I shall draw the important conclusion that the SoTs project implies epistemic relativism.

In the next section I shall present Hacking’s entity realism and theory antirealism and put them in relation with the SoTs project. In the third section, I shall first discuss the consequences of theory antirealism for the possibility of comparing different SoTs. Afterwards I shall deploy my argument that Hacking’s entity realism is not justified outside the laboratory SoT. The argument will be divided into two parts: whether or not we grant Hacking that experimentation is nearly theory-free the justification for the existence of unobservable entities is relative to the laboratory SoT.

7.2 Hacking’s Experimental Realism

The greater part of Hacking’s ideas about scientific realism is expounded in his book *Representing and Intervening* (Hacking 1983b), published one year later than the first paper on SoTs. This book mentions the notion of SoT three times (Hacking 1983b, pp. 56, 71, 127) but never refers to the notion of ‘laboratory SoT’ as the latter was introduced in the review of 1991 to *Leviathan and the air pump* (Hacking 1991a).

Hacking distinguishes between two types of realism: entity realism and theory realism (Hacking 1983b, p. 37). Generally speaking, *entity realists* deny
some or all of the beliefs held by the *theory realists* (such as: theories aim at the truth; if a theory is true then the fundamental terms of the theory denote real entities). However, unlike *entity anti-realists*, entity realists do consider fundamental entities as real and as part of the causal structure of the world. Hacking describes himself as an entity realist: an anti-realist about theories and a realist about entities. To start with I shall explain his theory anti-realism, then I shall focus on entity realism.

### 7.2.1 Hacking’s Entity Realism

In the paper ‘Experimentation and Scientific Realism’ Hacking acknowledged that ‘[his] own approach owes an enormous amount to Nancy Cartwright’s parallel developments, which have often preceded [his] own’ (Hacking 1984, note n. 3 p. 171). For her part, the philosopher Nancy Cartwright wrote that her book *How the laws of physics lie* (1983) is a complement to *Representing and Intervening* (Cartwright 1983, p. 20).

Hacking has espoused Cartwright’s distinction between phenomenological laws and fundamental laws, the former being laws that merely describe what happens in specific situations. In Cartwright’s example, Airy’s law of Faraday’s magneto-optical effect is a characteristic phenomenological law: the physicist George Airy (1801-1892) added some *ad hoc* further terms (first or third derivatives) to the equations of light (which are to be interpreted as fundamental laws) in order to represent analytically the rotation of the plane of polarization of a beam of light sent parallel to the lines of a magnetic field inside a piece of material. What Airy obtained is, in Cartwright’s terminology, a phenomenological law. The
wave equations of the theory of light are fundamental laws that Airy modified in order to describe the particular situation under study: the passage of light through a definite piece of material. Ultimately, Cartwright argues, as compared with fundamental laws, phenomenological laws do not describe idealized situations. They are faithful representations of certain aspects of the phenomena as they appear, considering all the circumstances in which they occur.

Building on these ideas, Cartwright argues that, although the fundamental laws of physics provide us with great explanatory power, they do not describe reality but rather idealized situations; as the title of her book states, the laws of physics lie, i.e. they do not get the facts right. On the other hand, when they are amended to be true by transforming them into phenomenological laws, they lose their fundamental explanatory force. Indeed, for her fundamental laws are actually ceteris paribus laws – for example the gravitational law is to be expressed: 'If there are no forces other than gravitational ones at work, then two bodies exert a force between each other which varies inversely as the square of the distance between them, and directly as the product of their masses' (Cartwright 1983, p. 58). So, for Cartwright it is wrong to say that for any two bodies the force between them is given by the law of gravitation. If the two bodies are charged, they will not behave just as the gravitational law states. '[The gravitational law] is irrelevant to the more complex and interesting situations' (Cartwright 1983, p. 39). To come out true the gravitation law must be reinterpreted either ceteris paribus (that is, by including the antecedent ‘if there are no forces other...’) or as describing a component force (that is, as one of the forces that act on the body in addition to, say, frictions, Coulomb law and so on). But for her to interpret a fundamental law
ceteris paribus means to lose descriptive adequacy since ceteris paribus conditions never hold in reality but only in ideal situations. On the other hand, she considers the addition of forces as an operation we perform when we do calculations; nature does not add forces.

Hacking also espouses Cartwright’s model of explanation. According to the covering law model, proposed by the American epistemologist Karl Hempel (1905-1997), to explain means to provide a law of nature that ‘covers’ the phenomenon, namely to show that the phenomenon had to be so given the circumstances in which it occurred and the existence of certain laws that always act in a certain way. Cartwright sets up against this model what she calls the ‘simulacrum account’: to explain means to construct models, within a theory (to which they may not be consistent), which are formal representations of the phenomenological laws. Indeed, she sees models as simulacra of things, i.e. mental pictures or even hold-in-your-hand models that mimic features of the world. As Cartwright says ‘[the simulacrum] account says that we lay out a model, and within the model we “derive” various laws which match more or less well with bits of the phenomenological behaviour’ (Cartwright 1983, p. 161). Once a model is deployed, phenomenological laws describe those aspects of the phenomenon that are siphoned off by the model.

Hacking stresses that, as there are many models, often mutually inconsistent and embedded in the same theory, there can be no single explanation:

The covering-law account supposes that there is, in principle, one ‘right’ explanation for each phenomenon. The simulacrum account denies this (1983, p. 17).
For the same reasons given by Cartwright, Hacking downplays the role of explanation:

[...] explanation may play a less central role in scientific reasoning than some philosophers imagine. [...] Explanations are relative to human interests. [...] Explaining is a feature of the historical or psychological circumstances of a moment (Hacking 1983b, p. 53 my emphasis).

The hypothetical construction of analogical models is, together with measuring and exploring relations between phenomena, part of the laboratory thinking and doing. According to Hacking’s ideas above we should conclude that thinking in the laboratory SoT by propounding models of phenomenological laws within a given theory does not provide any valid argument for scientific realism. There is no auto-corrective mechanism that simplifies models and makes them converge on better and better descriptions of reality: we are condemned to the local truths of phenomenological laws.

7.2.2 Hacking’s entity realism

There is an important point made by Cartwright that helps us understand how Hacking defends entity realism. She considers the following inference: ‘if x explains y and y is true then x is true’. For her it would be invalid if x were a fundamental law but it would be valid in the case of causal explanation:
Suppose we describe the concrete causal process by which a phenomenon is brought about. That kind of explanation succeeds only if the process described actually occurs. To the extent that we find the causal explanation acceptable, we must believe in the causes described (Cartwright 1983, p. 4).

To illustrate Cartwright’s claim let us consider the example of a Geiger counter: a cylindrical capacitor with a processing electronics that is filled with gas. When a $\alpha$-particle passes through it, it ionizes some of the atoms of the gas and produces a current pulse that can be measured. In some Geiger counters the pulse produces audible clicks associated with the number of $\alpha$-particles. To provide a causal account of why a Geiger counter produces a click, a physicist would implicitly resort to a certain number of fundamental physical laws and eventually would describe the principle of operation in terms of phenomenological laws such as: ‘when the gas is so-and-so and the ions travel with such-and-such speed then they create an avalanche described by this equation’. In Cartwright and Hacking’s view, Maxwell equations lie but, to the extent that we find credible the explanation that the cause of the clicks is a flux of particles, we must believe that, as a matter of fact, a flux of particles enters the Geiger counter. It is in this sense that Hacking’s anecdote about what convinced him of the existence of electrons (‘if you can spray them on a niobium ball they are real’ (Hacking 1983b, p. 22)) must be understood. Like in the case of the Geiger counter, if the existence of electrons is the only plausible cause of the effect of spraying, it must be true that they are real. Ultimately, causal reasoning provides good grounds for concluding that unobservable entities exist.
In order to be confident about the existence of a cause of a given effect - stresses Hacking - causal reasoning must be integrated with experimenting. He distinguishes two types of experiments: experiments-with and experiments-on (Zeidler and Sobczynska 1995/1996) (Hacking 1995/1996). For example, when in 1908 the physicist R.A. Millikan (1868-1953) determined the charge of the electron by experimenting on electrically charged oil droplets he was performing experiments-on. Conversely, electron diffraction, which makes it possible to infer the structure of a crystal by firing at it electrons and observing the interference pattern, is a kind of experiments-with. Experiments-on are experiments that aim to measure the properties of the putative entity. Experiments-with are manipulations of an alleged real entity in order to find out something else or create a new phenomenon such as the electron diffraction. Hacking directs the reader's attention to experiments-with and to the fact that a one-off experiment would not dispel our doubts about the most likely cause of a given effect. He maintains that when we regularly manipulate a putative entity in order to investigate other aspects of nature, that is when we routinely perform experiments-with, we are justified in thinking that the entity exists:

Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be 'observed' are regularly manipulated to produce new phenomena and to investigate other aspects of nature (Hacking 1983b, p. 262)
I wish to stress two points: the first is that Hacking is claiming that the experimental realist knows that theoretical entities exist:

My realism about entities implies both that a satisfactory theoretical entity would be one that existed (and was not merely a handy intellectual tool). That is a claim about entities and reality. It also implies that we actually know, or have good reason to believe in, at least some such entities in present science. That is a claim about knowledge (Hacking 1983b, p. 28 my emphasis)

The second point is that, since experimenting-with involves the ‘creation’ of new phenomena, it must be considered part of the laboratory way of doing. So we can say that it is by doing in the laboratory SoT, and in particular by using causal reasoning and experimenting in order to elicit new phenomena, that experimenters find the most likely cause of a certain effect.

7.2.3 Causal Effects as Style-independent Sentences
Consistently with his theory anti-realism, for Hacking experimentation is free from theory as far as it has the power to justify our beliefs in the existence of theoretical entities. He argues that an important part of experimentation is theory-free by making different points, which are relevant to the SoTs project, as I shall show here and in the next section. First of all, he points out that there is no theory of a theoretical entity to which all the experimenters are realistically committed (Hacking 1983b, p. 264). Within the same experiment different models coexist and each of them enables us to make calculations relevant to only one particular aspect
of the particle. To the question 'is there not a common core of theory, the intersection of all the different theories?' Hacking answers: 'there is a common lore, not a common core' (Hacking 1983b, p. 264). Furthermore:

We design apparatus relying on a modest number of home truths about electrons, in order to produce some other phenomenon that we wish to investigate (Hacking 1983b, p. 265)

The use of colloquial terms such as 'common lore' or 'home truth' is meant to downplay the theoretical analysis of experimentation: one of the aims of Representing and Intervening was to urge philosophers, too much concerned with theories, to focus on practical aspects of experiments. Hacking's point is that, although groups of experimenters 'may hold different and mutually incompatible accounts of electrons (Hacking 1983b, p. 264) they share a way of doing and a tacit knowledge of the behaviour of apparatuses, of various effects given certain conditions and of '[causal properties] derivative on properties like spin, mass, and charge [which] are the best candidates for home truths' (Morrison 1990, p. 20). Sentences such as 'a beam of electrons is halted by an aluminium plate' or 'electrons with features such-and-such spiralling in a magnetic field emit radiation so-and-so' are example of the shared language of experimenters not too much infected by theory, so to speak.

More importantly, I wish to remind the reader that in subsection 2.5.4 I have explained that for Hacking 'there have been important observations in the history of science which have included no theoretical assumptions at all' (Hacking 1983b, p. 176). To illustrate this point he gives the example of Herschel's discovery
of the infrared radiation of sunlight (Herschel 1800b) (Herschel 1800a, 1800c). As I have explained, Hacking is adamant that the corpuscular theory, in which the physicist Herschel believed, did not infect several statements of Herschel’s research on radiant heat. For example Herschel wrote:

When I used some of them [the filters] I felt a sensation of heat, though I had but little light, while others gave me much light with scarce any sensation of heat (Herschel 1800b)

Commenting on this sentence, by which Herschel described the sensation of heat caused by light rays coming through different filters, Hacking wrote that ‘[there is no] better sense-datum report than this in the whole of physical science’ (Hacking 1983b, p. 176).

Herschel’s sentence is to be considered style-independent. Indeed, as I have explained in subsection 2.5.4, Hacking contrasts it with another Herschel’s sentence, which for him is style-dependent: ‘The heat which has the refrangibility of the red rays is occasioned by the light of those rays’. Ultimately, the distinction between these two classes of sentences (style-independent and style-dependent sentences) mirrors the distinction theory-observation put forward at the level of his argument for realism: a sentence infected by theory (e.g. ‘the heat has the refrangibility…’) is supposed to be style-dependent on the laboratory SoT whereas a theory-free sentence ‘my skin is warmed’ is supposed to be style-independent.

Notice now that the sentence ‘my skin is warmed’ can be also viewed as a sentence that describes a causal effect of the light rays. Therefore, in the case of Herschel’s study of radiant heat, a causal effect is described by a style-
independent sentence. As I am going to argue, it is possible to say that this is a general fact: for Hacking, the causal effects of entities are described by style-independent (observational) sentences. For example, one can imagine that during the most important experiments that mark the history of the electron these style-independent sentences, which describe causal effects, have been pronounced:

a) 'The glass tube glows' (Thomson experiment)

b) 'The oil drops are in mechanical equilibrium' (Millikan experiment)

c) 'There is a track in the photographic plate (Wilson experiment)

Experimenters of different groups communicate by using sentences that can be more complex than these above and that can describe, in addition to effects, causes and properties of causes. For example, one can imagine these alterations of the sentences a) b) c):

'Electrons emitted by the cathode make the glass glow'

'The value of the charge on the droplets in mechanical equilibrium is always a multiple of the charge of an electron'

'That is the track of a positron in a photographic plate'
These sentences refer to causes (entities) and properties of causes (charge) in addiction to effects. For example, the first sentence states that the cause of the glowing is an entity called electron; the second that the droplets are dragged upward by the field because they are charged with particles called electrons; the third that the track is the effect produced by an entity that have opposite charge to the electrons.

Hacking is to be interpreted as maintaining that the sentences a) b) c), which describe causal effects, are style-independent (observational). Indeed, speaking of the sentence ‘That is the track of a positron in a photographic plate’ Hacking writes:

> There is a tendency to infer from stories like that of the positron that anyone who reports, on looking at a photographic plate, ‘that’s a positron’ is thereby implying or asserting a lot of theory. I do not think is so (Hacking 1983b, p. 179).

In this passage Hacking is implicitly expressing his disagreement with those philosophers, such as N.R. Hanson, Kuhn and Feyerabend, who put forward theses of theory-ladenness by claiming, for example, that observational reports assert theoretical presuppositions. I have already discussed in section 6.3.1 these differences of views between Hacking, Feyerabend and Kuhn. What interests me here is that the passage above clearly supports my interpretation: for Hacking sentences which describe causal effects are to be considered observation sentences and, therefore, style-independent. After all, to characterize the causal interactions of unobservable entities with other parts of nature in terms of observation
sentences is crucial for Hacking's argument for realism. Indeed, only if causal effects can be characterized without theory can someone be antirealist about theories and believe in the existence of an entity only on the basis of its causal effects.

Hacking's point that sentences such as 'that is the track of a positron in a photographic plate' are observation sentences contrasts with what many scholars think. For example, this is what the historian of science Gerald Holton writes re the particle tracks in a photographic plate:

our naked eyes would see only an unconvincing curlicue; but the mind's eye sees, through the use of a Feynman diagram version of the same phenomenon, that a neutrino scatters an electron without any change of charge (Holton 1996, p. 92).

Holton's point is in fundamental conflict with Hacking's. Indeed, Holton is claiming that Feynman diagrams are necessary to 'see' a scattering between particles instead of a squiggle. Feynman diagrams are models that both help to visualize the relevant aspects of particle interactions (in which strong, weak and electromagnetic forces are involved) (see Wüthrich 2012) and help to calculate their more interesting properties. They were introduced in the late 1940s by the physicist Richard Feynman for simplifying complex calculations in quantum electrodynamics (QED), the theory that explains the electromagnetic force at the quantum level. In other words, Holton is claiming that certain models are the presuppositions held by investigators who deal with the causal effects of
electrons, i.e. they infect those elements of experimentation that Hacking considers free of theory.

To summarize the content of the last two subsections, Hacking claims that we *know* that unobservable entities exist. Regular manipulation is part of the way of doing of the laboratory SoT and for Hacking it is the strongest evidence for the existence of an unobservable entity. What the expression regular manipulation of an entity wants to convey is the idea that we perform experiments *with* that putative entity in order to produce macroscopic causal effects. These effects are described by *style-independent* sentences, which may be used for forming more complex sentences, not so much infected by theory, which include causes and properties of causes. Consequently, it is possible to say that an important part of experimentation, which *in primis* includes causal effects, is theory-free. By making this point Hacking is able to defend his realism about entities while being antirealist about theories. Finally, since he is antirealist about theories regular manipulation is also *the only one* justification he gives for realism about theoretical entities.

7.2.4 Putnam’s Theory of Reference and Experimental Realism

In *Representing and Intervening* Hacking asks:

> [...] is not the substance of the theory about positrons among the truth conditions or truth presuppositions for the type of utterance that we may represent by ‘that’s a positron’? (Hacking 1983b, p. 179)
His answer confirms what I have explained in the previous section, i.e. that ‘that’s a positron’ is an observation sentence:

Possibly, but I doubt it. The theory might be abandoned or superseded by a totally different theory about positrons, leaving intact what had, by then, become the class of observation sentences represented by ‘that’s a positron’ (Hacking 1983b, p. 179).

However, the way Hacking replies also points to another problem: how can we be sure that scientists holding competing or successive theories are referring to the same entity, the positron? This difficulty is closely related to the notorious issue of incommensurability between different theories debated since the 1960s: as I have explained, thinkers such as Kuhn and Feyerabend maintained that certain terms take on different meanings if used in two incommensurable theories. In order to overcome this difficulty Hacking wedded himself to Putnam’s version of causal theory of reference32 because, as I am going to explain, it suggests that scientists having competing theories about the positron ‘may still be talking about the same thing’ (Hacking 1984, p. 157). In 1970s the philosophers Hilary Putnam and Saul Kripke proposed causal accounts of how terms acquire referents. They attacked the descriptivist theory of meaning according to which meanings of names coincide with the descriptions associated with them, whereas their referents consist of the objects that satisfy these descriptions.

32 Putnam’s theory has been criticized by many scholars (see for example Dupré 1996, pp. 17-36). Hacking is aware of these criticisms. However, he has made it clear that, although he does not believe Putnam’s theory literally, he is happy to employ his account (Hacking 1984). He has extensively written on Putnam’s theory of reference in (Hacking 1983b) and (Hacking 2007c).
As the descriptions of a name change (e.g. 'water is a colourless liquid' or 'water is that substance made of atoms of hydrogen and oxygen') the descriptivist view seems to imply that its reference changes as well (as if, by the two descriptions of water, we did not refer to the same thing).

Since Hacking claims is that a positron is something in the world, regardless of the theories or descriptions we have of it, the descriptivist view does not provide the support he needs.

According to Putnam the meaning of a word has four components: the word’s syntactic marker, the semantic marker, the stereotype and the referent. The syntactic marker states the grammatical role played by the word, e.g. subject or predicate; the semantic marker points to the category of things the word applies, e.g. the word ‘horse’ applies to mammals and quadruped; the stereotype is the conventional idea people have about a certain thing, that is the description commonly accepted, e.g. ‘zebra’ is thought of as a striped animal; the referent of a word is the thing or the class of things it refers to, e.g. the reference of ‘water’ is a certain kind of stuff individuated by the chemical formula H2O. Only when the reference of a term has varied is it legitimate to state that the meaning has changed. For example, the concept ‘natural kind all of whose members live under water, breath through gills, etc.’ does not fit the natural kind ‘fish’ because some fish do not breath through gills. However, according to Putnam it would be wrong to conclude that the concept ‘does not correspond to the natural kind Fish’ (Putnam 1975, p. 196). If anything, the concept of fish changes as our knowledge of the organisms develops. Yet Putnam maintains that it still corresponds to the same natural kind.
To argue for the invariance of reference Putnam put forward the concept of *introducing event*: everyone who uses a term is connected by a certain kind of causal chain to a situation in which a description of that term is given, e.g. an event that singles out 'positron' as the entity with certain features and responsible for certain effects. For example someone could tell me that the positron is that object that makes a Geiger counter produce a click, leaves a track in a gas chamber, has the same mass of an electron but different charge; hence, each of my later uses will be connected with this event. Furthermore, even the use of any other person who has been told about the term 'positron' by me will be causally linked to its being in my vocabulary and, therefore, to my original introducing event (Putnam 1975, p. 200).

Kripke argued that when the name of a referent is fixed by an act of naming it becomes a 'rigid designator'. By this expression he meant that, for example, the name George Washington refers to the same person in all the possible worlds whereas the expression 'the man who was the first president of the United Stated' could in principle refer to another person. If two rigid designators are found to designate the same object in the actual world it must be true – argued Kripke - that they designate the same object in all possible worlds. In other terms, if it is true that two rigid designators are found to refer to the same object then it must be necessarily true that they designate the same object. He famously gave the following example: suppose that Hesperus is the name that it is given to a star seen in the evening and Phosphorus is the name given to the same star seen in the morning. Suppose also that astronomers find out that Hesperus and Phosphorus
designation the same star, Venus. Kripke concluded that it is necessarily true, i.e.
true in all possible worlds that Hesperus=Phosphorus (Kripke 1972, pp. 213-215).

If we return to the initial question of this section: if a theory about the
positron is abandoned or superseded by a totally different one are we always
referring to the same object, the positron? The causal theory of reference suggests
that the answer is in the positive. Indeed 'positron' is a rigid designator: everyone
who has the minimal linguistic information about 'positron' and uses the word in
a way that is causally connected to an introducing event will refer to the positron,
even if she advances new theories or comes to possess additional information
about the properties of the positron. As in the case of the star Venus, if two groups
of researchers refer to same object, the positron, at a certain point in time they will
always refer to it in all possible worlds, for example in cases in which one of both
of them will advance radically new theories about positrons based on new
findings. There is no possible situation in which 'positron' designates something
else. In conclusion, we now have a deeper sense of why Hacking considers
Putnam's casual theory of reference as the logical skeleton for his experimental
realism view. He needs a notion of reference that is not defined in terms of our
theories and conceptions.
7.3 The Incommensurability of the Styles of Thinking and Doing

Now that I have offered an account of the position Hacking attempts to defend, I return to my original question: whether there is a criterion independent of any SoT by which to judge the claims of knowledge made by a community that has a particular SoT. Until it is found an example of a claim in a particular SoT whose justification is relative to that very SoT, it is not possible to conclude that the SoTs project implies epistemic relativism. I think that the experimental argument is a good candidate for representing such an example: Hacking maintains that we know about the existence of unobservable entities, in particular we are justified in believing so; I shall argue that his justification is not atemporal and universal but relative to the laboratory SoT. Differently stated, my point will be that Hacking's experimental argument might justify realism about theoretical entities in the laboratory SoT (if one agrees with Hacking); however, outside the laboratory SoT realism about theoretical entities is unjustified. Before focusing on Hacking's entity realism and arguing for this thesis I want to point out that Hacking's theory anti-realism makes it impossible to rank different SoTs in terms of their capacity to explain why certain phenomena happen.

7.3.1 Incommensurability and Theory Anti-realism

Consider the example I have given in section 4.3: the distance of the Moon from the Earth can be calculated by using the geometrical SoT or the laboratory SoT. As far as the calculation of the Moon distance is concerned, there is no much difference
between the postulational SoT adopted by Greeks and our laboratory SoT -- one has no strong reasons to conclude that a SoT is 'better' than another. However, if we take a realist attitude, unlike Hacking, and interpret the gravitation law as a law that is approximately true then, as far as the study of the Moon is concerned, the laboratory SoT can be considered 'superior' to the geometrical SoT. Indeed, in that case not only does the former provide the distance to the moon but also explains why the value of the distance is what it is; on the contrary, the geometrical SoT provides no explanation of why the Moon rotates at a certain distance with its specific period.

In contrast to theory realists Hacking's view towards the gravitational law is instrumental: the law 'lies' but represents a useful devise that helps to calculate the distance to the moon. It does not provide the explanation of the phenomenon. So Hacking would conclude that the laboratory SoT does not get any closer to the truth than the postulational SoT -- for him, as far the calculation of the distance to the moon is concerned, there can be no independent criterion for ranking these two SoTs.

The crux of the matter is that Hacking espouses Cartwright's view that 'rendered as descriptions of facts, fundamental laws are false; amended to be true, they lose their fundamental explanatory force' (1980, 75). Consequently, if Cartwright is right, we are left with no answer as to why the distance to the moon is what it is: the gravitational law has to be amended in order to be used for the calculation of the distance to the moon, therefore it does not explain. Alan Chalmers raised a similar point when he remarked that if we deny an explanatory
role to the fundamental laws we are at loss to say what governs the world outside experimental situations. His example concerns the orbit of Halley's comet:

The first sighting of the comet, by a Voyager spacecraft, enabled the predicted orbit to be corrected, thereby facilitating later terrestrial sightings. The regularity predicted on the basis of Newton's laws lied. Nevertheless, Halley's comet did return very nearly as scheduled. To echo Cartwright, if Halley's comet is not governed by Newton's laws, then I do not know why it returned as and when it did. (Chalmers 1999, p. 8-9)

To conclude, Hacking's theory antirealism leads to a view in which it is impossible to rank different SoTs in terms of their capacity to explain phenomena. On the other hand, Hacking's experimental realism represents an interesting example of a claim that might be universally and atemporally justified. I am going to show that this idea is fallacious.

7.3.2 The Incommensurability of Styles of Thinking and Doing: First Argument

As I have done in section 4.4, for the argument's sake I shall now imagine a situation in which a 'speaker' tries to communicate with a 'hearer' who has a different SoT. Speaker and hearer are to be imagined as belonging to different communities, which may be separated in time. Suppose that the speaker, who adopts the laboratory SoT, expressed the sentence: 'the muon is real'. Can a hearer who belongs to a community that does not adopt the laboratory SoT understand
that sentence? One problem is how the hearer could attribute a meaning to the term ‘muon’. Putnam’s causal account of reference can be applied to this problem. Although the hearer would have no knowledge of the working of experimental apparatuses, she could well be able to perceive the sound of a Geiger counter or see the effects of the track in a photographic plate; she could also acquire some information about other properties of the muon. On Putnam’s view, this can be defined as an introducing event, i.e. an event in which an adherent of a different SoT would grasp that the term ‘muon’ designates an entity responsible for certain effects and, for example, capable of motion and negatively charged. From that moment on, both the speaker and the hearer would be connected by a causal chain to the act of naming the muon. In sum, Putnam’s account fulfils our desideratum – it makes the reference of the term ‘muon’ independent of the SoT in which it is used.

Suppose now that the speaker justified the existence of the muon as Hacking does: she could say that the muon exists because it can be regularly manipulated in order to produce causal effects. Hacking assures that she would also be able to describe the causal effects in terms of style-independent sentences: for example, ‘that’s the track of a muon in a photographic plate’.

Ultimately, if the speaker made the claim ‘the muon is real’ and justified it by using Hacking’s experimental argument, the relevant sentences that describe the causal effects would have a truth-value in a SoT different from the laboratory SoT.

Then, is the hearer justified to believe in the claim that the muon is real? My answer is in the negative. Indeed, the meaning of the term ‘muon’ may not depend on the presuppositions of the laboratory SoT, Putnam account implies, but
the criteria for the existence of a muon do depend on the presuppositions of the laboratory SoT even if the causal effects can be described in terms of style-independent sentences. I shall explain. What the speaker proposes is to assess the existence of the muon by using devices that create new phenomena and involve the way of doing of the laboratory SoT. Whether or not the speaker uses style-independent sentences to illustrate his justification, the hearer finds herself in rather an awkward situation that may well be likened to that described by Shapin and Schaffer. Indeed, a hearer coeval with Hobbes could object to the speaker that God has given us enough phenomena and that we should not get involved in dubious ones. The speaker might well describe the causal effects by using style-independent propositions but this would not make her argument plausible for the hearer — the presuppositions of the laboratory SoT would be implicit in her argument. Indeed, when illustrated in detail, the speaker’s point would amount to say that, by adopting the way of doing of the laboratory SoT and its methods for finding out, it is possible to perform experiments with the muon in order to create new phenomena; these new phenomena, which are represented by macroscopic effects described in terms of observational sentences, are evidence of the existence of the muon. However, the speaker’s way of doing and her methods for finding out would be no presuppositions of the hearer’s SoT; nor could creating new phenomena represent evidence of the existence of an entity. I have already quoted Hacking as saying that ‘the substance of the theory about positrons [is not] among the truth conditions or truth presuppositions for the type of utterance that we may represent by ‘that’s a positron’’. This is beside the point. If anything, the crux of the matter is that, echoing Hacking, the ‘substance of the laboratory SoT’ (i.e. its
presuppositions) is among the truth presuppositions for the utterance ‘that’s a muon’. It is true that ‘the object called “Geiger counter” made a click’ is a style-independent sentence but it is the very practice of using the Geiger counter for producing new phenomena that does not belong to the presuppositions of SoTs different from the laboratory SoT. To use a Geiger counter in order to elicit a phenomenon does not count as evidence of anything for a member of a community that does not have the laboratory SoT. Sentences such as ‘the object called “Geiger counter” has made a click’ are not really projectable in a different SoT: they cannot be used as they are used in the laboratory SoT: in the laboratory SoT these sentences can be used to illustrate an argument for the existence of the muon; but that argument implicitly assumes a criterion of evidence that is not part of the presuppositions of different SoTs. It is as if the sentences that describe the causal effects of the muon can be admitted into the hearer’s world but nothing significant can be claimed about them. They are projectable in the ordinary language of the hearer for describing the consequences of certain actions performed by others but cannot be employed to present the argument of experimental realism: Hacking’s argument is relative to the laboratory SoT. In sum, if the SoTs are characterized as Hacking does, the knowledge of unobservable entities in the laboratory SoT has no foundations apart from the very presuppositions of the very laboratory SoT.
7.3.3 Supporting the First Argument: An Historical Example

To substantiate my argument I shall provide an historical example concerning Galileo’s discovery of Jupiter’s satellites. The example will show that, although the ‘speaker’ (Galileo) used style-independent sentences to deploy his argument, the ‘hearers’ (Aristotelian scholars) rejected it on the basis that the speaker’s kind of evidence and way of doing was not acceptable.

The last part of Galileo’s *Sidereus Nuncius* (Galilei 2008 [1610], pp. 299-319) reports that on the seventh of January 1610 Galileo ‘realised that close to Jupiter there were three stars, little in truth, but very bright [...] they were lying in a straight line and parallel to the ecliptic’ (Galilei 2008 [1610], p. 299). He went on observing in the successive nights until early March 1610 and detailed the relative positions by the simple insertion of linear drawings embedded within his published text. These numerous observations of Jupiter did not conform to the principles of Aristotelian cosmology but supported the Copernican sun-centred hypothesis of the world system in which Galileo believed. However, although Galileo did have Copernicanism in mind when he discovered the four moons, he did not mention this theory at all in his treatise, since it was forbidden; he just wanted to offer to the reader a visual experience.

Three points can be noted. Firstly, even if it had turned out that Copernicus’ model was wrong, this fact would not have called in question Galileo’s detailed observations. From Hacking’s perspective this fact could be expressed by saying that, even if theories change, different scientists can still refer to the same things, represented by the drawings in the *Sidereus Nuncius*. Secondly, the relative positions of Jupiter and its moons are described in terms of what Hacking would
call observation sentences: in many cases they could be viewed as captions for his drawings. From Hacking's perspective this second point suggests that the claim for the existence of an observable entity can be substantiated by pure observation sentences. Finally, as Biagioli stressed,

To Galileo [...] the evidence that counted was not a snapshot of individual luminous dots around Jupiter, but the 'movie' of their motions. It was a long chronological perspective that linked his string of observations and turned the luminous bodies near Jupiter into satellites, not fixed stars (Biagioli 2006, p. 103):

In other words, Galileo's observations presupposed a commitment to conducting observations over several days so as to differentiate the periodic motions of the moons from other visual patterns. In this sense his evidence was inherently historical. Anyone who had looked through a telescope for only a few minutes might have legitimately believed that the bright points were artefacts of the telescope, rather than satellites. But anyone who had observed the bright points for many days would have concluded that they followed over time the exact pattern of rotating satellites and that, therefore, they were not artefacts such as double images and colour fringes (which were present in early telescopes). This third point implies that one needs to commit oneself to a way of doing, as it happens in my imaginary discussion between a speaker who adopts the laboratory SoT and a hearer who adopts a different SoT: a one-off observation is not sufficient to realise and find out that four bright dots are satellites rather than artefacts; one needs a great incentive to take time to learn how to see through a telescope and to
observe for a long period. Above all, one needs to accept that the telescope, an instrument rarely used before to observe the sky, represents a plausible way of finding out.

Ultimately, these three points above suggest that, the differences notwithstanding, there are striking analogies between my imaginary example in the previous subsection and the concrete case of the discovery of Jupiter's moons. What I want to show now is that just as the imaginary hearer could not accept the speaker's experimental argument on the ground that it was based on presuppositions that were alien to her so Galileo's opponents could not accept the claim that Jupiter's moons exist on the ground that they did not accept his presuppositions.

Few months after the publication of the *Sidereus Nuncius* Galileo moved to Florence where his discoveries were met with disbelief by the court scholars. One of them, the Aristotelian philosopher Cesare Cremonini (1550-1631), did not want to look through the telescope. At that time it was thought that the physics of sky was different from the terrestrial physics, in particular that the laws of light interaction with terrestrial objects could not be extended to the sky (see Feyerabend 1988 [1975], Chapter 10). Consequently, Cremonini was sceptical about the idea that the telescope could provide evidence for the existence of celestial objects. He had little incentive to invest time in Galileo's observations. As Biagioli explains, 'an observation would have been a very unwise investment of time and resources for someone [i.e. Cremonini] of his disciplinary affiliation and professional identity' (Biagioli 2006, p. 113). In other words, Galileo's methods of finding out, the evidence he provided and his way of doing was so different from
Cremonini's that the latter would have lost his professional identity if he had invested time in doing what Galileo had done.

Unlike Cremonini other scholars did look through the telescope, but only for a short time. The brevity of the observation was fatal for them: for example, the mathematician Martin Horky saw minute spots close to Jupiter on April 24 and 25 in exactly the same configuration described by Galileo but he concluded that they were an optical illusion. That is not surprising. One needs to have Galileo’s style of doing for committing oneself to refining telescopes and observing bright spots for months.

In *Life of Galileo* (Brecht 2007 [1938]), the playwright Bertolt Brecht (1898-1956) presents a scene in which Galileo invites a philosopher and a mathematician, who act the part of the sceptical scholars, to observe Jupiter's satellites. The dialogue below is emblematic of the epistemic incommensurability between Galileo’s way of thinking and doing and that of the two scholars. Although Galileo’s arguments were not infected by theories, Hacking would say, the mathematician did not accept the kind of evidence offered by Galileo.

**GALILEO** If you gentlemen are agreeable, we shall begin with the inspection of the satellites of Jupiter, the Medicean stars.

[...]

**THE PHILOSOPHER** [...] before we apply ourselves to your famous tube, we should like to request the pleasure of a disputation: Can such planets exist?

**GALILEO** Your Highness, would you care to observe those impossible and unnecessary stars through the telescope?
THE MATHEMATICIAN One might be tempted to reply that if your tube shows something that cannot exist it must be a rather unreliable tube.

GALILEO What do you mean by that?

MATHEMATICIAN It certainly would be much more to the point, Mr. Galilei, if you were to tell us your reasons for supposing that there can be free-floating stars moving about in the highest sphere of the immutable heavens.

THE PHILOSOPHER Reasons, Mr. Galilei, reasons!

GALILEO My reasons? When a look at these stars and my calculations demonstrate the phenomenon? This debate is getting absurd, sir.

THE MATHEMATICIAN If it were not to be feared that you would get even more excited than you are, one might suggest that what is in your tube and what is in the sky might be two different things (Brecht 2007 [1938]).

In the passage above Galileo insists that for accepting his reasons, that is, his justification for the existence of Jupiter's moons, it is sufficient to look through the telescope. The two scholars, though, insist in asking other reasons because they do not accept his kind of evidence. Even if Galileo can offer an argument in terms of observable sentences – and, indeed, observations – his opponents do not accept his claim because the kind of evidence he offers does not belong to their way of thinking and doing.

It took some dozen of years before Galileo's truth was accepted. Galileo's milieu could do little to favour the establishment of new standards of truth. Indeed, Biagioli wrote that 'lack of consensus about style of argumentation and standards of
evidence as well as the scant interdependence among the members of this field hindered closure of the debate' (Biagioli 2006, p. 97 Italic is mine). Of course, had Galileo's discoveries been discussed within an intellectual milieu of astronomers, as Boyle's ones were discussed within an elite of people who explored nature in the same public place, his kind of evidence and his way of doing would not have been called into question.

I conclude by summing up my point. Hacking claims that we are justified in believing that unobservable entities exist when we regularly manipulate them. I counter that, if his characterization of SoTs is correct, then his justification is relative to the laboratory SoT. My argument has followed these steps: I have pointed out that regular manipulation is a way of doing that is not part of the presuppositions of another SoT. Even if causal effects obtained in experiments-with can be described by style-dependent statements, the experimental argument implicitly assumes the presuppositions of the laboratory SoT: methods of finding out, ways of thinking and doing and types of evidence. Therefore, the experimental argument is not a valid justification for the existence of a particle outside the laboratory SoT. To substantiate my thesis I have given an historical example in which it is not sufficient that a claim is justified in terms of observational statements in order to be accepted. Galileo's claim is dismissed a priori by Aristotelean scholars because it relies on a kind of evidence alien to their mental horizon.
7.3.4 The Incommensurability of Styles of Thinking and Doing:

Second Argument

Up to now, I have argued that Hacking’s argument for realism is relative to the laboratory SoT but I have not criticized it: in my imaginary example I have supposed that the claim that unobservable entities exist can be defended as Hacking does. In this subsection, I shall use the historical account of the discovery of the muon provided by the historian of science Peter Galison in *How the Experiments End* (Galison 1987) for assessing Hacking’s experimental realism. My analysis will show that, in contrast with what Hacking maintains, theoretical style-dependent sentences in the laboratory SoT are necessary to justify the claim that unobservable entity exist. Since style-dependent sentences in the laboratory SoT have no truth-value for a community that adopts a different SoT I shall conclude that, in this case too, the justification of an observable entity is relative.

The history of the discovery of the muon is intertwined with that of the research on the cosmic rays, high-energy particles of galactic and extra-galactic origins, most of them protons, which reach the Earth’s atmosphere. It was by investigating the compositions of cosmic rays that muons were detected for the first time. Today it is an undisputed fact that, when cosmic rays enter the atmosphere, they collide with its molecules and produce a so-called shower of ionized particles, of which some decade into muons, able to reach the surface of our planet. A shower is a cascade of secondary particles with smaller quantities of energy than the incoming particle, each of them producing other cascades of billions and billions of particles. As a proton collides with an air molecule, it
creates pions, protons and neutrons; pions decay in particles such as muons, which are penetratin particles, i.e. they are able to reach the surface of the Earth. Muons were unknown particles to physicists until the last years of the 1930s, when numerous experiments and theoretical considerations helped to settle the dispute about their existence.

The physicist Robert Millikan (1868-1953), one of the pioneers of this research, believed that the primary cosmic rays impinging the atmosphere were gamma rays, i.e. photons of high frequency penetrating the atmosphere isotropically from space. Gamma rays are the most penetrating of all radiations because they lose little energy in interactions with matter. In the 1920s Millikan, persuaded that in the cosmic rays there were no very penetrating charged particles, measured the intensity of radiation (he was convinced of measuring photons) as a function of penetration depth for different energies and concluded that his hypothesis was correct: for him the penetrating particles were gamma photons arriving from outside the atmosphere. However, in 1929 Walther Bothe (1891-1957) and Werner Kolhörster (1887-1946) set up an experiment that challenged Millikan's conclusions. By using two Geiger tubes separated by a block of lead and measuring the coincidences (when the same particle crosses both the counters and make them produce a click) they found evidence for the passage of a single charged particle through the intervening lead (Galison 1997, p. 441). Indeed, by measuring how the coincidences decrease as the thickness of the intervening lead is modified, it is possible to rule out the possibility that the cause of the coincidences is the passage of gamma rays. This was a conclusion based on the macroscopic effects of the investigated entity and on the knowledge of how gamma rays are attenuated.
by plates of different thickness. However, all Bothe and Kolhörster came to know was that the particles were charged; they acquired no knowledge about its real identity.

Later on the physicist Bruno Rossi (1905-1993) improved the apparatus and was able to confirm with more certainty Bothe and Kolhörster’s result. In the following years three independent experiments (see Swann 1961, p. 812) confirmed the existence of the so-called ‘latitude effect’: the variation of the intensity of cosmic rays with the latitude. This is the result of the Lorentz force generated by the terrestrial magnetic field on charged particles. This force has a full component when the speed of the particle is orthogonal to the Earth’s magnetic field (at the equator) and is null when the speed is parallel to it (at the Poles). The latitude effect supported the idea that cosmic rays could not consist of gamma rays, because on photons the Lorentz force has no effect since they are not charged. To illustrate the point of the experiment, that it is impossible that the penetrating particles are not charged, it is necessary to use the Lorentz force, a fundamental law, or at least phenomenological laws derived from it.

The latitude effect was the reason of a long dispute between Millikan and Arthur Compton (1892-1962) (Bertolotti 2013, pp. 85-95), who had conducted a large-scale geographical survey: in the beginning the former, who had made experiments himself, insisted that there was no latitude effect, then he tried to explain it under wrong assumptions, i.e. by claiming that primary photons could knock off some electrons from interstellar matter. Millikan also refused to accept the results of Bothe and Kolhörster’s experiments as well as Rossi’s on the basis that the internal processes inside the Geiger counter were misunderstood. As
Galison reports, in 1934 Millikan and three colleagues published a paper on *Physics Review*, in which they asserted:

[The counters' coincidences] cannot in general be due to the passage of one charged article through both counters and the intervening lead, but must rather be due to some mechanism but which a photon can release successively along, or in the general neighbourhood of, its path a number of different particles whose separate but practically simultaneous action on the two or more counters is responsible for the observed coincidence (quoted in Galison 1987, p. 95).

The way Millikan challenged the experiment is reminiscent of the criticism of Galileo’s opponents. The analogy lies in this fact: the scholars of Galileo’s time thought that the bright spots were not satellites but rather artefacts produced by the instrument, of which there was no accurate knowledge; similarly, Millikan and colleagues suggested that the coincidence was not the effect of a charged particle but that of a mechanism inside the counter produced by a gamma ray. In both cases, doubts about the internal processes of the instrument were a reason for not believing in the claim of the existence of an entity. Of course, there was an important difference between the two disputes: Galileo’s opponents contested the use of the telescope as a form of evidence in principle and did not share with Galileo the same way of thinking and doing; on the contrary, Millikan and Compton shared the same SoT and the former did accept the use of the Geiger counter as a instrument of investigation. This difference notwithstanding, in both
the situations it was thought that the instrument was misleading. I shall return to this point later.

In the light of the experiments performed in the 1930s it became clear to many physicists that the primary cosmic rays impinging the atmosphere were protons and that the coincidences at the Earth's surface were caused by charged particles. Millikan and others were not convinced, though. About this time or immediately following it, several phenomena concerning the interaction between protons and the molecules of atmosphere as well as the production of new particles became clear. In 1934 Bruno Rossi recognized the phenomenon of air shower: the primary cosmic rays when impinge the atmosphere produce a cascade of different particles (Rao and Sreekantan 1998, p. 5). However, whether or not a new kind of particle was the cause of the coincidence remained a mystery: it was thought by some that it was an electron.

The growth in complexity of theoretical physics became increasingly crucial for tackling the problem. New theoretical descriptions were important to find out the elementary processes that constituted the phenomenon of showers such as decays, processes of pair creations and radiation. For example, on the basis of these speculations the possibility that certain known particles could reach the surface of the Earth was ruled out. In particular, the theoretical physicists John Carlson and Robert Oppenheimer (1904-1967) modelled the showers by treating them as a diffusion process similar to the expansion of a drop of ink in a glass of water. In fact, they described the showers with phenomenological equations within the quantum theory. Their conclusion was that, either these equations were inapplicable in the domain of cosmic rays energies, or the actual penetration of
these rays had to be ascribed to the presence of a component either than electron
(and photon) (Galison 1987, p. 95). Carl Anderson (1905-1991) and Seth
Neddermeyer (1907-1988) made measurements of the energy loss of particles
occurring in the cosmic ray showers that led to rejecting the first hypothesis:
Carlson and Oppenheimer's theoretical estimates were applicable to cosmic ray
energies; therefore the coincidences could not be caused by electrons.

In 1937, Jabez Street and Edward Stevenson provided the first quantitative
analysis of the mass of the penetrating particle by examining the track curvature
of the particle in a double-cloud-chamber and subjected to a magnetic field.
Indeed, by measuring the radius of curvature and applying the formula of Lorentz
force they concluded that the value of the muon's mass is about 130 electron
masses.

Many experimental physicists were definitely persuaded of the existence of the
muon by this remarkable experiment. However, as Galison explains, 'among
theorists, especially, the acceptance of the new particle was greatly facilitated by
an argument from the lofty heights of quantum field theory that had been
absolutely irrelevant to the experimentalists' (Galison 1987, p. 124). Indeed the
muon had approximately the same mass of a particle predicted by a theory of
nuclear forces put forward in 1935 by the theoretical physicist Hideki Yukawa
(1907-1981). Nevertheless theoretical physicists were wrong; later on it became
clear that the muon's behaviour did not conform to that predicted by Yukawa's
particle, which actually describes the pion. It was only after the war that the muon
and the pion were distinguished, thanks to an improved theory of quantum field,
the discovery of new particles and the construction of new accelerators.
The story of the muon makes it clear that it is impossible to identify a ‘moment of discovery’, an event that convinces everybody of the existence and the identity of a particle: ‘we should envision the ending of the muon experiments as a progressively refined articulation of a set of phenomena’ (Galison 1987, p. 127). Furthermore, Galison draws from the story of the muon some conclusions that strikingly contrast with Hacking’s emphasis on experimentation. Hacking is adamant that high-level theories are not necessary to develop an argument for the existence of a theoretical entity. But Galison remarks that ‘theory itself played a complex role in ending of the cosmic ray experiment’ and adds that ‘if the first role of theory in the ending of these experiments was ostensive, demarcating a domain of phenomena, the second was constitutive’ (Galison 1987, p. 129). For example, the quantum theory of electrodynamics drew the attention on the phenomenon of penetrating radiation; and theory helped to isolate a set of possible experimental techniques as important. Beside this ostensive role, there is a constitutive role: ‘the acceptance of quantum theory of electrodynamics and its identification with shower particles was not separable from the acceptance of a new particle. These questions are complementary pieces of the same conceptual structure’ (Galison 1987, p. 129). Galison concludes:

Theory enters first ostensively, acting in broad, qualitatively way: look at penetrating particles, look at showers. [...] But theory enters a second time, now not to point out types of phenomena but to provide a quantitative analysis that plays a constitutive role in conclusion. And at this level theory is often mediated by a model which instantiates
features of the more general theory without invoking the generality of an overarching structure. (Galison 1987, p. 131).

Bearing these thoughts in mind let us return to imagining an adherent of the laboratory SoT (the speaker) who has to convince a member of a community who adopts a different SoT (the hearer) about the existence of a muon. Among other things, the speaker would need to prove that the causal effects are not artefacts of the Geiger counter as Millikan thought. Hacking might be right that Galileo did not need much theory to make his point, but the example of the muon shows that it is not possible to isolate a group of observation sentences in order to characterize this particle and convince someone of its existence. Indeed, Galison explains that particle physicists did need fundamental theories of theoretical physics to become confident about the existence of the muon. Our belief in muons is intimately connected with our ideas about the counters, the Lorentz's force and the phenomenological laws that describe the showers. So, while observation style-independent sentences were all Galileo needed for arguing for the existence of Jupiter's moons, theoretical style-dependent sentences were necessary in order to argue that the Geiger counter was really counting muons. For example, in the case of the muon, the hearer would need to resort to theoretical sentences to express the phenomenological laws of Carlson and Oppenheimer or the theoretical interpretation of the latitude effect by Lorentz's force. These sentences would have no truth-value in a SoT different from the laboratory SoT although, since the laboratory SoT is self-authenticating, they would be true by its standards of truth. Carlson and Oppenheimer's model, for example, is for Hacking not the explanation of the phenomenon of the shower but a theoretical characterization
that is true to the result of an experiment. In turn, the result of the experiment is true to the model. To sum up, the speaker would need theoretical style-dependent sentences in order to deploy her argument and the hearer could not attribute any truth-value to them. Hence, that very argument is relative to the laboratory SoT.

On the basis of the results of this section I conclude that whether or not it is possible to provide a justification for the existence of unobservable particle that is as theory-free as possible, Hacking's characterization of SoTs implies that that very justification is relative to the laboratory SoT. This conclusion has two important corollaries: the first is that Hacking's experimental realism is at odds with the relativistic implications of his SoTs project. Indeed, on the one hand Hacking's experimental realism is the claim that we know about unobservable entities, on the other hand, the SoTs project implies that experimental realism is unjustified outside the laboratory SoT. The second corollary is that by claiming that 'when the question is a live one [...] then aspects of the world determine what the answer is' Hacking does not really manage to distance himself from constructionism. Indeed, this claim amounts to saying that there is only one correct answer to a live question. But the answer to the question as to whether or not the particles posited by physicists exist cannot be considered as universally and atemporally 'correct': its being 'correct' – the SoTs project implies – is unjustified outside of a given SoT.
7.4 Conclusions

In this chapter I have analysed Hacking's experimental realism in the light of the SoTs project. Hacking's theory antirealism denies the possibility of ranking SoTs in terms of their capacity to explain phenomena. Furthermore, Hacking is not successful in showing that his entity realism is justified for a community that does not adopt the laboratory SoT. It follows that the SoTs project is in conflict with the claim that we know about unobservable. My discussion of incommensurability has focused on the laboratory SoT. In the next chapter I shall offer a more general argument that concerns any SoT.
8.1 Introduction

In this section I want to push further my argument that the SoTs are incommensurable. I believe that a critical dialogue between Ian Hacking and Ludwig Wittgenstein (1889-1951) is beneficial to this aim: by comparing and contrasting the SoTs project with Wittgenstein’s later works such as *Philosophical Investigations* (Wittgenstein 1997 [1953]) (henceforth PI) and *On Certainty* (Wittgenstein 1995 [1969]) (OC) it is possible to pinpoint the reasons that make the SoTs project relativistic in nature.

In his quest to describe the workings of human language Wittgenstein adopts different notions, of which those of ‘language game’ and ‘form of life’ will be the most interesting for my aims. As he says, ‘the term “language-game” is meant to bring into prominence the fact that the speaking of a language is part of an activity or a form of life’ (Wittgenstein 1997 [1953], PI 23). For him language is composed by different games with their own rules, e.g. giving orders and obeying them; making up stories; guessing riddles. What enables all these games to work,
and therefore the language itself to function, is a form of life, our biological, 
behavioural and sociological matrix.

The interpretation of ‘form of life’ has been widely debated. Wittgenstein 
says that ‘what has to be accepted, the given, is – one could say – forms of life’ 
(Wittgenstein 1997 [1953], p. 226), suggesting that a form of life is the ‘given’ in 
which a language has meaning. The plural in ‘forms of life’ has grounded 
relativistic readings of Wittgenstein, which have insisted on the idea that forms of 
life are to be understood as contingent, historical, dependent on culture. On the 
other hand, non-relativistic readings of Wittgenstein are suggested by other 
passages such as those in which a form of life seems to be what is common of 
human kind and makes the language possible, e.g. ‘the common behaviour of 
mankind is the system of reference by mean of which we interpret the unknown 
language’ (Wittgenstein 1997 [1953], PI 205).

Ever since Peter Winch applied Wittgenstein’s treatment of language as a 
series of games (‘language games’) to social science (Winch 1964), whether the 
later Wittgenstein should be considered an epistemic relativist has been a question 
fiercely debated. The list of the important contributions to this debate is quite long 
(see Kusch 2013, p. 37) and includes recent papers such as those of Annalisa 
Coliva (Coliva 2010) and Martin Kusch (Kusch 2013). For my purposes Coliva’s 
paper is sufficiently representative of that line of argumentation that aims to show 
that Wittgenstein is not an epistemic relativist. I shall disregard the trajectories of 
his critics’ different interpretations. My main objective in this chapter is to show 
that, despite of the similarities between Hacking and Wittgenstein, which I shall 
lay bare, Coliva’s arguments cannot be applied to Hacking so to conclude that his
project has no relativistic implications. Consequently, my case will be strengthened.

In the first section I shall point out that both the conceptions of meaning of the early and the later Wittgenstein can be recognized in the SoTs project and I shall discuss the philosophical consequences. In the second section, by relying on some analogies between the notion of ‘form of life’ and the notion of SoT, I shall argue that the SoTs project implies anti-foundationalism, i.e. the idea that each SoT is ungrounded, not epistemically justified. I shall conclude that, although in the case of Wittgenstein whether or not anti-foundationalism leads to epistemic relativism is at issue, in the case of Hacking this inference is inevitable.

8.2 Meaning, Language Games and Styles of Thinking

Hacking has recognized that only Foucault has exerted as much influence as Wittgenstein on the way he does philosophy (Hacking 2007b, p. 38). However, when in *Leviathan and the Air Pump* Shapin and Schaffer wrote that ‘[Boyle’s] experimental programme was a “language game” and a “form of life”’ (Shapin and Schaffer 1989 [1985]-a, p. 22), Hacking reacted with these words:

[Wittgenstein] certainly used 'form of life' when expressing the thought that some things are beyond or behind justification. But I
Hacking's reaction seems to me eloquent. As I have already highlighted by mentioning other passages of him, he has never wanted to leave room to relativistic implications of the SoTs project. In particular, he has always been unwilling to concede that the laboratory SoT can be likened to a form of life because that would have amounted to saying that there is no justification outside it (as he explicitly says). I shall clarify whether it is possible to draw a parallel between SoTs and forms of life further on in this section.

8.2.1 Meaning and Context: Comparing Wittgenstein and Hacking

I shall now select some Wittgenstein's ideas on meaning that I want to discuss vis-à-vis Hacking's. In the wake of the representationalist view of language put forward by Frege, Russell and Alfred Tarski (1901-1983), *The Tractatus Logico-Philosophicus* (Wittgenstein 1974 [1921]) had insisted that the truth of a proposition was determined for every possible situation. The early Wittgenstein regarded the meaning of a proposition as what must be the case in the world for the proposition to be true. In PI Wittgenstein developed an alternative view in which it is our *use* of the words, not the relation between them and the objects of the world, which provides meaning to what we say. He stressed that language has not only to do with identifying and representing but also with *activities* such as requesting (PI 2), naming colours (PI 48), inferring the intentions of an interlocutor from given expressions (PI 632). Each of these activities is a language-game, i.e. a 'language
and the actions into which it is woven’ (PI 7). For example, in (PI 2) the builder B brings to the builder A the stones that the latter has called out by using the words ‘block’, ‘pillar’, ‘slab’, ‘beam’: B has learned that when the word is such-and-such she has to perform a particular action with a specific type of stone. Wittgenstein invites the reader to think of the ‘primitive language’ (PI 2) used by the two builders as a language-game similar to ‘those games by means of which children learn their native language’ (PI 7). Ultimately,

The meaning of a word is its use in the language (PI 43)

Only from the use that we make of the word is it possible ‘to see’ its meaning – no ostensive definition can be sufficient for that aim. If we ‘look and see’ (PI 66) we realise that there is nothing common to the different uses of the same word. It is for this reason that the philosopher ought to look at the different conditions governing the employment of a concept in different circumstances, noting ‘the complicated network of similarities overlapping and criss-crossing’ (PI 66) which he calls family-resemblances. Far from proposing theories, philosophy should avoid ‘misunderstandings concerning the use of words, caused, among other things, by certain analogies between forms of expression in different regions of language’ (PI 90).

Historical epistemologists seem to follow these precepts when they consider that organizing concepts are situated. For Hacking a concept should be understood not by reflection on the human understanding but ‘in terms of the words that we use to express the concept, and the contexts in which we use those
words' (Hacking 2002, p. 35). Hacking also describes his way of doing philosophy by echoing Wittgenstein's 'looking and seeing':

All my work has turned to real life, real knowledge, real expertise. I have come to call that taking a look (Hacking 2007b, p. 36).

In drawing these parallels I have deliberately been silent on an important difference between Hacking and Wittgenstein. The former considers necessary to examine concepts in relation to contexts that are determined by *history*: the structure of organizing concepts changes and develops over time. In this sense, SoTs represent different contexts that provide meaning to words and sentences – only within a SoT does a style-dependent sentence acquire meaning. On the other hand, Wittgenstein generally contrasts ordinary language with metaphysical language. For example he says:

When philosophers use a word – for instance 'knowledge', 'being', 'object', 'I', 'proposition', or 'name' – and try to grasp the essence of the thing, one must always ask oneself: is the word ever actually used in this way in the language, which is after all its home? What we do is to bring words back from their metaphysical to their everyday use (PI 116).

Wittgenstein's main worry is to prevent us from going astray in our arguments when we use a word in ordinary language, out of its context. For him concepts are situated because, to be properly understood, their philosophical context or, in
general, their language-game, must be studied. A concept is a nonsense when it is out of any language-games we use in every day language. The language-games Wittgenstein considers are the language games that are ours, present, in use today. His concern is about how words are connected with other elements of human language, with their practices, and in particular with the language-games that provide meaning to them. For Wittgenstein even scientific language is simply a branch of the language we speak as human beings, whereas for Hacking it is that particular language that receives meaning from the presuppositions of a set of different SoTs that have accumulated throughout history.

In conclusion, both Wittgenstein and Hacking urge philosophers to pay attention to the 'contexts' that give meaning to words and sentences. However, Wittgenstein’s emphasis is on those ‘contexts’ that are represented by the countless activities and practices of our everyday language; Hacking’s emphasis is on those contexts that result from historical contingences such as SoTs.

8.2.2 Meaning and Understanding: Comparing Wittgenstein and Hacking

As I have mentioned, in the early works of Wittgenstein it is possible to find another conception of meaning, which was rejected later. He considered as a constitutive principle of meaning the memorable statement of logical positivism 'the meaning of a sentence is its method of verification', which he discussed with Schlick in 1929. At that time, Wittgenstein’s conception of meaning pointed to something external to the proposition, which provided a truth-value to it. Later on, when he abandoned the representationalist view and defined the meaning of a
word as its use in the language (PI43), he rejected the view that the meaning of a word is ‘contained’ within a particular sign or mental accompaniment of it. As Arif Ahmed explains, for Wittgenstein ‘what makes a sign of this rather than that is how we normally respond to it’ (Ahmed 2010, p. 96). Indeed Wittgenstein writes:

 [...] A person goes by a signpost only in so far as there exists a regular use of signposts, a custom (PI 198).

Even pointing at an object would not be sufficient for imparting the meaning of a word to a child who ‘came into a strange country and did not understand the language’ (PI 32). Ostensive definitions require a certain ‘background capacity’ (Ahmed 2010, p. 20) on the part of the child, which is not acquired by the mere pointing to an object. The child must be prepared to respond, say, to the definition of the word red ‘by using the word defined as we standardly use colour-words (e.g. applying it to both or neither of two chromatically indistinguishable objects in the same light)’ (Ahmed 2010, p. 20). Importantly, this pattern of usage is not contained within the definition or in some mental state that the definition helps to form in the mind of the child. It may well be the case, as Wittgenstein admits at (PI 39), that one forms an inner picture of a cube when one comes to understand the word ‘cube’. But the point is that, as one pattern of use can fit the picture in the way that just one jigsaw piece fits another, so could another.

As an example Wittgenstein shows that if a picture came before one’s mind in hearing the word ‘cube’, it would not be wrong to say that a triangular prism fits the picture. Indeed, ‘one can imagine a method of projection according to which the picture does fit after all’ (PI 139). To sum up, for Wittgenstein
understanding is not to form an image before one’s inner eye of a propositional expression. It means to think its sense, for example doing the ‘projection’, i.e. to think the sign and its method of application. Though, the picture alone does not force an application upon us (PI 140). It is use, the ‘normal response’ to the sign, which fits the picture. As Ahmed says in his comment to (PI 49):

The illegitimate idea is that the picture somehow forces an application upon us, so that a use fits the picture not in virtue of its being a normal response to the picture (i.e. a typical way of using it) but independently of how we normally respond to it (Ahmed 2010, p. 73).

The two conceptions of meaning, ‘meaning as method of verification’, as conceived by the early Wittgenstein, and ‘meaning as use’, as conceived by the later Wittgenstein, coexist in the SoTs project. Ever since Language, Truth and Reason Hacking has often used Schlick’s motto to convey a central point of the SoTs project:

we assert that until there are methods of reasoning that bear on the truth or falsehood of a scientific statement, the question of its truth and falsehood does not arise (Hacking 2009, p. 21)

The passage expresses the claim that certain (style-dependent) sentences acquire a truth-value only when there is a SoT that makes them up for grabs as true or false. Consider this sentence (subsection 3.3.4):
Any prism which has a triangular base is divided into three pyramids equal to one another which have triangular bases (Heath 1908 Proposition VII book XII p. 394).

Hacking would say that this sentence came up for grabs as true or false when the presuppositions (methods of reasoning, criteria of evidence etc.) of the postulational SoT came into play. Its meaning points to something external, although this ‘something’ is not a state of affairs in the real world, rather a state of affairs in the universe of discourse supplied by the lettered diagram. For someone who adopts the postulational SoT to understand Euclid’s statement can well be expressed by proposition 4.024 in the Tractatus: ‘to understand a proposition means to know what is the case if it is true’ (Wittgenstein 1974 [1921], prop. 4.024). The representationalist view held by Wittgenstein in the Tractatus is more evident in Hacking’s insistence that there exist purely observational sentences, which acquire their meaning from a correlation between words and states of affairs in the external world. In particular, for Hacking the truth of style-independent sentences can be assessed by relying on the evidence of senses, which is basic and out of history.

The conception of meaning of a proposition as its use within a language-game is also present in the SoTs project. Indeed, I remind the reader that a distinctive mark of the postulational SoT is the interdependence between lettered diagram and text. Consider the sentence in Euclid’s Elements XIII.4:

Let there be a straight line, AB (Netz 1999, p. 43)
This sentence does not assert a relation between a symbol and an object. It asserts an action, which is the following: take for granted of a certain line, then proceed to localize it in the diagram on the basis of the letters $A$ and $B$. As Netz points out ‘the identity of “the $AB$” as a certain line in the diagram is assumed by Euclid, rather than asserted by him’ (Netz 1999, p. 44). To paraphrase Wittgenstein, the action to be performed, i.e. assuming that there is a certain line in the diagram and localizing it so-and-so, is not ‘forced upon us’ by the written sentence. It is only by virtue of its use that Euclid’s sentence receives its meaning – to think its sense means to think the written sentence and its ‘normal response’.

In the light of Wittgenstein’s observations it is possible to reread Hacking’s point that certain sentences are style-dependent and have no meaning for someone who adopts a different SoT. Consider the example of Euclid’s sentence ‘let there be a straight line, $AB$’: the ‘normal response’ necessary to give meaning to it is a pattern of use that emerged in the ancient Greece, part of the way of doing of the postulational SoT. An imaginary Babylonian who had had to understand the meaning of that sentence would have been in the same position of that child who ‘came into a strange country and did not understand the language’, to echo Wittgenstein. Neither the signs that express the sentences nor the pointing to someone who responds to the sentence in a certain way could have revealed the meaning of Euclid’s sentence to a Babylonian. Like the imaginary child mentioned by Wittgenstein, the Babylonian would have had to possess a ‘background capacity’, be prepared to respond to Euclid’s sentence as the ancient Greeks standardly responded when they assumed the existence of a certain line and looked for it in the diagram. To use my terminology (section 4.4), Euclid’s sentence would
have been *intelligible* for a Babylonian, but it would have not been *understandable* for her. Indeed, a Babylonian would have needed to learn to *respond* to Euclid’s sentence as an ancient Greek did. But to act and rely on the evidence of the postulational SoT would have meant to drop out of her community. After all, returning to Wittgenstein, when a child learns the meaning of the words by *using* them as the members of her community do, she becomes a member of her community.

By the same token, consider the sentence below in the probabilistic SoT (section 2.2)

The adult height for one sex in an ethnic group follows a normal distribution

One can say that to understand, for example, the meaning of ‘distribution’ a person must develop an understanding of the practice of selecting a sample, arranging the values that the variable of height takes in the sample, recognizing the different frequencies within certain intervals. As Wittgenstein says, ‘the meaning of a word is a kind of employment of it. For it is what we learn when a word is incorporated into our language’ (OC 61). The employment of the word ‘distribution’ is that one which is dictated by the scientific community after the emergence of the statistical SoT.

Ultimately, notwithstanding the differences in terminology, Hacking makes a point very similar to Wittgenstein: for both of them the meaning of a sentence is determined by a pattern of usage that has emerged over a long period of time. In the case of Wittgenstein this pattern of usage is rooted on the ‘background capacity’ of a child; in the case of Hacking on the ‘presuppositions’ of a given SoT.
It is important to notice, once more, that Wittgenstein discusses meaning in the context of language-games that inhabit our everyday language: in order to exemplify the case of someone that is ‘out of a language-game’, he gives imaginary examples, e.g. that of a child who has to learn the language. On the other hand, Hacking discusses meaning in historical contexts that emerge at different points in time. In this sense, it is possible to say that Hacking brings history into Wittgenstein’s picture of meaning. In other words, Hacking presents concrete examples of different communities that cannot understand each other because they do not share the same presuppositions.

8.3 Ungroundedness, Self-Authentication and Epistemic Relativism

The point I want to make now is that, given certain crucial similarities between the notion of form of life and the notion of SoT, if the former implies anti-foundationalism the latter implies anti-foundationalism too. For a start, let me point out these similarities.

8.3.1 Forms of Life and Styles of Thinking

The expression of form of life is mentioned for example in PI where Wittgenstein says that ‘to imagine a language means to imagine a form of life’ (PI 19). He does not mean that languages are shaped by cultures, rather that human language, including the use we all associate to words, is associated with certain common
features of human life. Indeed, as Greg Hill suggests (Hill 1997, p. 565), (PI 19) can be compared to (Wittgenstein 1997 [1953], p. 174) in which Wittgenstein says:

One can imagine an animal angry, frightened, unhappy, happy, startled. But hopeful? And why not? [...] Can only those hope who can talk? Only those who have mastered the use of a language. That is to say, the phenomena of hope are modes of this complicated form of life (Wittgenstein 1997 [1953], p. 174).

In this passage, Wittgenstein speaks of a form of life to which the phenomenon of hope is peculiar. Therefore, the expression does not refer to a specific human culture but to all human beings, those beings who speak a language (and therefore can hope).

Another well-known passage helps to understand better what a form of life is:

So you are saying that human agreement decides what it is true and what it is false? — It is what human beings say that it is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life (PI 241).

Wittgenstein’s point is that there is a sort of ‘human agreement’ that allows the working of the language itself; within the framework of this ‘human agreement’ human beings express sentences that can be true or false. And what does ‘human agreement’ consist in? It is an agreement in form of life, an expression by which, as it will apparent soon, Wittgenstein points to a common way of acting; to human
reasoning procedures such as deduction and induction; to our taking for granted of certain propositions such as 'The earth has existed since long before my birth' or 'The earth is round' or 'Every human being has two parents'; and, in general, to what is part of our natural history such as walking, eating, drinking, playing.

The relation hypothesized by Wittgenstein between a form of life and the meaningful sentences is comparable to the relation between a SoT and its style-dependent sentences. Indeed, a form of life is the *sine qua non* of the existence of meaning, of a common 'response' - and, all the more so, of any true-or-false sentence; similarly, a SoT is what makes sentences true-or-false, meaningful, objective. Compare, for example (OC 94)

> [...] [My picture of the world] is the inherited background against which I distinguish between true or false (OC 94).

with

> Propositions of that sort that necessarily require reasoning to be substantiated have a positivity, a being true-or-false, only in consequence of the styles of reasoning in which they occur (Hacking 2002 [1982] -a, p. 175).

Often, Wittgenstein refers to our form of life as a way of *acting* that is part of our nature. For example, in (OC 144):

> The child learns to believe a host of things. I.e. he learns to act according to these beliefs. Bit by bit there forms a system of what it is
believed, and in that system some things stand unshakeably fast [...] (OC 144)

According to this passage, there is a close connection between what we take for granted and our way of acting. In (OC 109) Wittgenstein asks whether an empirical proposition can be tested and in (OC 110) he continues:

What counts as its test? -- But is this an adequate test? And if so, must it not be recognizable as such in logic? -- As if giving grounds did not come to an end sometime. But the end is not an ungrounded presupposition: it is an ungrounded way of acting (OC 110)

Giving justifications for the truth of a proposition comes to an end – and the end is a way of acting not a proposition. To understand the sense of this thought one has to bear in mind that a way of acting is ascribable to a way of judging, as Wittgenstein himself says en passant in (OC 232) ('our manner of judging and therefore of acting' my italics). In other terms, the human system of beliefs, the human procedures of reasoning, what we consider as irrefutable evidence, determine our way of judging and therefore the way of acting in order to prove, argue, experiment, measure etc. Therefore the passage above expresses the idea that there is no further justification for the human way of judging, e.g. our procedures of reasoning and our systems of evidence. Indeed, they determine our way of acting, which the passage defines as the end of our giving grounds. In Hacking the concept of ‘way of doing’ plays a similar role as I am going to show.
8.3.2 Ungroundedness

In the case of style-dependent propositions one can ask the same question posed by Wittgenstein regarding empirical propositions: does giving ground come to an end? The end is not a proposition – the end, according to Hacking, is represented by the presuppositions of the SoT itself, the way of thinking and doing of that very SoT. Indeed, compare:

There is no higher standard to which they [SoTs] directly answer

(Hacking 2002 [1992], p. 192)

with

At the foundation of well-founded belief lies belief that is not founded

(OC 253)

In the light of the second passage, from the former passage follows that there is no further justification outside the presuppositions of the SoT in which a belief is held. So, the presuppositions of the SoTs represent, so to speak, the belief that lies at the foundation of all our beliefs in that SoT – and there is no foundation for the presuppositions.

Notice that when Hacking says that we never call into question the presuppositions of a given SoT he seems to echo Wittgenstein. Compare the following passages:
We do not check to see whether mathematical proof or laboratory investigation or statistical studies are the right way to reason (Hacking 2002 [1992], p. 188).

[...] We can't just investigate everything, and for that reason we are forced to rest content with assumption. If we want the door to turn, the hinges must stay put (OC 343)

I have to stress again a substantial difference between the two pictures of Wittgenstein and Hacking. The former discusses meaning within the perspective of, nota bene, an ahistorical structure, human language, whereas Hacking discusses meaning within the perspective of different structures that emerge and evolve over time. In some sense, Wittgenstein's form of life is prior to the notion of SoT in that all the SoTs themselves presuppose the existence of human language, and in particular style-independent propositions that can be assessed by virtue of a universal and atemporal evidence of senses. When SoTs emerge and evolve they introduce new candidates for truth of falsehood leaving intact the basic structure of human language with its rules and languages games. For Hacking, as I interpret him, these rules include a way of judging that relies on the evidence of senses. SoTs do not change language games such as commanding, questioning, recounting, chatting, which 'are as much a part of our natural history as walking, eating, drinking, playing' (PI 25).

Wittgenstein's investigation reaches, in this sense, deeper levels: it is an investigation on what makes possible meaning at all. No wonder that, while
Hacking simply assumes that empirical propositions are assessed by the evidence of senses, Wittgenstein asks what makes us certain of their truth. In the context of his argument against two papers of the philosopher George Edward Moore (1873-1958), ‘A Defense of Common sense’ and ‘Proof of an External World’ (Moore, 1959), Wittgenstein raises the doubt that common-sense certainties (expressed by sentences such as ‘Every human being has two parents’ or ‘The earth has existed since long before my birth’) cannot be treated as the most certain knowledge.

If Moore says that he knows the earth existed etc., most of us will grant him that it has existed all that time, and also believe him when he says he is convinced of it. But has he also got the right ground for his conviction? For if not, then after all he doesn’t know (OC91)

According to him ‘Moore choses precisely a case in which we all seem to know the same as he and without being able to say how’ (OC 84): good philosophy should show that common-sense certainties are ungrounded propositions of our language-games. They are neither true nor false, neither justified nor unjustified, or neither empirical nor normative.

Common-sense certainties and the presuppositions of SoTs have the same epistemological status. The former lie at the bottom of all human languages games, are constitutive of our form of life, represent ‘the axis around which a body rotates’ (OC152): if we did not believe in their truth we could not assert that we know all we know. The presuppositions of a SoT stand exactly in the same relation with all is known by using that very SoT. In this respect in doing philosophy
Hacking has pursued similar aims as Wittgenstein in that he has identified the ungrounded presuppositions of the different ways of knowing. Compare:

We don't, for example, arrive at any of them [Moore's propositions] as a result of investigations (OC 137)

with

the propositions that are objectively found to be true are determined as true by styles of reasoning for which in principle there can be no external justification (Hacking 2002 [1982] -a, p. 175).

In Wittgenstein's passage it is said that our belief that common-sense certainties are true is not justified by an investigation. Similarly, in Hacking's passage it is said that the presuppositions of a SoT have no external justification.

To sum up, for Wittgenstein whether we consider the human way of acting or our procedures of reasoning or our common-sense certainties, which are all constitutive of our form of life, we have to conclude that they are ungrounded -- in short, our form of life is ungrounded. The parallels I have drawn between Wittgenstein's and Hacking's claims have brought to the fore that SoTs are ungrounded too. In particular, whether we consider our ways of thinking or doing, or our systems of evidence, or our types of explanations, which emerge over history and are part of the presuppositions of our SoTs, they are all ungrounded.
8.3.4 Ungroundedness and Self-authentication

The comparisons I have drawn also shed light on the concept of self-authentication. Indeed, one can express this concept by saying: a SoT becomes a standard of objectivity because it gets at the truth and we believe that it gets at the truth because we rely on its presuppositions. Now we can say that the circulus in probando emerges from the fact that we cannot give any ground for the presuppositions of a SoT: they are neither justified nor unjustified. It is ungroundedness that lies at the root of the 'phenomenon of self-authentication' as the following parallels between Wittgenstein and Hacking show.

Consider for example the algorithmic SoT. In section 3.2 I had argued that the algorithmic SoT, as it is described by Hacking, does not answer to any criteria except its own: what has been found by using an algorithm is assessed by using another algorithm. For example, in the ancient Egypt the 'method of false positions', an algorithmic method used to solve linear algebraic equations, was checked by the means of proportions. Let us compare this chain of reasoning concerning the self-authentication of the algorithmic SoT with the following chain of reasoning that we find in OC. Wittgenstein says:

We should ask: 'What is it like to make such a mistake as that?' – e.g. what's it like to discover that it was a mistake? (OC32)

and continues:
If someone is taught to calculate, is he also taught that he can rely on a
calculation of his teacher’s? But these explanations must after all
sometime come to an end. [...] (OC 34)

The answer is:

If you demand a rule from which it follows that there can’t have been a
miscalculation here, the answer is that we did not learn this through a
rule, but by learning to calculate (OC 44)

And

In certain circumstances, for example, we regard a calculation as
sufficiently checked. What gives us a right to do so? Experience? May
that not have deceived us? Somewhere we must be finished with
justification, and then there remains the proposition that this is how we
calculate (OC 212).

In conclusion:

When does someone say, I know that .... x.... = ... ? When one has
checked the calculation (OC 50)

From the perspective of the SoTs project, this conclusion amounts to stating that
the algorithmic SoT is self-authenticating. Indeed, to check ...x...=... by another
(or the same) method of calculation is for Wittgenstein the only way to assess the truth of ...x...=...  

8.3.5 Ungroundedness and Epistemic Relativism

Notice the use of the verb ‘know’ in the last passage above. Imagine a very ancient society who adopted a SoT different from the algorithmic one. One cannot say that a member of that society was able to ‘know’ that ...x...=... in that the only way to assess the truth of ...x...=... is to use the algorithmic SoT itself, which was alien to her. In the case of Hacking, this circumstance leads to epistemic relativism: ... x...=... is justified within the algorithmic SoT and unjustified outside of it. No wonder that this was my point in the previous chapter in the case of experimental realism: since the laboratory SoT is self-authenticating the claim that unobservable entities exist is assessed by using the laboratory SoT itself.

Now, observe that the example of a hypothetical ancient society I have made is not so far from Wittgenstein’s mind. He considered a hypothetical situation in which someone does not want to rely on any method of calculation:

If someone supposed that all our calculations were uncertain and that we could rely on none of them (justifying himself by saying that mistakes are always possible) perhaps we would say he was crazy. But can we say he is in error? Does he not just react differently? We rely on calculations, he doesn’t; we are sure, he isn’t (OC 217).

Wittgenstein is pointing to a situation in which a calculation would be justified for us and unjustified for someone else. However, we cannot conclude that
Wittgenstein's passage implies epistemic relativism since he is illustrating a *hypothetical* case for the argument's sake. Conversely, Hacking does *not* give hypothetical examples – as a matter of fact the SoTs project posits the existence of societies that have not thought according to certain SoTs. Consequently, whereas for Wittgenstein ungroundedness just implies the possibility of *hypothetical* cases of epistemic relativism for Hacking it does imply *actual* cases of epistemic relativism.

Let me illustrate this point in more general terms. Wittgenstein wrote that 'if a lion could talk we could not understand him' (Wittgenstein 1997 [1953], p. 223) meaning that human beings could not understand a talking lion because there would be no 'agreement in judgments' between the lion and us. Only through the connection with the form of the life of the individuals of its species could we learn to understand the lion. But since talking lions do not share our form of life we cannot project 'the lion's sentences'. In other terms, the lion's claims of knowledge would be justified in its form of life and unjustified in ours. Now, just as for Wittgenstein agreement in judgments among human beings is constitutive of our form of life, so for Hacking agreement in a standard of evidence, way of thinking and doing is constitutive of a given SoT. So for similar reasons to those expressed by Wittgenstein by his example of the lion, a claim of knowledge made in a certain SoT could not be projected. Importantly, whereas the case of a talking lion is a mere fictitious example, the emergence of different SoTs is for Hacking a *real* historical event. Therefore, while for Wittgenstein the consequences of the groundlessness of our form of life concern hypothetical cases,
for Hacking they imply that as a matter of fact a certain claim can be justified within a given SoT and unjustified outside it.

In order to support my thesis in this section, i.e. that the SoTs project implies epistemic relativism, I wish to quote and comment a passage of Annalisa Coliva, which I consider representative of a line of argumentation that aims to show that Wittgenstein was not an epistemic relativist:

[...] Wittgenstein was merely an anti-foundationalist: he believed that our world-picture is ungrounded and that it is not a mere reflection of a totally mind-independent reality. But anti-foundationalism is a long way short from relativism, let it be factual – the view according to which there actually are different incompatible epistemic systems that are all equally valid – or merely virtual – that is, equally valid, and incompatible epistemic systems, all in fact conceivable from our own standpoint. For simply to say that our world-picture is ungrounded does not entail either that there are actually different ones, or – more contentiously – that there could intelligibly be other ones, at least in principle (Coliva 2010, p. 3).

Whether or not Coliva is right in the case of Wittgenstein, her argument cannot be applied to Hacking’s case. In Hacking, both the elements, anti-foundationalism and the claim that there are different epistemic systems, are present. Indeed, Hacking himself once wrote: ‘it never occurred to me that all knowledge needed foundations’ (Hacking 2007b, p. 35). Moreover, the SoTs project posits ‘manifold styles of scientific reasoning about which he [Wittgenstein] was silent’ (Hacking
2002, p. 226). Therefore, nothing prevents us from stating that the SoTs project implies epistemic relativism.

8.4 Conclusions

In this chapter, on the basis of some parallels between the notion of ‘form of life’ and the notion of SoT, I have argued that the SoTs project implies anti-foundationalism. However, although it is at issue in the case of Wittgenstein whether anti-foundationalism leads to epistemic relativism, this inference is *inevitable* in the case of Hacking.
Chapter Nine: The Contingency Issue:

Some Major Implications of the Theory of Styles of Thinking

9.1 Introduction

In section 1.4 I have explained that by ‘contingency issue’ it is meant the question as to whether the history of a particular branch of our science could have taken a different route and provided results incompatible with those of our actual science. I have also pointed out that, apart from Hacking’s recent comments (Hacking 2014, chapter four), the discussions on the contingency issue have not involved the notion of SoT. I want to fill this gap in the literature by drawing some important implications for the issue of contingency from the theory of the SoTs that I have developed in chapters three and four. As far as the contingency issue is concerned, I shall address four fundamental questions. First of all, in the next section I shall discuss to which extent the emergence of SoTs at a certain point of history is a contingent circumstance. In the following section I shall ask whether the endurance of SoTs was inevitable by tackling the connected question as to why the SoTs are long lasting. Afterwards, I shall focus on the growth of knowledge by dealing with questions such as: if a certain SoT continues to be employed and if Q
is a 'live question', is science bound to converge on a single answer to Q? Finally, on the basis of my previous reflections I shall look into the question of the convergence of science in the long run. The answers to these questions will rely on some basic assumptions of the theory of SoTs and imply a picture of the evolution of sciences in which both contingency and inevitability play a key-role.

9.2 Why did styles of thinking emerge?

The question I shall examine in this section concerns the role played by contingency and inevitability in the genesis of SoTs. I think that the theory of SoTs can provide important insights into this question, although a comprehensive answer would require for each SoT a study of cognitive history. I shall start by arguing that the emergence of the algorithmic SoT was inevitable.

9.2.1 Inevitability and Algorithmic Style of Thinking

In section 3.21 I have maintained that the algorithmic SoT emerged in ancient civilizations such as the Egyptian or the Babylonian: in order to solve problems such as Ahmes', the Egyptian mathematicians used a standard set of rules involving numbers that were put down on papyri and handed down from a scholar to another. In subsection 3.2.5, I have also explained that the algorithmic SoT is grounded in typical human capacities: for millennia human beings have unconsciously followed step-by-step procedures for fulfilling basic needs as animals do, e.g. spiders make silk for catching insects by following three basic
steps (Devlin 2005, chapter 5); furthermore, basic numerical competences are rooted in biological built-in resources. Importantly, I have also explained that when the first human societies started to make use of commodity money a concept of number had to come about. In this sense the emergence of the concept of number has been inevitable.

Although key elements such as the capacity of following step-by-step procedures of reasoning and the very concept of number have been a sine qua non for the emergence of the algorithmic SoT, other factors have played a major role in its genesis. The introduction of zero, in addition to the positional system, allowed the representation of any number without ambiguities. The concept of zero was represented by the Babylonians by an empty space, it developed as a number in India, it was adopted by the Arabs and finally it wended its way across Europe when the mathematician Leonardo Fibonacci (1170-1250) promoted the Hindu-Arabic numeral system, which had appeared in Europe in the eleventh century through the Moors (see Seife 2000). Interestingly, in around 400-300 BC also the Mayan people invented independently the concept of zero and a relative symbol. If one considers that this people did not come into contact with the earlier European civilizations, the emergence of zero appears an inevitable fact. However, it should be considered that in the ancient China zero was not used as a number but as an empty place indicator. Without the number zero there could not have been negative numbers and algebra would not have been possible. In other words, at least as compared with the inevitable invention of positive integers, that of negative numbers appears contingent.

The positional system made much simpler to perform calculations, as it can
be noted by thinking of the difficulty of performing an addition by using Roman numbers. The Babylonians developed the sexagesimal system independently in about 2000 BC.; in the Classical period (250 to 900 AD), the Mayan people introduced a base-20 positional system. Other number bases have been developed by different cultures but eventually the decimal system prevailed in large parts of mathematics. Since all the Indo-European languages share the base-10 system it is likely that the peoples who spoke proto-Indo-European used it. A plausible hypothesis is that the decimal system derived from the use of the hands for counting and that it spread around the world for its convenience as a basis for calculation (Barrow 1992, p. 36). If this hypothesis is correct, given the basic features of humans, it seems inevitable that the base-10 system came to the fore and spread around the world. For sure, the use of positional systems in different ancient cultures, which did not come into contact, suggests that the emergence of a numeral system was an inevitable fact.

This brief account encourages the following reflections. The fact that the concept of number as well as the use of step-by-step procedures in mathematics emerged in different ancient societies suggests that, given any sufficiently developed civilization, it was inevitable that humans had to develop the algorithm SoT. However, it is a merely historical fact that the fields of mathematics to which we apply the algorithmic SoT are exactly what they are today: one might argue that even the coming to the fore of algebra is a fact contingent on the particular use of zero as a number in certain human civilizations. Some readers may feel that the extension of positive integers to negative ones was inevitable once the concept of zero came to the fore. I just want to point out that there is a point in time in
which it is possible, at least in principle, to imagine an algorithmic SoT that deals with a very different mathematics, in this case an impoverished mathematics rather different from ours. One might consider that as the point in time in which contingency makes its way into the history of science.

9.2.2 Contingency and Postulational Style of Thinking

I shall now move on to the emergence of the postulational SoT. According to some cognitive scientists, just as there is an ability to grasp the numerosity of sets of objects inherent in the human mind, so there is an innate sense of the basic conceptual principles of geometry. For example, a team of researchers has provided evidence for geometrical intuitions (e.g. symmetry, parallelism, congruence) in an Amazonian group who had no schooling and no experience with artefacts that employ geometrical concepts (Dehaene and al. 2006). In the mind of Netz and Hacking, to acknowledge the existence of such human resources is the starting point of the particular cognitive history of Greek geometry (Netz 1999, p. 6) (Hacking 2011) and, therefore, of the postulational SoT.

Netz points out three crucial factors for the emergence of proof. The first one concerns the fact that the Greek society was ready to question almost everything: following Lloyd, Netz claims that the Greek social environment was fertile ground for forms of persuasion (Lloyd 2000). The second factor concerns orality, i.e. the fact that in Athens ‘the characteristic mode of political debate was oral’ (Netz 1999, p. 292-293). Especially in chapter 5 section 3, Netz shows how the structure of proofs, the use of formulae, the reference to the diagram, i.e. to an immediately present visual object, reflect the particular form of orality and the importance of
persuasion in that *specific context*. In some sense, the demonstration was an oral argument written down next to a lettered diagram, argues Netz. The third factor concerns the existence a literate environment: the invention of a lettered diagram would not have been possible in a pre-literate society or a society which used pictograms (Netz 1999, p. 58).

Netz's theses suggest that the emergence of the postulational SoT was a fact contingent, especially if we compare it to the emergence of the algorithmic SoT. Indeed, a lettered diagram is an abstraction conceptually similar to that of number; nevertheless, whereas the concept of number comes about independently in different societies, that of lettered diagram is a specific Greek invention. Its coming to the fore would have been impossible without the presence of a literate and argumentative society, according to Netz. In brief the emergence of the postulational SoT has been contingent on social and historical factors. Another point concerns the way in which the innate intuitive comprehension of geometry was tapped. The postulational SoT represents one of the many possible ways, surely not the only possible one, of tapping human cognitive resources and logical deduction: other ancient societies did use diagrams, but in radical different modalities. Nor is the postulational SoT the only way for proving important geometrical truths: for example, Pythagoras theorem, which can be proven by using the postulational SoT (i.e. relying on the similitudes of triangles), can also be proven by using algebraic methods or even by rearrangement (i.e. by making three copies of the original triangle and arranging them in particular ways).

Ultimately, the specificity of the postulational SoT is contingent on the features of the Greek society, e.g. factors such as literacy, orality and persuasion. However
this specificity does not make the discovery of certain mathematical truths contingent since there might be other methods, different from the geometrical SoT, for arriving at them.

9.2.3 Contingency, Laboratory Style and the Evolutionary Tree of Styles of Thinking

Let us turn to the Laboratory SoT. Referring to the experimental reasoning in the early times of civilization Hacking wrote that ‘[i]t probably ha[d] no strict beginning. Human beings have been curious, looking, tinkering, exploring, even measuring, forever’ (Hacking 2009, p. 42). And, when he argued that the laboratory SoT is a particular way of combining experimental reasoning and modelling, he added that in the earlier times ‘the modelling was typically of the movements in the heavens’ (Hacking 2007a, p. 3). It seems plausible that humans have always reasoned by constructing models. One could well make the hypothesis that modelling is rooted in ancestral human ritual practices of representing in order to make something happen in the future, e.g. the ancient Egyptians believed that by writing the name of their hostile people in pottery vessels and smashing them they could destroy their enemies (Van De Mieroop 2011, p. 109). However, Hacking’s thesis that experimental activity had no strict beginning is not plausible. In order to measure something it is necessary to take two steps, which are not reducible to counting: firstly, to adopt a unity of measurement; secondly, to possess a concept of number. Indeed, the value of a measurement typically represents how many times a predetermined length is contained in the length to be measured. In many cases, integer numbers are not
sufficient because not all the lengths in geometry can be divided into an integer number of pieces of the same length. Therefore, the emergence of the concept of number and the algorithmic SoT have been necessary for the coming to the fore of the earlier forms of experimental reasoning. This is to say that, as contingent as the emergence the laboratory SoT may be, there is no contingency in the order in which the two SoTs, the algorithmic and the laboratory one, came about. If one 'replied the tape' of our cognitive history, the event of the crystallization of the laboratory SoT would be contingent on the emergence of the concept of number and, certainly, on the discovery of the irrational numbers, which was prompted, as far as we know, by the emergence of the postulational SoT. In this sense the analogy with the Darwinian evolution is striking: as, say, the emergence of mammals is contingent on the emergence of primitive chordates, so the laboratory SoT is contingent on the emergence of the algorithmic SoT. Algorithmic, postulational and laboratory SoT are in relation of extreme connectedness as different species in the same genus. However, although their order could not be reversed, each of them could have been different in content, e.g. it could not have achieved all the actual results.

To summarize it is useful to imagine a sort of evolutionary tree of the SoTs: the algorithmic SoT is in the tree 'trunk', as if it were an ancestor of all the other SoTs. Several branches grow from the trunk but are separated from it by a knot to indicate a sharp discontinuity: the first branch that we encounter up along the trunk is the postulational SoT, and in turn the laboratory SoT may be viewed as a branch of the postulational SoT, separated from it by a knot. Then, as I shall explain, the other SoTs form different branches of the trunk. The birth of the tree
has a strong character of inevitability, since the human cognitive resources and the universal human forms of socialization predetermined the emergence of the algorithmic SoT. At the next stages both the postulational SoT and the laboratory SoT are the result of particular ‘environmental pressures’, e.g. the unique features of the Greek society. Note that trunk and branches, although separated by knots, are somehow connected in that they are part of one and only organism. No postulational SoT could have been possible without the vast mess of problems about lengths, areas and volumes produced by reasoning in the algorithmic SoT by the ancient civilizations anterior or coeval with the ancient Greeks; no laboratory SoT could have been possible without the possibility of using real numbers to measure and a prior model of demonstration (Hacking 2014, p. 140). There is no contingency in the order in which these SoTs have emerged because there is no postulational SoT without some of the achievements of the algorithmic SoT and there is no laboratory SoT without some of the achievements of the postulational and the algorithmic SoT.

9.2.4 Contingency and Other Styles of Thinking

An idea of considerable significance for Paracelsus’s way of thinking was the Pythagorean conviction that everything in the universe is intrinsically mathematical. Those who adhered to Pythagoras’ school believed that numbers, to which they attributed mystical or divine significance, was associated to all sort of things that exist. This body of mystic tradition survived in the way of reasoning of Renaissance thinkers and of many naturalist philosophers still at Galileo’s time. Rather than thinking of the universe as a system governed by laws to be
discovered by performing experiments, these scholars directed their attention to single facts of observation that they interpreted as *signs*. Eventually, this conception of the universe was substituted by one in which single events have a cause and an effect, more than a meaning. However, it was not until this way of thinking underwent a mutation, whose consequence was the emergence of the statistical SoT, that it became extinct. As I have explained in section 2.2, according to Hacking this mutation consisted in a transformation of the concept of sign into a new concept of 'internal evidence', i.e. of evidence other than testimony. The statistical style was a new way of tapping this innate sense of evidence, a kind of evidence that was absent in the high sciences and that only in the seventeenth century become a standard of truth. In the evolutionary tree the style of the Renaissance way of thinking could be represented as a dead branch that, at a certain point, has a knot, which marks a point of discontinuity with the new branch of the statistical SoT.

What is made clear by this brief survey is that it is contingent when, how and if certain human innate resources come to be exploited. Among these innate human resources I include not only the ability to grasp the numerosity of a grouping of objects or to imagine two parallel lines. I also include logic abilities such as deduction, which is the central method of the postulational SoT, or the very innate tendency to use certain facts as 'evidence of something else', the evidence of things, which is the standard of evidence of the statistical SoT. According to the theory of SoTs I have developed, all these human innate resources are inborn features of human beings that at certain points of history are *co-opted* by a given SoT. The historico-genetic SoT is another case in point.
Abductive reasoning has always been used by human beings: it was in Aristotle’s Prior Analytics that C. S. Peirce (1839, 1914) found the notion of abduction (Psillos 2011, p. 119). When the seventeenth century intellectuals directed their attention to fossils, minerals and geological layers abductive reasoning was co-opted to find out the best explanation of those previously unnoticed objects and its relevance for the study of nature became considerable.

The same considerations can be repeated in the case of the taxonomic SoT. When Ray, Linnaeus and other naturalists set out to establish the relations between the different kinds of living beings they found a way of tapping the human ability to classify – in fact, they put the folk classifications of plants and animals into a scientific footing. The co-option of these human innate resources, i.e. the abductive reasoning in the case of the historico-genetic SoT and the common-sense view of natural relationships in the case of the taxonomic SoT, has been a fact contingent on the emergence of a vast amount of evidence, an entire portion of the world previously unnoticed.

The laboratory SoT played a role for the coming to the fore of the historico-genetic and the taxonomic SoT, so that their emergence can be considered contingent on that of experimental science. Indeed, as Peter Bowler maintains, what paved the way for the Darwinian revolution was the failure of the attempt to harmonize the new science inspired by Galileo and Newton with the contemporary understanding of the Bible (Bowler 2003 p. 27). On the other hand it seems inevitable that the taxonomic and the historico-genetic SoT had to rely on one another: e.g. without an unambiguous way of expressing the relationships between living beings, the historico-genetic SoT would have been useless.
In the evolutionary tree of SoTs both the taxonomic and the historico-genetic SoT represent two different branches. Their evolution can be explained in terms of the concept of exaptation in biology. The latter refers to a character of an organism that comes to serve a new function different from the previous one: e.g. although the feathers might have evolved for regulating the body temperature at a certain point in time they were co-opted by natural selection in order to allow the flight.

Similarly, when the historico-genetic SoT and the taxonomic SoT evolved, the abductive reasoning and the natural disposition for classifying took on new functions, perhaps not so distinct from the previous ones, but surely different in purpose.

To conclude, I have argued that the emergence of the algorithmic SoT has been inevitable. However, the emergence of all the other SoTs has been the result of both contingent and inevitable factors: on the one hand, only certain historical, social, cultural and intellectual contexts have made it possible their emergence; on the other hand, there is a rationale in the order of their emergence, i.e. there is no contingence in the order in which they have appeared. This suggests that, once what it is found out by using a certain SoT is in place, there are the epistemological conditions for the emergence of other SoTs; but which SoTs come about, whether they come about, and when they come about is contingent. There might have been different ways of tapping human resources and, above all, we could not have achieved the same results we have achieved today.
9.3 Why do SoTs endure?

In this section I shall argue that each SoT is suitable for dealing with certain objective structures of our world just as certain methods of lock picking are suitable for unlocking a given type of lock. Since Hacking is antirealist about theories, he has never put the accent on the ability of SoTs to represent the world. Consequently, the SoTs project makes no mention of what I consider an important fact: as I shall show, each SoT is grounded in objective features of that portion of the world it studies. I suggest that this fact should be taken into consideration when addressing the question as to why a SoT endures. Indeed, although the internal techniques of self-authentication are surely part of the explanation of the resilience of a given SoT (section 4.2.2), they cannot account for its success in representing the world more or less well than another possible way of thinking. Since success must be part of the explanation of why we do not abandon a given SoT, I claim that the ability of this very SoT to account for certain objective features of the world is relevant to the question of why SoTs are long lasting.

Ultimately, in addition to the self-authenticating character of SoTs, it is possible to find in the world other reasons that justify the endurance of SoTs. I cannot investigate here how the interplay between these two very different causes (which concern the internal features of SoTs and certain objective features of the world) can make a specific SoT long lasting. I shall only present four examples that illustrate how each of the six SoTs mirrors objective features of the portion of the world it deals with. They suggest that there is a deep connection between SoTs and the features of those portions of the world they study.
9.3.1 Laboratory SoT, Historico-genetic SoT and the Time's Arrow

Many events are causally connected in time in a 'one-way direction'. In the example of Carol Cleland, we may happen to see the shattering of a window by a baseball but we never see the ball going back along the same trajectory and the scattered pieces of glass suddenly jumping up and converging to the empty window frame (Cleland 2002, p. 487-494). Ever since Arthur Eddington's publication of *The Nature of the Physical World* (Eddington 1928) this basic feature of our world is known as the 'time's arrow'. A consequence of the time's arrow is the 'asymmetry of over-determination': to conclude that by throwing a ball the window glass gets broken one needs hypotheses on the ball, the resistance of the glass, the kind of throwing etc.; to conclude that the window glass got broken might be sufficient to find one of the many pieces of glass on the ground. Whereas causes are over-determined by their effects, the causal predecessors of an event under-determine it: there are much more traces than those needed for inferring that the window broke. Cleland notes that the undetermination of effects by their putative causes is both epistemic and causal: the shattering is epistemically and causally undetermined by the throwing of a baseball. On the other hand, the shards of glass epistemically over-determine the cause, but are not part of it. Cleland's point is that the patterns of reasoning of the experimental and the historical sciences are grounded in this pervasive time asymmetry of nature (Cleland 2002, p. 474). Her point can be developed: the time's arrow explains why the laboratory SoT and the historico-genetic SoT have just the features they have (e.g. their kinds of evidence, standard of truth and methods of reasoning) and why
they succeed in, respectively, predicting phenomena and inferring causes. Indeed, in order to infer that an asteroid hit the earth about 65 millions of years ago scientists must reason in the historico-genetic SoT. That is: they cannot perform an experiment in laboratory but they have to look for a 'smoking gun' (e.g. granules of shocked quartz), proliferate tentative explanations for their presence, use abductive reasoning and pick out the best explanation. The crux of the matter is that, because of the over-determination of past events, scientists do not need to find all the traces of the effects of the impact (and cannot look for all of them) in order to succeed in finding the cause. It is a basic feature of the world that explains the success and the necessity of the historico-genetic SoT. Now, as the over-determination of past events by few present events explains the use of the historico-genetic SoT, so the under-determination of future events by 'initial conditions' explains the use of the laboratory SoT. What the physicists need is to test a hypothesized law or an entire theory, rather than choosing between different explanations. They can manipulate the initial conditions that may trigger the phenomenon, make models under different hypotheses, test them, and reflect on the additional factors that may be relevant. In brief, they must reason in the laboratory SoT – and they must do so because of the under-determination of future events from present initial conditions. Again, it is a basic feature of the world that explains the success and the necessity of the laboratory SoT.

To conclude, the time's arrow is an objective feature of the world that explains why the laboratory and the historico-genetic SoTs are appropriate for finding out. It is reasonable to say that these two SoTs are not enduring for contingent reasons but because they are suited for predicting the future from
present events and inferring the causes from past events.

9.3.2 Taxonomic SoT and the Logic of Evolution

Stephen Jay Gould made a point (Gould 2000a) that can be pushed further in order to claim that the endurance of the taxonomic SoT is ascribable to its capacity to mirror the treelike form of the interrelations between the living species, i.e. the causality of the evolutionary world. Linnaeus' system uses a binomial nomenclature: his taxonomic scheme establishes a nested hierarchy in which every living group is contained in a larger group, e.g. species within genera, genera within families and so forth. One could depict this taxonomy by drawing a sort of dichotomous schema in which each element bifurcates; the entire schema would give the impression of a progressively inclusive hierarchy. Gould notes that, although Linnaeus believed in the fixity of species, his nested hierarchy implies Darwin's branching tree, in which a main trunk ramifies into finer divisions. In other terms, although Linnaeus was a firm creationist, his scheme happened to capture an objective feature of the living world: the fact that the interrelationships among organisms follow a genealogical hierarchy built by evolutionary branching. Gould concluded that

Linnaeus has endured because he combined the best observational skills of his time with a theoretical conception of organic relationships that happens to mirror—but not by pure accident—the topology of evolutionary systems, even though Linnaeus himself interpreted his organizing principle in creationist terms (Gould 2000a, p. 4).
Gould also reminds us that the form of logic of Linnaeus' system was not 'invented' by him but was in use in other fields of knowledge since the dawn of Western story. For example, Aristotle, adopted dichotomous maps for classifying the categories of reasoning (Gould 2000a, p. 7).

To sum up, a particular logic of classification, among the many others (e.g. those proposed by Linnaeus' contemporary scholars), happens to mirror a basic feature of the living world. We can draw two conclusions. Firstly, it was a contingent fact that Linnaeus proposed his system although, arguably, the old dichotomous logic of classification would have made its way into the study of living species. Secondly, provided that we continue to have interest in the living world, it is inevitable the taxonomic SoT will continue to be used since it succeeds in mirroring the Darwinian causality, a basic feature of the world.

9.3.3 Algorithmic SoT, Geometrical SoT and the phyllotaxis

My third example concerns a very well known problem posed by the mathematician Leonardo Fibonacci (1170-1250) in his book *Liber Abaci*:

Suppose a newly-born pair of rabbits, one male, one female, are put in a field. Rabbits are able to mate at the age of one month so that at the end of its second month a female can produce another pair of rabbits. Suppose that our rabbits never die and that the female always produces one new pair (one male, one female) every month from the second month on. How many pairs will there be in one year? (in Boyer 1968, p. 281)
His solution is an algorithm for calculating the number of rabbits after each month: starting with one couple, after \( n \) \((n>2)\) months there will be a number of couples equals to the sum of the numbers of couples after \( n-2 \) months plus the number of couples after \( n-1 \) months:

\[1, 1, 2, 3, 5, 8, 13, 21, 34, \ldots.\]

These are called Fibonacci's numbers.

On many plants, the number of petals is a Fibonacci number: for example, lilies and iris have 3 petals, larkspur and aquilegia 5; delphiniums 8; cineraria 13; some asters and chicory 21. Then there are daisies with 34, 55 or even 89 petals and the list could go on. Furthermore, Fibonacci numbers are common in fruit’s seeds, fruits’ sprouts, flowerings, arrangements of leaves.

I shall focus on the *phyllotaxis* of plants, i.e. the arrangements of leaves along the stem. In a plant the leaves are arranged around their stems so that each leaf does not shadow the others. Suppose to go around the stem from leaf to leaf until reaching the leaf directly above the starting leaf (on the same vertical line); and then to go around for another number of turns in order to reach another leaf directly above the starting one and so on. What it is found is that both the number of turns necessary to reach the leaf directly above the starting leaf and the numbers of leaves met are Fibonacci numbers. Take the sunflower. Starting from a leaf, going leaf to leaf and turning clockwise we need one turn to reach the leaf directly above the starting leaf, meeting 3 leaves. Then 2 turns, 5 leaves; three turns 8 leaves; 5 turns, 8 leaves; 8 turns, 13 leaves. That is: exactly the Fibonacci series.
It is possible to say that by using the algorithmic SoT we can know the number of leaves around the stem of a plant at certain fixed positions: the step-by-step procedure to follow is Fibonacci’s recurrence relation. Whether the mechanism of phyllotaxis involves genetic factors (Pennybacker and Newell 2013) or natural constraints (Douady and Couder 1992) it is also possible to say that, as it were, some plants follow the Fibonacci recurrence relation. To conclude, by using the algorithmic SoT we mirror a basic feature of the living world as if we adopted the same procedures adopted by plants, so to speak.

Let us take a further step. In mathematics two lengths A and B with A>B are in the golden ratio if their ratio A/B is equal to the ratio of their sum A+B to A; the value of the golden ratio is 1.61... Now, take a circle into golden proportions, i.e. a circle divided into two arc lengths whose ratio is the golden ratio. The value of the angle subtended by the minimum arc length is 137.5 degrees. It is found that many types of plants have adjacent leaves positioned around the stem at 137.5 degrees. Therefore, in order to find out the relative position of two successive leaves in many plants it is also possible to think in the postulational SoT: project the points in which they grow into a plan and imagine them as two point in a circle, they form an arc that subtends an angle of 137.5 degrees. Put in another way, by reasoning in terms of angles and circles in a diagram we happen to mirror a basic feature of the world: the way leaves are arranged by a plant. Incidentally, the ratio between two successive Fibonacci numbers tends to the golden ratio. This is not surprising since the number of leaves at each turn must grow under the constraint of being located at relative angles dictated by the golden ratio. This relationship between Fibonacci numbers
and the golden ratio reminds us that there is consistency between what it is found independently by reasoning in the algorithmic SoT and in the postulational SoT.

9.3.4 Statistical SoT, Induction and Probabilistic World

One of the results of Hacking's historical analysis in *The emergence of probability* is that the coming to the fore of the concept of probability opened the way to the *analytical problem of induction* and to the search for *stochastic laws of chance processes*. My first point concerns the analytical problem of induction. Inductive reasoning is grounded in an objective feature of the world, i.e. the unlimitedness of the space-time: we cannot search the whole world in order to reach certain conclusions; we have to infer them from *specific* facts. When the concept of internal evidence emerged, it was noticed that there are different degrees to which inferences are supported and the concept of probability allowed to measure their reliability. In this sense, the statistical SoT is grounded in the existence of inductive reasoning, which in turn is grounded in the unlimitedness of space-time.

My second point concerns the existence in nature of stochastic processes. Ever since their discovery, many scientists have believed in the possibility of finding the causal mechanisms that could generate statistical laws. They conceived probability as an epistemic fact, i.e. a fact that simply reflects our ignorance of deeper mechanisms. However, the advent of quantum mechanics convinced the majority of scientists that statistical regularity is a brute and irreducible fact of nature. Indeed, the works of the physicists John Von Neumann, Simon Kochen, Ernst Specker and John Bell have showed that it is impossible to provide a
'classical' reformulation of quantum mechanics formalism. That meant the failure of the programme of hidden variable theorists who contended that the statistical laws are the manifestation of some unspecified deterministic laws.

If the world is only seemingly deterministic but at bottom intrinsically probabilistic, the use of the statistical SoT is necessary. In other words, if stochastic processes are irreducible to deterministic processes the use of the statistical SoT for representing this feature of our world is inevitable. That is, the statistical SoT is grounded in the probabilistic nature of reality. As contingent as the emergence of the statistical SoT may appear, if stochastic processes are an objective feature of the world and we continue to be interested in them, the statistical SoT will endure.

9.4 Questions, Answers, Inevitability

In this section I shall address this question: supposed that a question is asked, is science bound to converge on a single answer? Hacking says:

I am not a contingentist about the content of science, once the questions are intelligible and are asked. But I am inclined to contingentism about the questions themselves (Hacking 1999b, p. 165).

Hacking maintains that, once certain questions are asked and the correct methods are used, the answers are free of the contingency of human history and represent a fingerpost for successive possible sciences (e.g. see Hacking 1999b). His contingentism about questions is well justified in the light of the fact that
contingency plays a crucial role in the emergency of SoTs, which in turn bring into being new questions. However, as I have argued in subsection 7.3.3, Hacking’s inevitabilism about the answers appears to me correct but quite unjustified within his philosophy. When he suggests that there is only a correct answer to live question the adjective ‘correct’ might be, as in the case of entity realism, relative to the SoT in which the answer is given. By the same token, his formulation of the contingency issue given in chapter one (‘If the results R of a scientific investigation are correct, would any investigation of roughly the same subject matter, if successful, at least implicitly contain or imply the same results?’) is affected by the same issue: given the anti-foundationalism of the SoTs theory how could we establish that a result R is correct? What makes the things worse is the fact that Hacking believes in the possibility of an alternative physics successful and progressive like ours but incommensurable with ours.

Such imaginary stable sciences would not even be comparable, because they would be true to different and quite literally incommensurable classes of phenomena and instrumentation (Hacking 1992b, p. 31).

As Léna Soler has remarked, this is ‘a new and fundamental type of incommensurability ignored by traditional philosophers of science’ (Soler 2011, p. 9), which is different from that I have been dealing with in chapter seven. She calls it ‘machinic incommensurability’ because it concerns a scientific practice whose instruments, laboratory procedures, measurement processes share no common measure with the actual scientific practice. Since these technical differences translate into different standards of truths, it is difficult to establish by which
standards an answer to a live question is correct.

I suggest that, although within the framework of SoTs theory there are answers about which we do not have a universal justification, but only a style-dependent justification, there are several important cases in which we have good reasons to conclude that an answer is correct. A case in point is when a scientific result can be achieved by reasoning independently in different SoTs. I have already pointed out some of these cases. For example, I have already remarked that the Pythagorean theorem has numerous proofs, e.g. it can be proven by adopting the postulational SoT, i.e. in terms of the similarity of triangles, or by rearrangement or, still, by using the differential calculus. Also, in the previous section I have shown how both the postulational SoT and the algorithmic SoT reach the same conclusions about the phyllotaxis of plants. Furthermore, in chapter four I have explained that, although Aristarchus calculated the distances of the Sun and the Moon from the Earth by using the postulational SoT, the same result can be obtained by reasoning in the laboratory SoT. Note also that, as a matter of fact, instead of using demonstrations in order to achieve a geometrical result, one could prove a great number of particular cases by using computer programs, i.e. the algorithmic SoT, and concluding by induction that the demonstration is correct.

This convergence of scientific results independently obtained by adopting different SoTs also concerns scientific theories. In Flussi e riflussi (Russo 2003), Lucio Russo argued that although the theory of tides had already been developed in the ancient Greece later on it sank into oblivion and different parts of it were taken over by diverse intellectuals. After about 2000 years, Newton reunited these complementary parts by relying on distinct sources and reworked the theory. On
the one hand Newton’s theory of tides was not conceptually different from that developed in Antiquity, on the other hand the theory literally rose from its ashes in the shape of one of the remarkable achievements of the laboratory SoT.

It is important to note that there are other cases in science in which there are no strong reasons for believing in the correctness of the ontology of a given theory. For example, as James Cushing noted, we could today have arrived at a very different worldview of microphenomena if, for contingent reasons, a causal quantum-theory program had been pursued (Cushing 1992) (Cushing 1994).

To conclude, although there are specific cases in which we are at a loss to know whether or not a scientific answer is correct, in other cases the fact that different SoTs achieve the same results suggests that those very SoTs and their methods are reliable. Then, if the SoTs are reliable, at least in certain cases it is plausible to believe that soon or later, by a series of fallible steps, science will obtain the correct answer to a ‘live’ question. In the light of these considerations, there are good reasons for being inevitabilist about the convergence of science on correct answers to single questions; but it is contingent that those questions have been asked.

9.5 The Long-Term Evolution of Science

I now want to examine the implications of the theory of SoTs concerning the long-term evolution of the sciences. I shall start by noting that, according to the SoTs project, over time there has been a growth of many epistemological items:
Phenomena, [...] manipulative and technological skills [...] styles of thinking accumulate. (Hacking 1983b, p. 56):

Before looking into the consequences of this fact, I want to clear the ground from the common misconception that if science grows, in any possible sense, it means that it converges on truth:

The phenomenon of growth is at most a monotonic increase of knowledge, not convergence. [...] ‘Convergence’ implies somewhat that there is one thing being converged on, but ‘increase’ has not such implication (Hacking 1983b, pp. 55-56).

In this connection, it is useful to mention a point made by Howard Sankey: from no realist commitment does it follow that we will end up with a true complete theory of the world (Sankey 2008, p. 261). For instance, the realist claim that scientific progress consists in advancing towards the truth does not imply that science converges on a single true theory: scientific inquiry might converge on different truths that do not add up to one whole truth. Similarly, to say that science leads to knowledge does not amount to saying that scientific inquiry will lead to the whole truth about the world. Sankey's point suggests that for a realist it is not inevitable that science will reach a final, coherent, single description of the world. But, of course, from the realist claim that science leads to knowledge it does follow that it is inevitable that science will converge on at least some truths, e.g. a (approximately) true description of a portion of the world.
Let us now keep realism aside. I want to investigate why one cannot expect that there is not one thing being converged on. Hacking says that there can be heapings up of knowledge without there being any unity of science to which they all add up (Hacking 1983b, p. 55).

By spelling out this sentence it is possible to provide a picture of the evolution of scientific inquiry in the long run. The metaphor of 'heapings up' of knowledge suggests that some of the knowledge acquired by using a given SoT is in some sense distinct from that acquired by using another SoT; the idea of no unity suggests that these supposed different parts of the our knowledge are not interconnected one with another. I shall clarify first what exactly accumulates and how; then I shall examine the question of the interconnectedness.

I recall that the discontinuous emergence of different SoTs implies that at different times of the history unrelated sets of questions have been considered important. For example, while after the emergence of the statistical SoT problems in social mathematics such as the distribution of age at marriage have been a matter of study, they were not 'live' in the ancient Greece.

The metaphor of 'heapings up of knowledge' should not invite the thought that knowledge is simply something that is 'out there' and needs to be picked up. What a SoT 'puts in a heap' is for example 'new objects', objects that did not exist before, such as the number zero in the case of the postulational SoT or the concept of species in the case of taxonomic SoT, or 'new phenomena', which did not exist before the emergence of the laboratory SoT, e.g. the laser or the Compton effect.

Furthermore, as Stéphanie Ruphy points out, 'new objects' do not simply
add further to all the objects of our knowledge but determine an 'ontological enrichment of the objects of science, to the extent that the use in scientific practice of different styles of reasoning widen and diversify the classes of propositions that can be true or false about them' (Ruphy 2011, p. 1220-1223). She gives the example of a forest fire: the statistical style adds statistical properties and the laboratory style adds controlled and purified versions of the natural phenomenon. The result is an object ontologically richer than the object 'forest fire' of the everyday life.

A first point we can make is that in as much as new questions, objects and phenomena are introduced by SoTs, their contingency is parasitic on that of the SoT they belong to. Ultimately, the contingency at the level of styles of reasoning implies a contingency at the level of the ontology.

The next question is whether the knowledge acquired by all the SoTs converges on any unity. I think that one can give a straight answer in the negative for several reasons. Indeed, the concept of convergence implies that what it is converged on is something that for some reason is final and completed. But the process by which many items of knowledge emerge is open-ended. Firstly, within a SoT new phenomena as well as new objects are in principle 'created' continuously. Secondly, new SoTs may emerge in the future, e.g. as consequence of the informational revolution, bringing into being new questions, instruments and previously unknown phenomena.

Furthermore, it might be impossible to connect all we have already found out by adopting the six SoTs, from the evolution of disc galaxies to how enzymes work (see also Hacking 1996). Firstly, even though it were possible in principle to connect logically different parts of sciences there would not be certain that actually,
soon or later, they would get connected. The history of physics offers many examples of different accounts that ended up being considered equivalent: e.g. the two different formulations of the differential calculus of Newton and Leibniz or the wave and matrix mechanics formulations in quantum mechanics. But, as Hacking points out, that was a contingent fact:

[M]en like Laplace and Lagrange working around 1800, were in some sense obtaining consequences of Newton’s laws of motion [...]. They had to invent the language in which the conclusions could be expressed. [...] They were not just joining up the dots to complete a picture. They had to put in the dots (Hacking 2002 [1982] -a, p. 175).

In addition, the theory of SoTs opposes two ‘theses of unity’, which Hacking has discussed in ‘The disunity of science’ (Hacking 1999b, p. 76). The first one is a thesis of interconnectedness: the idea that all the phenomena are related to each other. This is the conception of the world that urges scientists to unify all the fundamental forces in terms of a single field. The second is a structural thesis: the idea that the truths about the world are connected with each other and form a structure in which some of them imply all the others. In particular, the advocates of this thesis might maintain that there are certain causes, necessary or probabilistic, which imply all the other causes of phenomena; or they might claim that there are logical relations between all the laws of the universe. Some scientists are neither reductionists nor holists but they believe that all the explanations stand in logical relation (Hacking 1996, p. 46, 47).

If the laws of two classes of phenomena are not logically connected, it seems
impossible that these two classes of phenomena are interconnected. So, to deny the second thesis means to deny the first one too. Now, the theory of SoTs denies the second thesis. Indeed, for instance, the kind of explanations provided by the historico-genetic SoT are radically different from those provided by the laboratory SoT. The historical explanations have the character of a story that cannot be tested in a laboratory.

The points I have made in this section make it clear that the knowledge that we acquire by adopting different SoTs has no unity and cannot converge on any kind of unitary description of the world. It is for this reason that Hacking's metaphor is appropriate: there are 'heapings up' of knowledge because each SoT accumulates new laws, phenomena, facts, and objects that cannot be connected by a single description of the world. The idea of convergence of science on a single true, coherent and complete description in the long run is at odds with the mechanism by which the SoTs produce knowledge: the growth of many different epistemological items is open-ended and never final and complete.

9.6 Conclusions

In this chapter, I have considered a number of connections between various ideas of the theory of SoTs and the issue of the inevitability of science. By addressing four fundamental questions I have discussed how both inevitability and contingency play a major role in the evolution of sciences. On the basis of my discussion, I advocate a position between the two extremes of contingentism and
inevitabilism. Given the point of origin of the evolutionary tree of the SoTs, i.e.
the birth of the algorithmic SoT, the expansion of trunk and branches is a random
growth away from simple beginnings that produces more and more complexity.
Chapter Ten: Concluding Remarks

10.1 Summary of the argument

In chapter two and three I have given substance to the notion of SoT by characterizing six modes of thinking that, although different in nature, subject matter and historical trajectory share a set of common features. SoTs are not only specific ways of reasoning but also ways of doing and finding out; they are long lasting, have sharp beginnings, possess techniques of self-vindication and introduce new objects, standards of truth and candidates for truth or falsehood. The internal techniques of self-vindication - I have argued in chapter four - make SoTs different from other ways of thinking, in particular those that are not in use in scientific research.

Despite the doubts expressed by Hacking and the close connection between his kind of study and what Netz has called ‘cognitive history’, in chapter five I have argued that the SoTs project fits into the area of research of historical epistemology. Tracing the connections between historical epistemology and a specific French tradition in philosophy of science has helped me to reinforce this thesis. I have then compared Hacking’s SoTs project with Daston’s study on objectivity and Williams’s genealogy of knowledge. Thanks to these comparative analyses I have identified points where the three projects support one another and
I have concluded that SoTs can be viewed both as different ways of being objective and as different perspectives from which to look at the world.

In chapter six I have started to draw some major implications from the SoTs project. First of all, in the light of a line of inquiry undertaken by the philosopher Edward Craig I have raised a philosophical issue, which I have called the incommensurability issue, and I have characterized it within the context of the recent discussions in philosophy of science. In brief, the issue amounts to the question as to whether there is an independent and atemporal justification for every claim made in a certain SoT. My answer has been in the negative and my argument can be found in chapter seven and eight. I have argued that the claim of entity realism made by Hacking is a case of a claim in the laboratory SoT that has no independent and atemporal justification outside it: whether the laboratory SoT is a way of finding out nearly theory-free (as Hacking claims) or not, Hacking’s characterization of SoTs implies that entity realism is justified in the laboratory SoT and unjustified outside of it. This is a thesis of incommensurability: two different communities that at different times adopt two different SoTs may not have common research standards to assess the truth of a scientific claim. At a more general level, my point implies that Hacking’s experimental realism – the claim that we know about unobservable entities - is not consistent with the epistemic relativistic implications of his SoTs project.

In chapter eight I have offered a more general argument for the thesis of incommensurability that concerns all the SoTs, i.e. an argument that is not specific to the laboratory SoT. I have compared the notion of SoT with Wittgenstein’s
notion of form of life and concluded, as a general fact concerning all the SoTs, that they are ungrounded and epistemically incommensurable.

Finally, I have examined the implications of the SoTs project for the contingency issue. I have 1) given a picture of the evolution of sciences in terms of the emergence and stabilization of the SoTs 2) discussed how both inevitability and contingency play a major role in the emergence and endurance of SoTs 3) pointed out that each SoT is grounded in certain objective features of the portion of the world it studies 4) argued that there are good reasons for being inevitabilist about the convergence of science on correct answers to single questions; although, as Hacking maintains, it is contingent that those questions have been asked. Point 3) and the self-authenticating character of SoTs can explain why SoTs are long lasting. This relevant point can be drawn from my discussion in section 4.2 and 9.3. To finish, on the basis of the features of the SoTs I have concluded that the idea of convergence of science in the long run on a single true, coherent and complete description does not jibe with the claim that the SoTs continually enrich our present ontology.

10.2 Looking ahead

I would like to conclude this dissertation by introducing some possible themes for future research. One of the aims of this thesis has been to draw philosophical implications from the SoTs project and assess them both in terms of coherence internal to Hacking’s philosophy of science and in terms of comparative analyses with other authors. To this intent, in the first chapters I have developed a theory of
SoTs by relying on Hacking’s suggestions as well as on most of the sources he considered valuable. I now want to stress that in order to subscribe to Hacking’s account of SoTs, or my developed version, it would be necessary to present independent evidence. In particular, it could be a task for further research to analyse Hacking’s sources and compare them with other texts. Hacking’s claims about the emergence and the stabilization of SoTs should be tested against a wider portion of history of science. For example, the methods of historical epistemology could be used in order to investigate a point that is crucial for the incommensurability claim: Hacking’s point that SoTs have sharp beginnings, which mark a point of discontinuity in the history of thought.

Viewed as a study on how new concepts and sentences become possible, the SoTs project needs a more detailed comparative analysis of the different responses given by the historical epistemologists to the problem of how objectivity changes. The theory of SoTs would also benefit from the contribution of cognitive and biological sciences. Indeed, if this theory can be described as a work of cognitive history, first of all it is necessary to identify better the cognitive capacities that led to certain ways of acquiring knowledge. It might also be that by identifying new ways of tapping human resources, for example by investigating the impact of the Internet on our actual ways of knowing and doing, we can discover new SoTs.

I also think that the theory of SoTs has much to say in relation to the problem of demarcation between science and non-science. Hacking has suggested that only the sciences exhibit styles of reasoning that have developed self-authenticating techniques. However, he did not develop enough the concept of
self-authentication, perhaps because he was not interested in a normative criterion for establishing what is scientific and what is not. Yet I think that the more we characterize the ways of thinking of the sciences the more we understand about what distinguishes science from non-science.

To conclude, I have conducted a philosophical analysis of Hacking's SoTs project. It seems to me that the most pregnant conclusion of this analysis is that, if we accept Hacking's characterization of SoTs, our ways of knowing are ungrounded - our knowledge has no foundations. Moreover, we must be prepared to accept the idea that in some cases our claims have no atemporal and independent justification. Having said that, I think it is important to realise that it is possible to assess these conclusions by carrying out a closer examination of the SoTs features suggested by Hacking. I do feel that it would be interesting to continue the investigation on this basis. My belief is that history of science and epistemology would have to play a decisive role, together with philosophical argumentation.
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