Scientific realism and the contingency of the history of science

Emiliano Trizio
Seattle University
Archives Poincaré, Nancy
Archives Husserl, Paris

1. Introduction: in the maze of possible histories of science

The temptation to imagine alternative historical developments of political, social, and cultural phenomena has always been a strong one. It is thus not surprising that one might try to include the history of science in this exercise and wonder whether it could have ended up differently than it actually did. Yet this attempt becomes problematic as soon as we impose epistemological conditions on the alternatives whose possibility (and plausibility) we are trying to evaluate, because, in order for the imagined scenarios to be interesting from an epistemological point of view, 1) we need to refer to an at least roughly defined specific subject-matter such as optics or high energy physics, 2) the imagined alternative histories of science must arrive to a different and yet *equally successful* stabilized stage, 3) they must imply some fundamental disagreement on the subject-matter in question. We are, therefore, putting heavy constraints on the alternatives we whish to consider, for most of the possible histories of science differ from our own in ways that are epistemologically uninteresting due to one or more of the following reasons: they are about the investigation of subjects other than the actual ones; they are histories of failure, not of achievement; they lead to results that are not incompatible with ours and therefore can be combined with them. The epistemological relevance of these three conditions should not be missed. Indeed, there is little epistemological interest in comparing what our science says about planets with what one might have ended up thinking about plants, or with what a bunch of fools unable to conduct any scientific research could have dreamed about planets, or, finally, with a planetology differing from ours as to the nomenclature only. Keeping this in mind, we can now turn to the relevant definitions.

Contingentism, as it has been defined by Ian Hacking,[1] is the claim that the history of a particular field of science could have turned out otherwise than it actually did, and that it could have resulted in a science as successful as the actual one but, in a non-trivial way, incompatible with it. Inevitabilism consists in the denial of this claim.

All crucial terms involved in this definition are affected by a certain degree of vagueness and can be defined in multiple ways,[2] but probably the trickiest of them is the term "successful". There are of course different definitions of scientific success, depending on the aim that one assigns to science.[3] Following Hacking, it is reasonable to include in the idea of scientific success a certain degree of progressiveness. However, the idea of progressiveness already implies a number of positive features that admit of improvement (such as predictive power, technical achievements, etc.). We shall see that the notion of success, along with the even thornier idea of evaluating "degrees of success" can be better analyzed while examining specific contingentist scenarios.

If we believe that the history of the scientific investigation of a subject matter could have led to a stabilized stage as successful as our own, but incompatible with it, we also believe that different mutually incompatible and equally successful scientific accounts of the subject matter in question are possible. I call the latter claim the *multiplicity thesis*. Contingentism, therefore, implies the multiplicity thesis. More precisely, the contingentism can be equivalently reformulated as the conjunction of the multiplicity thesis with the claim of the possibility of an alternative history of science leading to one of the successful alternatives incompatible with our own science.

A logically weaker form of contingentism (and, respectively, a stronger a form of inevitabilism) is sometimes evoked when one approaches the issue by asking whether *any* scientific investigation as successful as ours of a given subject matter would need to lead to roughly the same results as ours. For instance, one can ask whether, had modern science developed outside the Western world and had it reached a level of success comparable to ours, it would have necessarily achieved the same results, or, to push the example to the extreme, one can ask whether the results of an alien science as successful as ours would have to look pretty much like those that are familiar to us. The claim that, say, an alien science could be different than ours while enjoying the same degree of success is logically compatible with the idea that we could not possibly

[1] Hacking 2000.
[2] For a detailed analysis of the most interesting alternatives, see Soler 2008a.
[3] See Allamel Raffin and Gangloff in this volume chapter??.

come out with that science either for wont of material and intellectual resources, or for intrinsic features of our historical starting point. The multiplicity thesis is thus compatible with the denial of contingentism as previously defined: some alternative successful sciences might be possible, but simply *de facto* out of *our* reach, given our cognitive structure and the cultural and scientific stage at which our research into the a certain subject-matter developed, or, for short, given the cognitive, cultural, and scientific background underlying the research. In this article, unless otherwise stated, contingentism will be intended in this stronger sense, that is as implying that some successful alternatives remains open even once fixed the background of the research.[4]

One further point needs discussion. It will be argued that the opposition between inevitabilism and contingentism, thus defined, somehow presupposes a more or less fixed notion of *science*. This is certainly the case. We are trying to understand what *degrees of freedom* are left to the historical evolution of that cognitive activity that we call *science*, no matter how difficult it is to specify, in general, its nature. In other words, we are concerned with the extent to which successful science is *bound* to evolve the way it does; we are not concerned with the deeper issue of whether, as cognitive subjects, we are *bound by* the standards of scientific rationality, let alone with the even more fundamental problem of whether there are universally binding standards of rationality at all. If we drop these constraints and thereby also any shared notion of success, we also give up any epistemological criteria restricting the family of

[4] We can therefore introduce the distinction between weak and strong inevitabilism (and, respectively, strong and weak contingentism): *weak inevitabilism* is inevitabilism as defined by Hacking and is logically compatible with the multiplicity thesis. It is the claim that *our* history of science could not have led to alternative stabilized stages as successful as ours of the investigation of a given subject matter. *Strong inevitabilism*, instead, is incompatible with the multiplicity thesis; it implies that equally successful mutually incompatible stabilized stages of the scientific investigations of a given subject matter are impossible. Therefore, given a certain subject matter, one can only allow for mutually incompatible scientific accounts of it enjoying degrees of success that are sharply different from one another. According to this view, any historical trajectory leading to a theory incompatible with ours must lead either to a theory less successful than ours or to a theory more successful than ours, that is either less advanced or more advanced. Simply put, according to strong inevitabilism, mutually incompatible successful scientific accounts of a given subject matter must form a series of increasing successfulness; hence they could be, potentially, different successive steps in the investigation of a subject matter. To go back to the previous example, if intelligent aliens have developed a particle physics as successful as ours, according to weak inevitabilism that physics might be one we could not possibly have come up with in the course of our historical trajectory; while, according to strong inevitabilism, an alien particle physics as successful as ours should necessarily have to look pretty much like our own.

In this respect, I should also add that the note 5 of my 2008 article contains a mistake (Trizio 2008, p. 254), for it equates strong inevitabilism with the thesis that there is only one possible account of a given subject matter that could ever deserved to be called successful. The latter thesis would instead amount to a sort of "extreme" inevitabilism asserting that success does not come in degrees and that there is only one possible successful account of a given subject matter. As one can see, the maze of possible history of science is quite intricate, even setting aside the complex problem of giving a satisfactory characterization of scientific success and a clear criterion for its evaluation.

cognitive activities we are considering, and we end up comparing epistemic "forms of life" that may have little or nothing in common.

These introductory remarks suffice for the purpose of this article, which is twofold: first, to circumscribe and analyze the conflict between contingentism and scientific realism; second, to characterize it from a methodological point of view. The term *scientific realism* will cover the family of theses according to which our successful scientific theories make claims that are true (or approximately true) about the aspects of the world that they describe. I will adhere to the canonical distinction between scientific realism and *metaphysical realism*, which is the thesis according to which the aspects or parts of the world investigated by science have a given, intrinsic nature, whether we succeed in acquiring knowledge about it or not. According to metaphysical realism, there exists a *true description* of the entities and processes inhabiting the world, a description that our theories try to approximate. I also assume that scientific realism implies metaphysical realism, while the contrary does not hold.[5]

More precisely, in *section 2* I will reconstruct the antirealist motivations of the classic contingentist scenarios developed by James Cushing and by Andrew Pickering; in *section 3*, by taking into account some versions of scientific realism that are more sophisticated than those discussed by contingentists up to now, I will clarify the logical relations of compatibility and incompatibility existing between contingentism and inevitabilism on the one hand, and scientific realism and antirealism on the other; in *Section 4* I will try to spell out the specific contribution of contingentist historical reconstructions to the critique of scientific realism; finally, in *Section 5*, I will recapitulate the results of the article and argue that the conflict between contingentist antirealism and scientific realism can be seen as a clash of inferences based on interpretations of the history of science. This article will thus consist of a philosophical meta-analysis of a controversy existing between different meta-scientific investigations.

[5] I prefer this formulation to the slightly different one taking a metaphysical realistic thesis as component of the definition of scientific realism, as it is done by Stathis Psillos (see Psillos 1999 and 2000). Throughout this article, words such as realism or antirealism used without further specification refer to *scientific* realism and antirealism.

2. Contingentist scenarios as challenges to scientific realism

Questions about the contingency of the history of science can in principle be discussed without reference to the debate over scientific realism; nevertheless, most of the works that have raised the issue were written with the intention to challenge standard realistic standpoints[6] or, at any rate, to provide a framework for discussions over anti-realist constructivism.[7]

There are, as far as I can see, two different scenarios giving a more precise content to the idea of alternative successful developments of the history of science. The two scenarios correspond to two ways in which the multiplicity thesis can be declined. This first is the good old *underdetermination scenario*, according to which a given subject matter could be described by different mutually incompatible theories that, nevertheless, equally succeed in accounting for some of all the relevant phenomena.

The multiplicity thesis in this case would boil down to the underdetermination thesis. In order to find examples of this type, we would need to look for a successful theory that was developed at a moment in which an alternative underdetermined theory could have been conceived, given the historical background existing at the time. Let us further notice, that the strongest possible argument for contingentism would be based on the very existence of an alternative incompatible development,[8] therefore, ideally, we should also be able to produce the alternative theory or, at least, the embryo of it.

As a matter of fact, there is one detailed example of such a contingentist scenario based on underdetermination and on an historically plausible reconstruction of a counterfactual history of science, an example meeting also the strong demand about the possibility to produce the core of an alternative theory, and it is described by James Cushing in his '94 book *Quantum mechanics, historical contingency and the Copenhagen hegemony*.[9] It clearly illustrates that, during a scientific controversy over

[6] See, for instance, Pickering 1984, Cushing 1994.
[7] Hacking 2000.
[8] See Soler 2008a pp. 235-241 for a thorough analysis of the different possibilities of conflict between rival theories, conducted in the interesting framework of the "divided physics" thought experiment.
[9] Cushing 1994. More recently, Kyle Stanford has developed a new detailed argument for the underdetermination of scientific theory by empirical evidence. According to Stanford "we have, throughout the history of scientific inquiry and in virtually every scientific field, repeatedly occupied an epistemic position in which we could conceive of only one or a few theories that were well confirmed by the available evidence, while subsequent inquiry would routinely (if not invariably) reveal further, radically distinct alternatives as well confirmed by the previously available evidence as those we were inclined to accept on the strength of that evidence." (Stanford 2006, p. 19). These historical facts are

a new theory or experimental results, what matters is not only the very fact that somebody comes up with an idea (a contingent factor that should not be underplayed), but also at what point of the controversy that happens. Indeed, in any debate, the temporal order in which arguments and counterarguments are given can turn out to be decisive. Let us recall the essential traits of this story. In 1952 Bohm developed a version of quantum mechanics empirically equivalent to standard quantum mechanics, but radically different at the ontological level. As Cushing puts it, the two theories share the formalism, but not its ontological interpretation.[10] In particular, at the ontological level Bohmian mechanics is much more similar to classical physics for it ascribes to each particle at each instant a defined position evolving in a deterministic way. Further, it explains the collapse of the wave function as a consequence of the equations of motion, thus abolishing the special status the standard quantum mechanics assigns to the observation process and the thereby related paradoxes of quantum measurement. Bohmian mechanics does imply paying the price of non-locality, but so does standard quantum mechanics.[11] Cushing's thesis, which is supported by a careful reconstruction of the scientific debate between the 20ies and the 50ies, is that it is only a matter of historical contingency if that theory was not put forward in the 20ies. For that, only a few other results would have been needed, such as the proof that the instantaneous collapse of the wave function does not contradict the no-signaling principle of special relativity. All of these results could have been obtained with the theoretical resources available at the time.

> The choice would then, early on, have been starkly clear: *either* a realistic, nearly classical worldview based on a theory like Bohm's, with the price of non-locality, *or* an indeterministic and nonlocal Copenhagen worldview with its truly bizarre ontology and a radical, revolutionary departure from any comprehensible "picture" of physical process. The causal quantum-theory program could have been off and running.[12]

---

taken as the inferential basis for a new "induction over the history of science", whose conclusion is that "there typically are alternatives to our best theories equally well-confirmed by the evidence, even when we are unable to conceive of them at the time." (Ibid. p. 20). As a matter of fact, Stanford does not use this thesis to argue for contingentism, for he targets directly scientific realism. For this reason, and even if Stanford's analyses could be exploited to make a case for contingentism, I will focus on Cushing's work.

[10] Cushing 1994, p. 9.

[11] The non-locality of Bohmian mechanics is illustrated by the case of entangled states of a system of particles. In such states the velocity of a particle depends on the positions of other distant particles of the system.

[12] Cushing 1994, p. 186. Ironically, a theory such as Bohm's, whose existence is used by Cushing as a weapon against scientific realism, presents a picture of reality that is much more "realistic" than that of

This counterfactual scenario is particularly interesting because it is based on the modification of historical occurrences that one could hardly consider inevitable such as the temporal order of events that actually took place in the mind of a handful of researchers. The choice Cushing refers to could have not been done on the basis of logical coherence and experience alone, and, in cases like this one, it is legitimate to suppose that social and cultural factors, not to mention subjective preferences, play an important role in the final decision. The case at hand is particularly interesting also because what now appears to us, accustomed as we are to the oddities of standard quantum mechanics, as a bizarre quasi-classical quantum theory would have looked much more palatable in the early Twenty Century precisely on the grounds of its conservative character with respect to the at that time dominant paradigm of classical physics.[13]

Historical contingency is thus used by Cushing to destabilize the belief in literal truth of the "worldview" deriving from physical theories. There is, though, a second, more radical, way to draw anti-realist arguments from contingency: it is what can be called the *robust fit scenario*. In a nutshell, the idea is that successful science is not based on a predictive or explanatory match between theories on the one hand and fixed phenomena on the other. The so called "phenomena" emerge from a complex interplay of several practical and theoretical items ranging from raw data, techniques of data analysis, and methods of approximation, to background theories, accepted experimental facts, and phenomenological laws, and including the very material aspects of the relevant equipment as well as its expert use. According to this account, there is no experimental bedrock invariant throughout history that all rival theories would have to predict and explain. Rather, experimental activities and theoretical beliefs must co-stabilize in such a way that they produce a robust fit. The key-aspect of this process of co-stabilization consists in a sort of generalization of Duhemian holism to the ensemble of aforementioned ingredients of experimental science,[14] ingredients

---

standard quantum mechanics. Of course then, when Cushing defends the rights of the "realistic" Bohmian mechanics to be acknowledged as a legitimate alternative, he is not thereby defending scientific realism. One thing is the realism *of* a scientific theory, quite another the realism *about* scientific theory.

[13] Indeed there is same perversion in the way in which the paradoxical character of a theory is used for or against it depending on whether it is already in a dominant position or not. Much in the same way, the eccentric behavior of a celebrity is taken as a sign of genius, whereas that of an unknown man is judged as a pathetic weakness of the mind.

[14] For a clear statement of the extended Duhem Thesis, see Hacking 1992 pp. 30-31 and 52-55.

that, let us stress it once more, are not restricted to intellectual items, but include also material ones. According to this extended holism, when the researchers' expectations are disappointed by the upshot of the experiments, all the items of the list can, in principle, admit of modifications, in view of restoring coherence among them. Crucially, the so-called experimental data, whether raw or interpreted, are no more fixed and given than any other item. In the sense, the evolution of experimental science implies always a co-evolution of intellectual, material, and practical elements whose aim is the achievement of a robust fit, that is a configuration in which each element works well in the system of all other elements.[15] It is precisely the need of achieving virtuous adjustments among the components of experimental science, that put constraints on the scientists' choices.

The multiplicity of possible robust fits even within the investigation of a single subject matter would now amount to a new version of the multiplicity thesis. This is, in short, the conception emerging from Pickering's sociological history of particle physics. Pickering's work constitutes the constructivist approach to science that is more explicitly tied to the notion of contingency.[16] His historical reconstruction is explicitly presented as a contingentist alternative to the way in which scientists tend to view the history of their own field, that is on the basis of the belief in the truth of the theories that have ended up being accepted, and in the existence on the entities postulated by them. This ontological bias retrospectively renders unproblematic, to the scientists' eyes, the choices made in the past, which were responsible for the emergence and acceptance of what came to be their world-view.[17] Note once more that, as it follows from the thesis of extended holism, those choices concerned both which experimental results had to be accepted as established "facts" or "phenomena" and what theory should be retained as more capable of explaining them. Pickering argues that the choices made in the history of particle physics were in no way determined either by experimental "facts" or by any available method:

[15] This situation can also be described by means of the metaphor of symbiosis. A robust fit would be a situation in which there obtains a good symbiosis among the ingredients of experimental science. No such item, including empirical data, could thus have, so to speak, a life on it own, for it can be valuable only in a community of "symbiotic" items. The symbiotic metaphor has been introduced and developed by Pickering, see especially Pickering 1995. See also Soler 2008c.
[16] Philosophical discussions of the various constructivist approaches can be found in Hacking 1999 and Kukla 2000.
[17] Pickering 1984, p. 7.

Historically, particle physicists never seem to have been *obliged* to make the decisions they did; philosophically, it seems unlikely that literal obligation could ever arise. This is an important point because the choices which were made *produced the world of the new physics*, its phenomena and its theoretical entities. As we saw in most detail in the discussion of the neutral current discovery, the existence or nonexistence of pertinent natural phenomena was a product of irreducible scientific judgments.[18]

Pickering's final point is that there is no obligation to "take account" of the ontology of particle physics, on the grounds of its being a contingent cultural product.[19] The realism defended by certain scientists is, in his view, a mistake that fosters an inevitabilist view of the history science, reinforcing the mistake itself.

One should not miss the sharp difference separating Cushing's and Pickering's brands of contingentism, difference that clearly surfaces in the two passages quoted above. Cushing talks of the contingency of a *worldview*, while Pickering refers to the contingent production of the *world* of the new physics.[20] Those terminological choices mark different if not opposing attitudes towards *metaphysical realism*. Cushing does not question metaphysical realism; rather, he seems to presuppose it, for he defends a form of skepticism about the power of successful physical theories to yield a reliable ontological picture *of* reality. According to Cushing, physical world*views* can prove to be untrustworthy representations of the *world itself*.[21] His conclusions, as we have seen, rests on a fully representational analysis aimed at showing that physical ontology is underdetermined by empirical evidence. Pickering, on the contrary, was heavily influenced by Kuhn's notions of world-change and incommensurability and by the metaphysical anti-realism that he sometimes associated with them.[22] Not only does Pickering claim that the old and the new particle physics predicted and explained different sets of data and different phenomena, but he tries to build on this interpretation a non-representational, agency-based, account of the very notions of

[18] Ibid. p. 404.
[19] Ibid. pp. 413-414.
[20] In other passages of the book the expression "production of a world" and "production of a worldview" are used interchangeably, see, for instance, Ibid. p. 405 and p. 407.
[21] See Cushing 1994, p. 215: "Successful theories can prove to be poor guides in providing deep ontological lessons about the nature of physical reality." See also Cushing's subsequent reference to Quine's naturalistic account of underdetermination.
[22] For a recent development of this position, see also Pickering 2012. For Kuhn's metaphysical antirealism see Kuhn 2000, p. 104: "The ways of being-in-the-world which a lexicon provides are not candidates for true/false." See also Kuhn 2000 pp. 219-221.

world-change and incommensurability.[23] He believes that a careful analysis of historical cases-studies undermines “the intuition of uniqueness” motivating metaphysical realism and, in turn, inevitabilism.[24]

Note further that the difference between the two scenarios is not without consequence for the clause “equally successful” contained in the definition of contingentism. Indeed the predictive success of two theories can be compared to certain extent as long as they both try to account for the same phenomena. This comparison becomes however more and more difficult if, as one envisages under the robust fit scenario, there appear discrepancies between the data or the phenomena themselves. It this case, I suggest that the clause “equally successful” be construed as “both very successful, without any way to decide which is more successful than the other”.[25]

These examples illustrate what is at stake in many discussions concerning the contingency of the history of science. They both explicitly imply a criticism of realistic standpoints: Pickering focuses on scientists’ spontaneous realism, while Cushing challenges an unqualified belief in the ontological reliability of physical theory. However, what is still missing is an analysis taking into account more elaborated version of scientific realism and investigating the logical relations between the latter and contingentism. To what extent is scientific realism *incompatible* with the

---

[23] Incommensurability is here intended in a sense stronger than the original Kuhnian one, as implying the impossibility to adjudicate between two theories. On Kuhn’s own view about the relation between incommensurability and incomparability see Kuhn 2000, pp. 33-57. For general analyses of Kuhnian incommensurability see Hoyningen-Huene 1993, pp. 206-222, Soler 2004, and, if I may, Trizio 2004.

[24] For one of the most recent versions of Pickering’s “ontological”, agency-based contingentism see also chapter ?? of this volume. A host of philosophical challenges that cannot be discussed here awaits whoever tries to abandon metaphysical realism, whether this is done adopting a representational or an agency-based framework. *Inter alia*, the efforts of thinkers like Pickering must be at once to give a precise sense to and convincing arguments for the thesis that the choices of physicists *produce a world* and not just a world*view*, for, clearly, this requires a sense of “production” and of “existence” that are not the ordinary ones. Indeed, if scientists’ choices had literally brought elementary particles into existence, the scientists’ realism about them could hardly be criticized. Doing it would not differ much from saying that God was wrong in believing that the world exists, right after he created it. Hence, an entire redefinition of notions such as “existence of the world” and “existence in the world” are needed. Moreover, Pickering must persuade us that his account of scientific practices is not entirely compatible with metaphysical realism. Are we sure that the world could not just be endowed with a fixed, inner structure rich, complex and inaccessible enough to support a huge variety of performative engagements *in* and *with* it? More generally, what needs to be proved is that the choice between metaphysical realism and metaphysical antirealism is not underdetermined by all the evidence that can ever be provided by empirical research *on* science. To paraphrase Cushing’s skepticism about the ontological import of physical theory, sciences studies might just prove to be poor guides in providing deep lessons about the very *ontological status* of reality. For the purpose of this article, this is an important point, for, in principle, contingentism and the multiplicity thesis can be stated in terms of the notion of robust fit without endorsing metaphysical antirealism.

[25] I prefer this solution to my earlier view according to which, in cases like this, the clause “equally successful” should be simply dropped (see Trizio 2008).

contingency of science? Or, more generally, where would a scientific realist stand on the contingency issue?

3. To what extent is contingency compatible with scientific realism?

Scientists' scientific realism is not philosophers' scientific realism. The former is voiced at time by some members of the scientific community not rarely in the form of a blunt faith in the unshakable truth of scientific achievements. It surfaces mainly in debates taking place outside the laboratories and the academic institutions, and it is not even clear that it is so widespread among scientists themselves.[26] Normally, for such realists, the experimental evidence available for a scientific theory is enough to believe in its literal truth. In contrast, professional philosophers of science who advocate one or another version of scientific realism do not argue for their position by simply pointing out the evidence scientists purport to have for their theories, nor do they defend a theory against a rival one: their analyses take place at the meta-level at which one wonders which epistemic attitude it is rational to adopt towards what science in general, or more frequently, from a specific branch of it teaches us about the observable and the unobservable aspects of the world. To say that the properly philosophical debate about scientific realism takes place at a meta-level with respect to the level of working scientists in no way means to claim that that debate is entirely and necessarily based on purely philosophical, *a priori* arguments. Quite the contrary: nowadays the vast majority of those who are occupied with the issue, whether in the realist or in the antirealist camp, share one or an other variety of epistemological naturalism, or, even when that is not the case, tend to be skeptical towards the possibility and legitimacy of a foundational philosophy. Their philosophical contributions owe a lot to the traditional logical analysis of the relation between theory and evidence, but are nourished also by the results of empirical disciplines such as history and sociology of science, (or even cognitive science), results that are often produced by highly specific case-studies, or detailed reconstruction of historical

[26] Scientific realism in general should also not be confused with what can be called the *realistic attitude* of scientists, that is the objectifying attitude inbuilt in the engagement in scientific research, in virtue of which the claims and theories resulting from the latter are in most cases implicitly intended as tentative descriptions of how things really are. In virtue of this attitude, criticism of a claim is only a way to argue for competing scientific claims, or for the need to conceive of them: it never becomes a criticism of the epistemic limitations of science as such.

episodes. The latter point is particularly important for us. Most versions of scientific realism (and scientific antirealism) are developed from a philosophical standpoint according to which the epistemic import of a scientific discipline becomes understandable only if that discipline is considered as embedded in its history,[27] if not in the wider social context surrounding it, or even in the natural history of humans as cognitive agents endowed with certain mental capabilities. Philosophy of science and science studies in general develop, in this way, a critique of a more or less broadly conceived *scientific* world-view, a critique which, in most cases, is nourished by the empirical results of one or another kind of (more or less broadly conceived) *scientific* investigations. Today's trend thus contrasts with more formal and a priori approaches such as logical empiricism, neo-Kantianism, and phenomenology that were predominant before World War 2. As we shall see, this has important consequences for the very nature of the answers presently given to traditional philosophical questions such as those concerning scientific realism.[28] To develop a complete account of the different brands of scientific realism and antirealism lies outside the scope of this article. I will only single out the theses and the arguments that are more significant in order to understand the relation between contingentism and scientific realism.

It is by no means a coincidence that the whole contingency debate is framed in terms of successfulness, for, at least with respect to the field of natural science, the common starting point of scientific realists is the argument from success. It is the impressive predictive, explanatory and technical success of natural sciences that promotes epistemic optimism about the truths of their claims. In a word, how could our view of the natural world be entirely wrong, given the outstanding theoretical and practical accomplishments that derive from it? The realists' acceptance of a fallibilist epistemology will then allow them to revise at least part of their beliefs in the light of new evidence or new theoretical developments, without having to drop his epistemic optimism altogether, for, in a fallibilist perspective, knowledge does not equate to certainty.

[27] See Psillos 1999.
[28] One would like to say that philosophy of science and science studies are today, methodologically speaking, apart from a few exceptions, naturalistic, if many current research trends were not rather sociologistic or historicistic in character. Perhaps the best way to capture this state of affairs is to speak, as Andrea Woody has done, of a shift from a priori to the empirical, without prejudging what kind of empirical evidence is involved, (see Woody "The Turn to Practice: Rethinking Representation and Explanation" In progress).

As it is well known, the usual responses to this argument rest on the underdetermination of theory by empirical evidence and, most of all, on the so-called pessimistic meta-induction.[29] Indeed, in the past years, in the absence of a consensus of the actual import of arguments based on underdetermination, the debate has focused on the threat that the pessimistic meta-induction poses to the arguments from success.[30] Nevertheless, a refined version of realism will take up the challenge deriving from a pessimistic reading of historical records. A realist knows or at any rate expects that our current scientific theories will be modified by future scientific research in ways that cannot simply be equated to emendation, completion or improvement. The way out of the difficulty of mediating between the realist intuition that the success of science is a sign that its theories cannot be completely false on the one hand, and the various arguments akin to the pessimistic metainduction on the other is often given in terms of positions that can defined as “selective or preservative realism”. According to the latter, past theoretical changes must be taken seriously when evaluating the kind of epistemic warrant that our well-confirmed scientific theories can enjoy. The result is the attempt to specify what parts or aspects of scientific theories have been retained through theoretical change, and are likely to be retained also by future successful science. These parts or aspects will in turn be considered to be true or, more often, approximately true. There are several different versions of partial realism, but they all share the feature of being based on a discussion of actual historical case studies and to be compatible, to a certain extent, with the prospect of future major theoretical changes. Here is a short presentation of it based on the work of John Worrall.[31]

As we have seen, the historical records indicate that past predictive and explanatory successful theories, like Newton’s mechanics or Fresnel’s optics have been superseded by successor theories that postulate a very different ontology: electromagnetic field instead of the ether, curved space-time instead of the gravitational forces, etc. The history of science teaches us, therefore, that there is no continuity at the ontological level when a major theoretical change takes place. Structural realists accept this conclusion, but do not accept that the success argument in favor of the partial truth of science must be given up altogether. A form of realism can survive even if we give up the idea that the central theoretical terms of our

[29] Laudan 1981.
[30] But see again Stanford 2006 for an attempt to foreground the antirealist consequences of the doctrine of underdetermination.
[31] Worrall 1989.

successful theories must refer to real entities. By looking at actual historical cases of theory change, the structuralist aims at highlighting the existence of continuity at the structural/syntactic level in spite of the discontinuity at the ontological level. As it is well known, the most famous example of structural continuity was given by Poincaré: when the ether was replaced by the electromagnetic field, what was retained, according to Worrall was not only predictive power, for the forms of the equations governing optical phenomena were preserved by the new theory. The interpretation of the symbols appearing in the formulae changed, for the oscillations of the particles of the ether were replaced by the oscillations of the electromagnetic field, but, crucially, the mathematical laws governing these phenomena have the same forms. In conclusion: the predictive success of science does not guarantee knowledge of the entities that really inhabit the world, but only knowledge of the relations among them. There are of course several possible criticisms to this approach,[32] which remains, by and large, an incomplete research program that should be developed through a careful analysis of a huge number of different examples possibly issued from more recent scientific developments. Here I will not try to evaluate the plausibility of structural realism *per se*, for my aim is rather to address the relation a realism of *this kind* entertains with the contingency issue. These brief indications can suffice for our purpose. We can now go back to the problem of the relation between contingentism and realism.

As we have seen, the very notion of contingentism has emerged in the context of history-based critiques of scientific realism. However, it is important to understand that the couple inevitabilism/contingentism does not overlap with the couple realism/antirealism.[33] To see this it suffices to realize that inevitabilism can coexist with both scientific realism and antirealism. Let us recall that according to the inevitabilist, it is impossible that the history of science could have yielded a scientific account of a given subject matter as successful and ours but incompatible with it. Now, this tenet is of course logically compatible with the view according to which it is rational to believe that some or all theoretical components of our current science are literally true, but it is also compatible even with the gloomiest version of the pessimistic metainduction. One could endorse the view according to which all of our

---

[32] For instance, as Hasok Chang has shown, preservative realists face the problem of having to rely on controversial continuity claims about the series of past successful theories, and on bold inferences from this alleged continuity to truth. See Chang 2003.
[33] See Soler 2008a and Sankey 2008.

scientific theories will be abandoned in due time, and replaced by new, wildly different ones, and still claim that there is a certain fixed pattern in the succession of successful theories that the history of successful science must inevitably follow. For instance, the shift from Newton's theory to Einstein's could be, according to this view, just as inevitable as the prophesized future shift from Einstein's to the "who-knows-what" theory that will supersede it washing away its ontology of curved space-time manifolds, more or less in the way in which the latter ousted the classic ontology of gravitational forces acting in absolute space and time. To be sure, the anti-realist inevitabilism would prompt different reactions than the realist one. The latter sounds as a controversial but fairly complete account of science, insofar as we can here generalize what Pickering has shown in the case of the scientist's realism, that is that their realism fuels an inevitabilistic reading of the history of science, and provides a sort of *post facto* intuitive (albeit unrigorous) explanation of the inevitability of successful science. In contrast, the antirealist inevitabilists could be at a loss about how to argue for the inevitability of the historical trajectory of successful science, given that the success of science, according to them, is not a sign of the truth of its theoretical claims about the world. As far as I can see, there is though a strategy that one could follow to render anti-realistic inevitabilism more than an ungrounded logical possibility: one could 1) endorse metaphysical realism, and 2) stress the role of the initial starting point of a research as a constraint on its future development.[34] As we have already indicated, given a certain subject matter, scientific research always develops on a cultural and scientific background. One could argue that, in particular, the scientific background mediates our access to the subject matter in question, or else that that objective domain appears to us in a certain way also because of the technical and theoretical resources allowing us to access it. This theoretical and technical mediation would thus constrain the way in which science will further develop, *given* the way reality is. Under this perspective, reality might well admit of different successful scientific accounts of it, and therefore there would be no guarantee that our account is the true one, and yet, given a certain theoretical and instrumental way of access to it, only once such an account is possible. In some sense, scientists will doomed by of their own scientific background to *read* reality in a certain way, even when that very same scientific background, or more realistically, a part of it, is abandoned in the course of the research: the past of science plus the way reality is would thus determine the

[34] But see, on this issue, Soler 2006.

future of science.[35] This position is admittedly very speculative, but it is worth mentioning it in order to map correctly the differences between the couple contingentism/inevitabilism and realism/antirealism.[36]

Given the obvious fact that a contingentist *can* be an antirealist, we still need to settle the issue whether a contingentist *must* be an antirealist, or, equivalently, whether, notwithstanding the anti-realist inspiration of contingentist accounts of the history of science, there is a form of scientific realism that can be reconciled with contingentism.[37] In order to defend the view that scientific realism is compatible with contingentism one might recall that most contemporary versions of scientific realism are not committed to an uncritical belief in whatever claim is derivable from successful theories. One might then argue that, after all, scientific realists can concede the possibility of alternative incompatible successful sciences accounting for a certain domain of objects, but then they would add that all these alternative routes, when successfully pursued, would progressively converge towards a unified final account of that domain. In contrast with antirealist inevitabilism, which is a position that, though logically coherent, is hard to establish, the combination of contingentism with scientific realism does not even seem to qualify as a logical possibility. The reason is that scientific realism *does not amount* to what could be called *eschatological realism*, for it

---

[35] To be precise, this position would reconcile scientific antirealism with *weak* inevitabilism, an inevitabilism compatible with the view that it is not reality as such that "determines" the course of successful research, but, rather, reality coupled with some specific historical (or perhaps even biological) conditions of research. Instead, it is really hard to try and given any plausibility to a position holding together strong inevitabilism (which implies a denial of the multiplicity thesis) and scientific anti-realism. Let us add, in passim, that this discussion highlights the interest of the distinction between strong and weak inevitabilism. The former grounds the inevitability of our successful science in the idea that the *world* does not support rival accounts of it as successful as ours, while the latter is committed only to a claim concerning our historical trajectory.

[36] An extreme form of social determinism would provide another form of inevitabilist antirealism. In this a case, all the burden of explaining the inevitability of science will fall on the social, historical, cultural conditions surrounding the emergence of a scientific result. For this reason, it would be a form of weak inevitabilism, hence compatible with the multiplicity thesis. Needless to say, such an extreme form of social determinism is hard to defend for properly social and historical phenomena themselves, let alone for the innermost content of scientific achievements. I am indebted to Katherina Kinzel for reminding me of me the doctrine of social determinism.

[37] *Metaphysical* realism, as we already know, is compatible with contingentism. One should also mention that some constructivists, while rejecting metaphysical realism, hold unorthodox forms of realism that are deeply intertwined with contingentism. Pickering himself has subsequently characterized his own position as *noncorrespondence* or *pragmatic realism* (Pickering 1989, pp. 279-282). According to this position it would make sense to call "reality" (or at least "reality for us") precisely the contingent outcome of the processes of material and intellectual negotiation with the world that lead to the emergence and stabilization of scientific results. Needless to say, these forms of pluralistic or even relativist views of reality somehow "stretch" the very notion of realism in such a way that it can cover most of the positions that are normally termed anti-realist and constructivist (with the disadvantage that the only anti-realists would then be the radical empiricists such as van Fraassen). Hence, in this article, I prefer to conform to the standard terminology, and I discuss forms of scientific realism that imply metaphysical realism and are incompatible with constructivism and relativism.

does imply more than a vague confidence in fact that science will eventually yield a true account of the world. Again, I have to insist on the fact that the alternative routes must differ in what are called stabilized stages of science. Any realist would admit that a host of practical and theoretical aspects of today scientific practices could have been different, and that, even the same theoretical or practical results could have been achieved in many different ways even once the historical series of stabilized stages is fixed. However, realism implies an epistemic optimism about our present successful scientific stabilized science, and that optimism cannot live up to the idea that other, wildly different science could be or could have been just as successful as ours. Incidentally, only a very weak form of contingentism is compatible even with eschatological realism, one according to which as science progresses, its development becomes less and less affected by contingency. Indeed, how is to be possible that all successful histories of science have to converge towards a final unified account of the world, if at each temporary stabilization of a particular field of research several bifurcations are always possible, as the original definition of contingentism requires?

Thus, at a very general level, there is no easy way to reconcile scientific realism and contingentism. Yet a more fine-grained analysis is needed, one that takes into account both a specific version of scientific realism and the differences between the two previously discussed contingentist scenarios.

Let us consider each contingentist scenario in turn, starting with that based on underdetermination. What could a structural realist say about that? As we have seen, structural realism does not consider the success of our present scientific theories as a reason to regard them as literally true. It is rather a meta-approach aimed at finding elements of theoretical knowledge that have shown to be more or less invariant under actual theoretical changes. We sense already that any kind of selective scientific realism, whether structural or not, any kind of realism that is, which would be based on the comparative analysis of actual successful scientific theories would not be troubled by talk of possibilities. It is a central feature of this approach to come to a conclusion about the realism issue and, in general about the evidential basis for inferring the correspondence to reality of a constituent of theoretical knowledge, only after a careful comparative examination of *actual* successful theories.[38] In order to see this in detail, let's go back to the multiplicity thesis. This thesis has to be made more precise if its

[38] This methodological choice would be easily dismissed if a general argument for the existence of underdetermined theories were at hand. But this is hardly the case at the moment.

implications for structural realism are to be worked out. In particular, we need to be more precise as to the nature of the supposed incompatibility between the rival underdetermined theories. A conflict at the level of the entities posited by the theories, for instance, would not trouble the structural realist at all. In this respect, the often-cited example of an imagined non-quarky high-energy physics, when reformulated in the framework of the underdetermination scenario, would not imply, in principle, a deep incompatibility at the structural level. In general, two underdetermined theories with a different ontology could share a deep structural similarity, and, if this were the case, their existence, far from constituting a threat for structural realism, would instead provide further evidence for it. In a sense, structural realism is designed to cope with the situation of empirically equivalent theories that postulate different kinds of entities, although it was based on the comparative historical analysis of different successor theories retaining their predecessors' empirical content, rather than on imagined globally empirically equivalent alternatives. In order to be harmful for this rather cautious form of realism, the multiplicity thesis must be sharpened in the following way:

> Given a certain subject matter, different scientific accounts of it are possible which are 1) equally successful 2) incompatible at the structural level.

Now, the multiplicity thesis thus formulated is incompatible with structural realism, and, hence, truth/plausibility of the former would imply the falsity/implausibility of the latter respectively. *A fortiori*, therefore, structural realism is incompatible with a contingentism based on this version of the multiplicity thesis, while it is fully compatible with a weaker contingentism restricted to the ontological implications of scientific theories.

The incompatibility between structural realism and a qualified version of contingentism has been discussed, so far, in the framework of the so-called underdetermination scenario. What can be said about the second contingentist scenario that we have considered, the one based on the notion of interactive stabilization and robust fit? Again, I will take structural realism as a representative of any kind of selective realism intended to draw consequences about reality from the comparative consideration of different successful theories. When turning to the robust fit scenario we fully appreciate the philosophical consequences of the so-called practices turn, with its insistence on the importance of the generative process of the experimental activities. As we have already noticed, the anti-realist arguments based on underdetermination need not challenge the solidity of the empirical evidence produced by experimentation. This choice has motivated philosophical analyses that focus almost solely on the representational aspects of scientific inquiry. Many types of scientific realisms have been developed in this vein, and the various kinds of selective realism are no exception to the rule. The quest for reality has been interpreted as fined-grained analyses of the parts of the representational contents that are deemed to account for the predictive success of science and that, furthermore, appear to be retained through theory change. To this approach, a multiplicity thesis based on the notion of robust fit could pose a very serious threat.

In the first place, it becomes more difficult to imagine a contingentist scenario of this kind that could be compatible with the kind of scientific realism I have considered. As we have seen, in this case, all the ingredients of scientific practices are allowed to vary, instrumentation, know how, techniques of data analyses, theoretical hypotheses and the data themselves. Now, it is certainly possible to imagine that two groups of researchers might get to the same theoretical result while using different instrumentations, know-how and techniques of data analysis. It is, instead, harder to see how they might get at the same conclusion, from a theoretical point of view, if the data and the models of data are different. The whole idea of looking for historically invariant components of theoretical knowledge that are responsible for the predictive success and its retention through theory change becomes problematic. Does it really make sense to look for structural similarities between theories that make different predictions verified with different experimental techniques? Here we come to a somewhat stronger opposition between contingentism and scientific realism.

In conclusion, it is impossible to reconcile "realism about X" with "contingency about X", if to be realist about X means to hold the view that the success of the theory implying X gives us rational grounds to believe that X or something similar to X actually exists or is true.[39] A realist about structures, as we have seen, while allowing contingentism about entities, would certainly be against contingentism about structures. Hence, with the previous qualifications, contingentism cannot be reconciled with scientific realism. But the result is also that this cannot be taken as an unqualified thesis. An unqualified contingentist thesis, that does not make explicit reference to the

[39] Although, to be sure, contingentism about X is not incompatible with the claim that X exists.

level at which the scientific investigations are deemed to be mutually incompatible (empirical basis, entities, structures…) is harmful only for a wholesale realism that takes virtually all our scientific claims as literally true, it is harmful at bottom for the realism of some working scientists.

4. The specific contribution of contingentist history of science to the critique of scientific realism

As we have seen, contingentism amounts to the conjunction of the multiplicity thesis with the claim asserting the possibility of a history of science leading to one of the supposed successful alternatives incompatible with our own science. The short discussion just presented should suffice to persuade us that the part of the contingentist thesis that is problematic for scientific realism is the multiplicity thesis, which can be seen as a sort of generalization of the doctrine of underdetermination. In sum, the scientific realists who recommend an optimistic epistemic attitude towards the ingredients of successful scientific theories that they deem preserved through theoretical change, cannot at the same time be contingentist about that ingredient, *i.e.* they cannot consistently endorse a multiplicity thesis involving it. This does not mean that scientific realists have the burden of the proof that the multiplicity thesis involving the components of scientific theory they are realists about is *false*. It would be an unreasonable demand. The situation here is, once more, just a generalization of the one we are used to in the debates about realism and underdetermination. As long as scientific realists base their recommendation of optimism on the available historical records (or, at any rate, on the performances of actual scientific practices), they cannot be required to prove the impossibility of rival alternatives, unless they were claiming certainty for their realist tenets.[40]

This being the situation, one might formulate the following doubt: if scientific realism is, at bottom, threatened by the multiplicity thesis and if contingentism, as we know, *implies* the multiplicity thesis, that is, if multiplicity thesis is logically weaker than contingentism, it becomes then unclear what the specific contribution of *contingentism* as such to the realism/antirealism debate might be. In other words, one

[40] However, as we shall shortly see, this does not imply that they are allowed not to take into account the problem of the plausibility of existence of alternatives.

might argue, if scientific realism is jeopardized whenever a consistent case is made that there exists a plurality of equally successful accounts of a given subject matter, considerations concerning actual or potential *historical paths* are redundant, for already the actual reality or established possibility of the *stabilized stages* to which they lead count, by themselves, as powerful threats to scientific realism. However, the structure of the debate cannot be portrayed in this way. True, from a logical point of view, contingentism says something *more* than the multiplicity thesis, something specifically *historical*; nevertheless historical reconstructions do have their own peculiar function in the critique of scientific realism, for they can enhance the degree of *plausibility* of successful alternatives developments, and, thereby, the degree of *plausibility* of the multiplicity thesis itself. In this way, contingentist historical reconstructions can at least weaken the position of scientific realists, even of the moderate kind epitomized by structural realists. Going back to Cushing's and Pickering's examples will help us understanding it.

Cushing's analysis, as we have seen, provides probably the most complete example of contingentist account of the history of science, an account that does not only contain a plausible counterfactual history, but also an alternative theoretical development in flash and blood. However, even Cushing's analysis does not really provide a full-fledged alternative development. The reason is that Bohmian quantum mechanics, as it is discussed by Cushing, is a non-relativistic theory, i.e. an empirically equivalent competitor of non-relativistic standard quantum mechanics only. Bohmian mechanics does not account for particle creation and annihilation, as is done, instead, by quantum field theory. Some attempts to develop a Bohemian quantum field theory are under way,[41] however, as of today, there is no consensus on a single quantum field generalization of Bohm's theory. Indeed, at the moment, one would be right in claiming that Bohemian mechanics is, strictly speaking, *less successful* than standard quantum mechanics, 1) it is less progressive, in the sense that the rate at which it produces consensus on new results is far slower, and 2) it only tries to keep up with the advances obtained by mainstream quantum physicists. Yet, this fact, instead of weakening Cushing's analysis, foregrounds the real import of its *historical* dimension. Cushing shows us that Bohmian physics, which is at present a minority view among physicists, could have occupied the center of stage from the very beginning. In that case, not only, as we have already stressed in the section 2, Bohmian mechanics would

[41] See, for instance, Dürr et al 2003 and 2005.

have not *looked* so odd and far-fetched after all, but it would have also provided the shared theoretical background for the vast majority of the community of theoretical physicists, who would have produced a massive amount of theoretical work based on it. Standard quantum mechanics, consequently, could have been a minority view among researchers (or even an unsettling dead-end in the history of physics). Of course, there is no absolute guarantee that a given scientific research program *could have been* successful, and this general rule applies also to a causal program in quantum mechanics, for there is not guarantee that it would have proved *as fertile as* standard quantum mechanics in the extension to field theory. But, does it now really look so difficult to imagine an alternative present in which the balance of success is reversed and Bohmian mechanics both has a wider empirical scope and enjoys a higher degree of progressiveness than standard quantum mechanics?

Let us also note that Cushing's example is particularly dangerous for structural realism too. Structural realists would have to show that Bohmian and standard quantum theories, both in non-relativistic and relativistic form, are compatible at the structural level, and this does not seem to be very simple. For instance, does it really make sense to say that standard quantum mechanics and Bohm's mechanics make *similar* claims about the structures existing among the real entities inhabiting the universe (entities that cannot be equated with the Bohmian particles of course, for this would imply the acceptance of a full-blown kind of realism)?

If one turns to Pickering's brand on contingentism, as we have already noted at the end of the previous paragraph, we certainly find less clearly delineated alternative scientific developments. This is not surprising after all, for alternative developments involving a sharp difference at the level of material and practical resources are unlikely to cohabitate for long periods of time, given the non-pluralistic ideology that has so far dominated the scientific community.[42] An actual example of alternative development of this kind, such as Bohm's theory in the case of underdetermination, is less likely to be available. The raison is that the scientific community can and sometimes does tolerate the existence of deviant theoreticians trying to subvert the dominant views of their research field; but it is very unlikely that it should tolerate the coexistence of two different and conflicting experimental traditions, both of which would require the support of several interconnected communities of technicians and manufacturers, a

---

[42] See Chang contribution to this volume for an analysis of the relation between contingentism and pluralism.

related process of standardization of tools and instruments, and a network of recognized institutions in which experimenters could be trained to use them. There certainly is a strong tendency to preserve the unity of the material infrastructure of scientific research. And this tendency is likely to hide the contingent factors at work in the history of science. Therefore, the historical examples of actual alternatives at the level of laboratory practices are bound to be very local, especially when the most recent episodes of the history of science are taken into account, for contemporary science involves a huge amount of financial, technical and human resources.[43] Nevertheless, we do find in studies such as that of Pickering a specific historical element lending credibility to contingentism, that is the plasticity of so-called empirical basis of science. Indeed analyses such as those of Pickering, in so far as they makes plausible that the so-called "phenomena" can stabilize in a number of different ways, threaten to undercut the very project of any preservative realism, which always presupposes the invariance of the phenomenal basis of science.

In sum, contingentist histories of science pose a challenge to scientific realism (whether wholesale of selective), which, though akin to that of the more familiar arguments based on the doctrine of underdetermination and on the pessimistic metainduction, is logically distinct from them. In the case of underdetermination, the discussion is twofold: whether there is a general argument to the effect that each theory admits undetermined rivals, and whether there actual cases of rival, radically underdetermined theories. In the case of the pessimistic metainduction the debates heavily depend on examples of past successful theories that were subsequently superseded. Thus, in both case the philosophical discussion is fed either by *logical possibilities* or by specific actual historical *facts*. In contrast, contingentist reconstructions of the history of science, by striving to enhance to plausibility of alternative successful developments, occupy a space that is intermediate between sheer logical possibility and historical factuality. In that lies the specificity of their challenge to scientific realism.

[43] See Trizio 2008, p. 258. An example of such a local, small scale alternative accounts at the level of laboratory science Pickering's reconstruction of Giacomo Morpurgo's researches on quarks (Pickering 1989, and 1995, Chapter 3).

5. Conclusion

In this article, I have tried to show that the critique of scientific realism is the driving motive of contingentist reconstructions of the history of science. This fact has called for an analysis of the relations of logical compatibility between the couples inevitabilism/contingentism and realism/antirealism, an analysis taking into account also structural realism as a representative of preservative (or selective) variants of scientific realism. It has appeared that contingentism and scientific realism, when referred to a specific component of scientific knowledge, are incompatible. On the other hand, inevitabilism could in principle coexist both with realism and antirealism, even though the latter theoretical configuration appears difficult to substantiate and defend. Further, I have suggested that contingentist histories pose a *sui generis* challenge to scientific realism, consisting in enhancing the degree of plausibility of alternative scientific developments. The alternative scenarios presented in the examples of contingentist history of science that I have taken into account, threaten also the "continuistic" strategy of structural realism.

One should not forget, however, that historical reconstructions are by definition local in character and can provide no general argument for a claim such as contingentism. [44] It is not impossible to imagine that only some scientific disciplines or only some aspects of some scientific discipline are contingent, while others are inevitable.[45] Philosophers of science working in the realism/antirealism debate are familiar with this situation. If we focus on the way in which the confrontation between contingentist antirealism on the one hand and preservative realism on the other has developed so far, we can observe a clash of empirical inferences resting on evidence mainly deriving from the history of science: on the one hand scientific realists, from the standpoint of their meta-approach, posit structures, entities, properties, (or whatever is the case depending the specific type of scientific realism) on the grounds of their enduring role throughout the historical succession of successful theories, and, on the other, contingentist antirealists, from their own meta-approach, posit possible

[44] But see Soler in this volume for an attempt to develop a general contingentist argument.

[45] See Allamel-Raffin and Gangloff in this volume pp.?? For instance, it does seem easier to concede the contingency of particle physics than that of, say, cellular biology, for it is really hard to imagine how our science could have been as successful as our without the notion of cell.

alternative successful sciences.[46] On the one hand we find hypotheses about the natural world, and, on the other, hypotheses about possible sciences and possible social arrangements supporting them. Realists view the history of science as a smooth and uniform land, on which an external force has left readable and persistent signs that we can decipher and tell apart from our own prints; contingentists contemplate a varied landscape, rich in brakes and discontinuity, and disseminated by signs of unfulfilled possibilities of human intellectual and practical life. On both sides we find theoretical constructs whose legitimacy does not imply any mutual contradiction: even the demonstrated possibility of an entire maze of alternative successful incompatible sciences is not logically incompatible with the *existence* of the entities, structures or properties realists believe in. The world may be *one way* at the level of its deep constitution, as metaphysical realism claims, while still supporting many conflicting scientific accounts of it. The conflict between these two types of hypotheses is not at the ontological level but at the epistemic one.[47] The more one believes in the possibility of alternative successful developments the less one feels entitled to believe in the reality of a given component of our successful theories.

This methodological characterization of the debate is not intended to denounce its inconclusiveness, but it does indicate that, until a general argument is at hand, the controversy that I have reconstructed in this article will probably evolve on the basis of case studies supporting more or less *local* claims of antirealist contingentism or scientific realism.

[46] See, for instance, Pickering 2012 p. 323: "Within the culture of 1950s particle physics the world revealed itself to us in the shape of the bubble chamber. In the culture of the 1960s, it revealed itself to Morpurgo as having no free quarks. But I find it easy to imagine that different cultures could have elicited quite different machines and instruments and material performances from the world; and I can see no reason not to imagine that."

[47] In other word, the conflict concerns scientific realism, not metaphysical realism.

References

Change, Hasok (2003), ‘Preservative Realism and Its Discontent: Revisiting Caloric’ *Philosophy of Science*, 70, pp. 902-912.

Cushing, James T. (1994), *Quantum Mechanics. Historical Contingency and the Copenhagen Hegemony* (Chicago, London: University of Chicago Press).

Dürr, Detlef et al (2004), ‘Bohmian Mechanics and Quantum Field Theory’ *Phys. Rev. Lett.*, 93: 1–4.

Dürr, Detlef et al (2005), ‘Bell-Type Quantum Field Theories’ *J. Phys. A: Math. Gen.*, 38: R1-R43.

Hacking, Ian (1992), ‘The Self-Vindication of the Laboratory Sciences’, in A. Pickering, ed., *Science as practice and culture*. Chicago: The University of Chicago Press, 29-64.

Hacking, Ian (1999), *The social construction of what?* Cambridge, MA: Harvard University Press.

Hacking, Ian (2000), ‘How inevitable are the results of successful science?’ Philosophy of Science, 67(Proceedings), 58–71.

Kuhn, Thomas (2000), *The Road Since Structure.* Chicago: University of Chicago Press.

Kukla, André (2000), *Social constructivism and the philosophy of science*. New York: Routledge.

Laudan, Larry (1981), 'A Confutation of Convergent Realism', *Philosophy of Science,* 48 (1), 19-49.

Pickering, Andrew (1984), *Constructing Quarks. A Sociological History of Particle Physics* (Chicago, London: University of Chicago Press).

Pickering, Andrew (1989), ‘Living in the material world: on realism and experimental practice’ in *The Uses of Experiment*, eds by David Gooding et al. (Cambridge: Cambridge University Press), pp. 275-297.

Pickering Andrew (1995), *The Mangle of Practice: Time, Agency, and Science*. Chicago: University of Chicago Press.

Pickering, Andrew (2012), ‘The robustness of science and the dance of agency’, in *Characterizing the Robustness of Science: after the Practice Turn of Philosophy in Science*, eds. Léna Soler et al. (Boston : Springer), pp. 317-327.

Psillos, Stathis (1999), *Scientific Realism. How Science Tracks Truth* (London, New York: Routledge).

Psillos, Stathis (2000),‘The Present State of the Scientific Realism Debate’ *The British Journal for the Philosophy of Science* 51: 705-28—reprinted in Clark, P. & Hawley, K. (eds) (2003) *Philosophy of Science Today*. Oxford: Clarendon Press.

Sankey, Howard (2008), ‘Scientific realism and the inevitability of science’, *Studies In History and Philosophy of Science,* 39 (2), 259-64.

Soler (2004) ‘The Incommensurability Problem : Evolution, Current Approaches and Recent Issues’ *Philosophia Scientiae*, 8(1), 2004, 1-38.

Soler, Léna (2006), ‘Contingence ou inévitabilité des résultats de notre science ?’, *Philosophiques*, 33(2), 363-378.

Soler, Léna (2008a), 'Are the results of our science contingent or inevitable?', *Studies In History and Philosophy of Science,* 39 (2), 221-29.

Soler, Léna (2008b), 'Revealing the analytical structure and some intrinsic major difficulties of the contingentist/inevitabilist issue', *Studies In History and Philosophy of Science,* 39 (2), 230-41.

Soler, Léna (2008c) 'The Incommensurability of Experimental Practices : the Incommensurability *of what* ? An Incommensurability *of the third-type*?', In *Rethinking Scientific Change and Theory Comparison. Stabilities, Ruptures, Incommensurabilities ?*, L. Soler, H. Sankey et P. Hoyningen (éds.), Springer, Boston Studies for Philosophy of Science, 299-340.

Stanford, P. Kyle (2006), *Exceeding our Grasp. Science, History and the Problem of Unconceived Alternatives* (Oxford: Oxford University Press).

Trizio, Emiliano (2004), 'Incommensurability and laboratory science', *Philosophia Scientiae*, 8(1), 235-267.

Trizio, Emiliano (2008), 'How many sciences for one world? Contingency and the success of science', *Studies In History and Philosophy of Science,* 39 (2), 253-58.

Worrall, John (1989), 'Structural Realism: The Best of Both Worlds?', *Dialectica,* 43 (1-2), 99–124.