Conceiving the Inconceivable: An Assessment of Stanford's New Induction

Giovanni Buonocore, Emilia Margoni,** Francesca Pero**

** University of Florence*

*** University of Pisa, University of Florence, Université de Genève*

Abstract

Stanford's unconceived alternative argument is inductively based on the history of science and tells us that when a scientist is choosing a theory T_1 at time t_1 over a set of less promising alternatives, she is concurrently failing to conceive valid theoretical alternatives to T_1, i.e., theories that will be accepted by a scientific community at later times, thus displacing T_1. The aim of the present paper is to argue that the actual strength and reach of Stanford's argument sensibly vary according to the status of the unconceived alternatives at time t_1, i.e, whether they are *conceivable* (theories that could be conceived by scientists at t_1, but in fact are not) or *inconceivable* (theories which can not be conceived at t_1 as they are incompatible with scientists' background knowledge at t_1). As Stanford does not explicitly address this issue, we give reasons to conclude that alternatives considered in the unconceived alternative argument are supposedly conceivable at time t_1, and we investigate the consequences of this conclusion for the alleged novel induction the argument draws upon. We then investigate what are the implications for Stanford's analysis if inconceivability is considered as a possible status of an unconceived alternative at t_1, and we argue that, in this case, Stanford's antirealism has to be severily restricted to specific phases of theory-change, thus making room for tamed forms of realism.

Keywords: Instrumentalism, Pessimistic induction, Scientific realism, Unconceived alternative argument.

1. Introduction

Realist and antirealist stances have been developed into such articulated proposals that defining the key features of both positions risks ending in deadlock. Broadly speaking, scientific realism is taken as a "positive epistemic attitude" (Chakravartty 2017) towards the content of well-established scientific theories and models, whereas antirealism either questions the even approximate truthfulness of currently available scientific paradigms or declares to be agnostic about it.

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Stanford's (2006) proposal sets forth novel arrows in the quiver of the antirealist party. To this end, he develops the unconceived alternatives (UA) argument that supposedly provides a new form of induction (NI), directed towards theorists rather than theories, as opposed to the old pessimistic induction (PI). The core of the UA argument is the claim that scientists have repeatedly failed to conceive of reasonable alternatives to time by time well-established scientific theories. On this view, the acceptance of a later theory provides the retrospective evidence of the inability to conceive of at least one alternative at the time the earlier theory was conceived, and this is what shall be inductively projected to current successful theories. Coupling this historical consideration with the assumption that scientific practice typically operates via eliminative inference methodology, it looks as if scientists are not able to exhaust the space of alternative theoretical explanations for a given set of phenomena.

According to Stanford, the problem of UA is different from the one posed by classical underdetermination from empirical equivalents insofar as it refers to the very epistemic, cognitive limits of those human agents—the scientists—who are in charge of delving into the maze of plausible theoretical candidates for a given set of phenomena (Stanford 2006: 16-17). Put it otherwise, Stanford's proposal to shift the focus from theories to theorists is what, in his intention, should strengthen the antirealist argument. It is very unlikely, he reasons, that current scientists have succeeded in what their predecessors have failed to, namely, to exhaust the space of plausible alternative theories for a given set of phenomena. On this view, for all available evidence at every given moment and in every sociocultural context there are always unconceived alternatives. And these unconceived alternatives are what, according to Stanford, seriously undermine scientific realism broadly construed.

Stanford's (2006) UA argument has been discussed and criticized by various authors. Based on diverse argumentative lines, they have pinpointed a few weaknesses that might affect his theoretical framework. On the one hand, Saatsi (2009, 2019) argues that Stanford's new induction does not look that novel at all, in that the problem of UA is still based on the well-known traditional weaponry of antirealists, namely, the underdetermination problem and the pessimistic induction. On the other hand, both Winther (2009) and Rowbottom (2019) claim that Stanford's proposal is too focused on theories and should rather extend the analysis to other aspects of scientific practice, to offer a more appropriate characterization of the latter.

In this paper we argue that Stanford's proposal does not take into consideration a crucial distinction, i.e., the one between conceivable and inconceivable alternatives. Conceivable alternatives are those theories which could have been conceived by scientists involved in a certain field at a specific time but in fact were not, despite these alternatives' compatibility with the evidence available at that time and the context's background assumptions. Conversely, inconceivable alternatives are those theories whose conceivability is prevented by empirical, methodological, and theoretical limitations.[1] Of course, being theories at stake (and

[1] It may be argued that the notion proposed here of inconceivable alternatives conflicts with the fact that there are historical cases in which a certain theory is developed despite being formally inconceivable. However, our argument is not to deny that scientists can conceive of alternative theories even in the absence of theoretical, methodological, and empirical elements—a paradigmatic example being the formulation of the heliocentric

assuming we are not extending platonism to theories themselves and treat them as abstract, non-mental, objects), we are considering conceivability and inconceivability as relative concepts, i.e, predicated in relation to an individual or an epistemic community who is capable (or incapable) of formulating the theory.

On the backdrop of this distinction, we argue that, whether an unconceived alternative at t_1 is conceivable or inconceivable, Stanford's argument and its antirealist claim are affected. We reach this conclusion by first arguing that the status of an unconceived alternative at time t_1 that is consistent with Stanford's UA argument is conceivability, mainly for two reasons. First, were UA inconceivable at t_1 it would be hard to maintain the very core of Stanford's argument: how could we talk of a failure in conceiving something if it was, in fact, inconceivable? In other words, as we are going to argue in the following section, for Stanford's argument to even kick off and be a real threat to scientific realism (and a compelling form of the underdetermination argument against it) conceivability, as a feature of unconceived alternatives, should be as relevant as the other explicitly considered by Stanford.

Second, we think that the very requirements Stanford sets up for an unconceived alternative strongly hint at conceivability as its status at t_1. These requirements mainly amount to being empirically non-equivalent yet equally well confirmed by evidence as the 'dominant' theory and, more importantly, to be compatible with the same constraints and general metaphysical principles that guided the development and acceptance of the 'dominant' theory. According to our reading, Stanford presents unconceived alternatives as *consistent with* (which we read as *conceivable according to*) the evidence and the scientific environment (methodological and metaphysical) at stake. Therefore, according to Stanford's analysis and the conceivability of UA that, we argue, it calls upon, the only elements preventing the conception of UA are the scientists' epistemic limits. We agree with Stanford that scientists, as epistemic agents, come equipped with a remarkable yet limited capacity to explore the complex targets of scientific research. However, such limitations are mainly imposed by the background knowledge collected up to the time scientists live in. Consequently, in such a conceptually bounded context, many of the alternatives later accepted by a different or following scientific community could not even be imagined and formulated, let alone conceived. In other words, Stanford portrays as a limit, namely as an epistemic failure, what is not even attainable in principle, i.e., conceiving something that we will probably not be even able to adopt for explanatory purposes, as it clashes with the way we conceptualize physical reality. We attempt to make this point by considering a case study where a formerly unconceived alternative was first and foremost inconceivable (Sect. 3).

We then turn to investigate what kind of notion of conceivability would be consistent with Stanford's account and why such a notion would undermine the core of the NI, being it only allegedly a property relative to theorists' capabilities, and actually pertaining to theories. If this is the case, the focus of the meta-inductive claim would be on theories rather than on theorists, just as in the old PI (Sect. 4). We finally investigate what are the implications for Stanford's accounts and

theory by Aristarchus of Samos despite Aristotelian mechanics and the lack of empirical data. What we mean to question is that one can construct a novel antirealist argument based on Stanford's shift from theories to theorists. And this, we believe, gets signaled by focusing on the distinction between conceivable and inconceivable alternatives.

the reach of its antirealist stance if inconceivability is considered as a possible status of an unconceived alternative at t_1 and we argue that, in this case, Stanford's antirealism has to be restricted to those specific phases of theory-change in which a certain theory remains unconceived, despite being conceivable (Sect. 5).

2. Conceivable *vs* Inconceivable Theories

In considering the spectrum of theoretical alternatives to a given set of phenomena, we would like to introduce a taxonomy that will pave the way for the present discussion. Our claim is that the distinction between conceivable and inconceivable alternatives is not only valuable, but also necessary if one aims to take at face value the import of Stanford's proposal. Our theoretical framework envisages the following taxonomy:

(1) inconceivable – unconceived
(2) conceivable – unconceived
(3) conceivable – conceived

This paper will focus on the problematic relationship between options (1) and (2). Our claim is that the difference between (1) and (2) is crucial when it comes to the problem of UA. Missing this distinction, our argument runs, risks rendering Stanford's argument either trivial or incomplete. For it is one thing not conceiving alternative theories that we could have conceived—being them conceivable—but did not for some (putative) socio-cultural limitations, quite another not conceiving those alternative theories which are inconceivable because of empirical, methodological, and theoretical limitations.

Stanford's historical reconstruction draws a picture in which theories which are newly developed repeatedly turned out to be the unrecognized, unconceived alternative to an antecedent well established one, with the intent to inductively generalize such pattern to possibly every case of theory-change. This inductive generalization is what determines his antirealist stance towards scientific practice broadly construed, rather than confining his analysis to specifically selected theories or specifically selected theory-change contexts.

However, Stanford's analysis of historical records makes no reference to the concept of inconceivability and seems to only account for alternatives of type (2)—namely, conceivable theories that remained unconceived because of a contingent failure within the scientific community to conceive them. According to Stanford, such a failure is due solely to the fact that "our cognitive constitutions or faculties are not well suited to exhausting the kinds of spaces of serious candidate theoretical explanations" (45). In fact, Stanford devotes part of his book to the analysis of cases from the history of science where there is an alleged continuity among evidential, metaphysical, and methodological constraints, such as in the case of the transition first from Darwin's pangenesis theory of inheritance to Weismann's germ-plasm theory, then to the Mendelian theory and, finally, to contemporary molecular genetics.[2] Such a continuity would rule out any potential incompatibility of background knowledge and assumptions among scientific communities as a possible reason for scientists' failure to conceiving "scientifically serious alternatives" to a

[2] It is important to note that what Stanford (2006) means by "metaphysical limitation" remains unspecified in the context of theory generation. In the following section we thus adopt the more adjustable notion of "theoretical limitation".

theory T_1. Such a label is particularly relevant to Stanford's analysis as it marks the difference between the unconceived alternatives and those alternatives invoked by the traditional form of underdetermination. In fact, the alternatives considered by the UA argument are not "construct[ed] parasitically so as to perfectly mimic the predictive and explanatory achievements of our own theories" (18-19), but are genuine theoretical alternatives that simply remain unconceived up to a certain time and eventually "accepted by some actual scientific community" (21). More importantly, these alternatives were "scientifically serious even by the standards of the day despite being unconceived and therefore unconsidered by theorists at the time" (60). This is exactly what should make the UA argument a bigger threat for scientific realism than the traditional underdetermination argument: being the alternatives it considers scientifically serious, UA cannot be reduced and dismissed by a realist as a philosophical speculation valid only from the logical viewpoint, or in-principle.

Summing up, the main claims of Stanford's UA argument are the following:

(a) At a time t_1, a theory T_1 is conceived and preferred over a set of other conceived but not equally well-confirmed theories.

(b) At a later time t_2, an empirically non-equivalent, but equally well confirmed, alternative T_2 is conceived and preferred over T_1.

(c) At the time t_1, the theory T_2 was conceivable (as equally well confirmed) despite remaining unconceived until t_2.

Now, let us make the conceivability condition explicit:

(L1) T_2 needs to be at least equally well supported by the evidence that supported T_1, at t_1, and compatible with T_1's background assumptions (or constraints), whether they be theoretical, empirical, or methodological.[3]

The question that naturally arises is: does the UA argument require new theories to be conceivable at the time in which old ones were conceived and accepted? If this were the case the choice between T_1 and T_2 would be underdetermined at t_1, precisely when the conceivability condition of T_2 needs to be met. As we shall see in the following section, the implications of such a framework are quite radical and implausible when generalized and applied to other (well-known) cases of theory-change, for which the conceivability condition clearly does not hold. In fact, what happens between t_1 and t_2 matters. As pointed out in Magnus' critical assessment of the UA argument (2006), a theory T_2 is conceived within a period of revolution to try to account for some evidential anomalies the theory T_1 struggles with. During the period of controversy, T_1-supporters try to account for such anomalies within T_1 itself, while others formulate the new theory T_2. At this moment (and only at this moment) there really is a problem of underdetermination between T_1 and T_2, but as further, decisive, evidence is gathered, the problem might eventually vanish, thus defining a preference between T_1 and T_2.

Let us now turn our attention to a paradigmatic case in which this aspect is brought up to the forefront.

[3] The issue of background assumptions and their role in theory choice is a topic which exceeds by far the limited scope of the present paper. We just like to note in passing that the ambiguities associated to this topic have been tackled by eminent scientists (Einstein 1936), philosophers of science (Reichenbach 1958, Kuhn 1970) and have a proper status as an issue in social epistemology (Longino 2002, Nelson 1993, Potter 1996).

3. From Newtonian Mechanics to Relativistic Physics: A Case Study

The UA framework, while successfully applicable to the instances of scientific theorizing from biological sciences Stanford considers, is not as successful when, e.g, applied to the history of physics. This is rather evident when considering the paradigmatic passage from Newtonian theory of space and time to Einstein's theory of special relativity (STR). As Stanford states, "the evidence available at the time the earlier theory was accepted offered equally strong support to the (then-unimagined) later alternative" (19). As shown at the end of the previous section, the UA argument implicitly requires the conceivability condition of the later theory to be met already at the time the earlier was conceived and accepted: at the time of Newton there were no empirical, theoretical, or methodological constraints that could prevent the scientific community to conceive of STR, but only cognitive-epistemic limitations. STR was conceivable and, yet, remained unconceived until 1905.

Drawing on the works by DiSalle (1990), Norton (2004) and Cassini and Levinas (2019), in the following we trace back those theoretical, empirical, and methodological constraints for STR's conceivability that, contrary to Stanford's point, were in fact inaccessible at the time of Newton and that turned out to be essential to Einstein's fundamental intuition about the relativization of the notion of simultaneity.

As for the theoretical constraints, conceiving an equivalence-class as the fundamental spatiotemporal framework required a level of abstraction attainable only with the mathematics of the 19th century. Thomson's (1884) reassessment of the laws of inertia highlighted the fundamental relation between Newton's laws of motion and inertial frames, namely, the existence of (at least) one inertial frame, with respect to which any other is in uniform motion. The point was that any inertial frame could be constructed as the "absolute" space in which all the others are uniformly moving, and, therefore, the crucial issue was no longer to identify the frame of reference in which the dynamical laws hold, but, rather, how the laws of motion are able to define an appropriate class of reference frames (DiSalle 2020: 23). Lange (1885), independently of Thomson's work, introduced a new definition of inertial system based on the intuition that all motion is relative: an inertial system is a coordinate system with respect to which three free particles move in straight lines and travel mutually proportional distances as they are projected from a single point and are moving in non-coplanar directions (DiSalle 1990). According to the laws of inertia, any fourth free particle will move uniformly with respect to any inertial system; thus, Newton's notion of absolute acceleration (and rotation) can be replaced by that of acceleration (and rotation), relative to an inertial system (and timescale). Although Lange's and Thomson's direct influence on Einstein, as well as their broader historical impact, is difficult to assess (DiSalle 1990: 140), by the beginning of 1900 the notion of inertial system had permeated the debate around mechanical philosophy and was assumed as the foundation for classical mechanics. In fact, Einstein (1905) took it for granted that his readers consider an equivalence-class of frames of reference rather than a privileged frame (see DiSalle 2020).

Turning to the empirical constraints necessary for STR to be conceived, the historical record of Einstein's oral presentations shows some explicit references to the relevance of Fizeau's results (see Shankland 1963: 48), although not stated in published or unpublished works (see Norton 2004).[4] Fizeau tried to measure the relative speed of light in water, using a particular interference system that measured the effect of the moving medium on the speed of light itself, by observing interference fringes produced by two rays of light passing through two parallel pipes filled with water flowing in opposite directions.[5] Fizeau considered three hypotheses, only one of which to be confirmed by his experiment: (1) the ether has no interaction with the moving medium, (2) it is partially dragged by the moving medium (Fresnel's hypothesis), (3) it is fully dragged. He erroneously considered his observations of small fringes displacement to confirm (2), by assuming a portion of the ether was fixed to the water molecules, but Fizeau never considered that the effect could have been explained without any reference to matter-ether interaction (Patton 2011: 215). And, in fact, Lorenz (1895) considered this fourth hypothesis, and proved it to be the right one: the effects obtained by Fizeau, despite being compatible with (2), were determined solely by the reflection and refraction of light waves, rather than matter-ether interaction. This fact alone, however, did not prompt the Dutch scientist to abandon 'still ether' as a reference frame. It was only with the successive reinterpretation of Fizeau's experiment under the new conceptual framework of the equality of all inertial systems that its results turned out to be crucial for STR's conceivability.

Finally, as also pointed out by Norton (2004), Einstein's methodological debts to the writings of Hume and Mach are evident when it comes to his account of the nature of concepts in general rather than the specific analysis of space and time carried out by the two authors. Einstein himself pointed out that his intuition came from a reconsideration of certain types of concepts that physical theories include, which, in order for them to represent something physical, must be grounded in experience (Einstein [1917] 1954: § 8). Einstein (1916) makes explicit reference to the valuable method of conceiving concepts as physically meaningful only in so far as they are empirically grounded. But in Mach's writings specifically (see, e.g., Mach [1907] 1960), it also emerges a radical attitude towards fictional concepts that leads to their complete elimination from any relevant account of the physical world to which Einstein was reluctant (Holton 1968: 231). Hume's analysis ([1748] 1988) is also based on certain notions ("ideas") that must be grounded in sense experience ("impressions"), in line with Mach's empiricism. But on the other hand, Hume did not propose to completely eradicate such notions that were not empirically grounded, as in the case of causality. And, indeed, the reconceptualization of a fictional concept whose uncertain character is recognized but accommodated within the physical theory in such a way to "preclude unwitting introduction of false presumptions" (Norton 2004: 3) is precisely the theoretical step that Einstein took towards the relativization of the notion of simultaneity. It is perhaps for this reason that Einstein firsthand declared Hume's work having "much more influence" than Mach in the formulation of STR (Einstein 1949, as quoted in Norton 2004: 2).

4. What if Unconceived Alternatives Are Conceivable

The old induction statement is confined to theories and, in particular, it casts doubts on the truth of theoretical claims. Differently, NI redirects the pessimism

[4] For additional references on the influence of Fizeau's results see Einstein 1923 and Moszkowski 1972.

[5] For a detailed presentation of the experiment see Patton 2011 and Cassini and Levinas 2019.

of PI from theories to theorists as cognitive agents, asserting the impossibility for theorists to ever exhaust the space of possible alternatives to the theory accepted at a certain moment. According to Stanford, this sort of pessimism is difficult not to subscribe to, thus making NI a bigger draw for an antirealist than the old PI. In fact, Stanford claims, we have collected throughout history of science enough evidence to inductively rule out the possibility that future scientific communities will epistemically improve to the point they will not fail to exhaust the alternatives' space.

Stanford's analysis is convincing as long as the only possible scenario that leads to what he defines as a "scientifically serious alternative" (2006: 20), i.e., a theory later accepted by a scientific community, is the one depicted in condition (2) (Sect. 2): alternatives remained unconceived are formerly conceivable ones. We question whether we can talk of an epistemic failure in not conceiving a theory which then turned out to be a serious scientific alternative if the latter was inconceivable at the time another one was accepted. A way to approach such an issue is by conditionally investigating why Stanford would neglect the crucial difference between conditions (1) and (2) above. In fact, such an omission seems to contravene the interest Stanford proclaims for the "empirical exploration of the various dynamical processes that help explain how and why particular unconceived alternatives remain unconceived by particular (human!) scientists and scientific communities" (2009: 381).

One reason why Stanford could reject the distinction is built into the transient nature of the underdetermination as intended by NI, upon which the conceivability notion depends.[6] Recall that recurrent transient underdetermination requires unconceived alternatives to be equally (roughly, at least) well confirmed by the available evidence although empirically non-equivalent to their rival, and to be so up to a certain historical development when enough evidence has been collected so that the rival theory is differently confirmed. Such a requirement, Stanford claims, "deflects any suggestion that such alternatives were ignored on evidential grounds rather than simply unconceived" (2006: 26). Deflecting this sort of suggestion is crucial since it is a threat to the notion of conceivability: if the alternative unconceived theory is unable to make evidence intelligible, then the alternative is inconceivable.

The scientific context, together with its methodological, theoretical, and metaphysical assumptions, informs the way a scientific community of a certain time classifies phenomena and, consequently, collects evidence to test a hypothesis. This state of affairs negatively affects the idea of conceivability Stanford promotes as an atemporal quality of a theory.[7] In fact, inconceivability could be denied as a former status of an unconceived alternative by claiming that the very existence of evidence equally supporting the unconceived alternative suffices to make it

[6] Another reason why one could neglect the case unconceived alternatives were formerly inconceivable is because such occurrence is considered as impossible. We assume this would be too bold of a claim to subscribe.

[7] Magnus (2010) takes "being a scientifically serious alternative" to be treated by Stanford as a "timeless property of a theory" (7). We think that ascribing timelessness to the property of "being conceivable" is more consistent with Stanford's proposal. This is because "being a scientifically serious alternative" depends upon a subsequent and ultimate act of a scientific community which takes place at a specific moment in time while conceivability is something that, according to Stanford, could be predicated of a serious scientific alternative prior to its acceptance by a scientific community.

conceivable, independently of the theorist's epistemic ability to use that theory to read such evidence. In other words, as long as a theory is equally well supported by evidence as its rival(s), such theory has the ability to be conceived, regardless of whether the contemporary scientific community has the ability to conceive it. Consequently, conceivability has little to do with scientists' epistemic possibilities in that it is rather a way to sort out the possible interaction of theories alone with evidence as it is given by itself, and not accordingly to a scientific frame of reference. Evidence could be read in the light of the unconceived alternative, thus making it conceivable. However, the consistency of the later-accepted theory with respect to the evidence available at the time the later-overturned theory was dominant can be identified only retrospectively. Beforehand, it might be the case that, to the eyes of the scientific community of the time, any reading of the evidence according to standards incompatible with their epistemic, metaphysical or methodological background assumption was, in fact, impossible.

Neglecting that unconceived alternatives can be inconceivable is legitimate only if we narrow the analysis, and the inductive generalization we want to make with it, to theories as final byproducts of theorizing, as well as to their relations to evidence and to preceding accepted theories. The fact that scientists and scientific communities consistently fall short of conceiving scientifically serious alternatives at a certain time does not affect the conceivability of those theories at that time (being them conceivable regardless of humans' ability to conceive them) In fact, conceivability is cast in Stanford's analysis mainly as a property of theories, due to their relation to evidence rather than to a theorist's epistemic capacity. Accordingly, the predicament NI is about does not concern the disadvantaged epistemic position we are doomed to occupy across the history of science, that is, the position where serious scientific alternatives remain unconceived by us. Rather, the predicament is just the same that serves as the empirical premise for the old PI: the recurrent turnover of older theories in favour of new ones. Adding conceivability to the picture grants no novelty to the inductive argument, as the way Stanford casts this notion is not informative about the processes that lead unconceived alternatives to remain unconceived by certain scientific communities. Evidence could be read in the light of the unconceived alternative, thus making it conceivable.

So far, the novelty of NI has been questioned by looking at its inductive basis rather than at the concepts it is built upon. In fact, Magnus (2006) and Saatsi (2009) worry that NI either fails as induction or it is old-fashioned after all. They question Stanford's account of what might happen at time t_1 among rival theories (including unconceived ones), theorists and available evidence on the grounds of counterfactual claims, based on the concept of plausibility. Given that a scientific community comes with some standards of plausibility, which in turn fix some criteria for the definition of what counts as experience, it can be the case that a scientifically serious alternative, were it conceived and presented by any member of the community, would not have seemed to be plausible to the rest of it. Considering the stand-off between classical mechanics and relativity, the question arose whether the latter would have been considered as plausible had it been presented to the scientific community subscribing the former. More precisely, could the data available and, most of all, the way they were collected and interpreted by Newtonians, license any plausibility claim about the alternative reading provided by the theory of special relativity? Given that standards of plausibility change according to the scientific context at stake and crucially determine what is classifiable as an experience, Saatsi and Magnus opt for a negative answer to that question.

Stanford (2009, 2017) replies to Magnus and Saatsi that the change of standards of plausibility across history of science does not suffice to conclude that genuine alternatives never existed and ever won't and that, consequently, NI is undermined. For this to be the case, it should also be assumed that implausibility actually prevented Newtonians from conceiving special relativity and also that future scientific communities will not undergo the same changes of scientific background assumptions, thus eventually discrediting previously entrenched judgments of implausibility. Therefore, *mutatis mutandis*, according to Stanford (2009) implausibility shows that we cannot rely on our own standards of plausibility and, consequently, we are doomed to occupy the same epistemic predicament of earlier scientific communities.

There is a crucial conceptual difference between the concepts of implausibility and inconceivability. Implausibility assumes as very unlikely the case where a theory which later turned out to be a serious scientific alternative was first conceived by someone in the scientific community and then immediately withdrawn as implausible. However, implausibility does not rule out such a case as impossible. On the other hand, inconceivability does not allow for such a circumstance to take place: the ephemeral conception of an alternative theory that has to wait many more years to be accepted by a scientific community is not an option, as long as what is required to conceive it is incompatible with the background assumptions held at that time. Ruling out the possibility for such a scenario, inconceivability does not lay itself open to Stanford's reply that time-dependence of plausibility judgements proves that science is at any time unreliable and that dismissed possibilities were actually preferable: they were not conceivable in the first place!

5. Accepting the Distinction between Conceivable and Inconceivable Alternatives: Possible Consequences for the Realist *vs* Antirealist Debate

Newton and his contemporaries did not simply fail to conceive of an alternative theory to Newtonian mechanics such as the theory of special relativity: in that context, the latter was in fact inconceivable. That being so, what is at stake is whether the inconceivability of a certain theory provides elements in favour of the antirealist perspective.

Now, to evaluate this point, let us unpack what the inconceivability of a theory stands for. As already discussed in the previous sections, if a theory is inconceivable because of empirical, methodological, and theoretical reasons, this means that, at the time the inconceivability is met, there is a lack of empirical data, or the absence of a suitable methodological apparatus, or the unavailability of an appropriate theoretical formalism.[8] Either way, two considerations can be made.

First, it is not simply the case that the community of scientists fail to conceive of an alternative theory for a given set of phenomena. Rather, the missing background knowledge is what prevents the attempts in the first place. Again, it looks

[8] Let us for now grant the (highly implausible) thesis according to which there simply are empirical data. We will come back to this point later on in this section.

as if the most we can concede to Stanford is that we are back to the old pessimistic induction situation. In other words, if a theory is unconceived because it is inconceivable, then Stanford's NI does not look that novel at all.

Second, there are different versions of realism and antirealism: a contemporary realist would hardly claim that we should accord currently successful theories a state of complete truthfulness. For in its broadest characterization, realism is simply taken as a positive attitude toward the content of well-established scientific theories and models (Chakravartty 2017). Obviously, then, those scientific theories and models may not accommodate lacking data, methodologies, and formalisms. If this is the case, then the problem is not that in each historical context scientists fail to exhaust the space of plausible alternatives to a given set of phenomena. Rather, the fact is that the set of phenomena (plus the associated methodological and theoretical toolkit) is insufficient to make a certain theory conceivable. But then a perhaps mild realist does have some elements to resist the antirealist claim of Stanford's UA argument. Indeed, she might claim that, though we currently lack those empirical, methodological, and theoretical elements that are necessary to the formulation of a currently inconceivable theory, there are good reasons to believe—*contra* Stanford's historical record—that when the conceivability condition is met, the theory gets eventually formulated. To conclude, if we confine our analysis to the first case, namely to a theory which is unconceived and inconceivable, it looks as if we either get back to the old pessimistic induction situation or that the realist party can cope with the inconceivability condition by smoothing her own commitments, thus conveying a form of "tamed" realism.

Let us now turn our attention to the subtler case in which a certain theory is conceivable, yet unconceived. To stick to the example of Sect. 3, this condition applies to that period, between the end of the 19th century and the beginning of the 20th century, in which both the Fizeau and the Michelson-Morley experiments were being discussed and Lorentz provided a mathematical formalism and a theoretical hypothesis for that experiment. Again, the point at stake is to evaluate the consequences of a theory being conceivable, yet unconceived upon the realist *vs* antirealist debate. This case looks more hospitable to Stanford's proposal, in that one cannot advocate the inconceivability condition whereby the mild realist is able to construct her counterargument.

We acknowledge that Stanford's proposal does apply to those historical cases in which a certain theory is conceivable-yet unconceived, with the following provisos. First, if one confines Stanford's proposal to such cases, it looks as if his overall enterprise has to seriously narrow its scope. Indeed, the main claim of the present paper is precisely to argue that Stanford's lesson cannot be generalized to every theory-change. Second, provided that Stanford's argument applies to conceivable, yet unconceived alternative theories, one of the most challenging points becomes how to identify them (on a similar vein, see Ruhmkorff 2019: 3937-38).[9] Finally, there is a third problem which, according to us, affects Stanford's proposal and that, in a way, might be used against us, for it implies problematizing even the conceivability *vs* inconceivability condition.

[9] Importantly, the selection of theories exposed to the problem of UA—namely, those who are unconceived yet conceivable—can only be reconstructed retrospectively. This is why, even if the scope of Stanford's proposal gets narrowed, it is still unclear to which case-studies it should actually be applied.

The point at stake is: How can we say that a certain theory is conceivable, yet unconceived? Evidently, to do so, we are hypothesizing that a certain set of empirical, methodological, and theoretical components can be mapped in a somewhat traceable way from one theory to another. It is no coincidence, then, that Stanford (2006: 22) declares his argument to be incompatible with the Kuhnian notion of incommensurability. However, there is no clear justification for such a statement. Better, as already noted by Winther (2009), on Stanford's account a whole perspective is lacking, to such an extent that data are given in an utterly unproblematic way. On this reading, if one aims at engaging with the realist *vs* antirealist debate, one should primarily investigate the bottom-up mechanisms whereby entities get reified in the scientific practice.[10]

The example of the transition from Newtonian to relativistic physics hardly serves as a case study supporting Stanford's view. And, more importantly, one needs to be very cautious in regarding data as preconceptual elements that can be selectively rearranged in various theoretical contexts. In fact, data must be interpreted as the by-product of scientific pragmatic agency, so much so that it does not make sense to evaluate them without reference to all the background assumptions, which in turn reify, i.e., produce those same data. In conclusion, the available evidence Stanford invokes to construct his argument does not look amenable to a straightforward mapping between different theoretical backgrounds. For if one is interested in detailing the scientific practice, one has to seriously engage with that myriad of factors (such as experiments, models, techniques, observations, tools, expectations, predictions) that not only figure in a certain scientific context but help shape it in the first place.

6. Final Remarks

Stanford's analysis is surprisingly elusive about the concept of inconceivability, despite its pivotal role for the UA argument. In particular, the issue of whether a certain theory is compatible with a scientific community's background assumptions—an issue which is required by the conceivability condition—is mentioned yet left untackled by Stanford. And this, we think, is a gap in his proposal that needs to be filled in, as making the conceivability condition explicit might improve the UA argument resilience to criticisms such as the one advanced here.

Stanford does acknowledge the possibility for a criticism in line with the one we focused on here, when he mentions the following question: "were the later alternatives unconceived by earlier practitioners really even serious ones at the time, given profound differences in available evidence, metaphysical presuppositions about nature, and methodological assumptions about its investigation?" (Stanford 2006: 59). However, we find his reaction to this specific point far from being a plausible answer, in that it does not really address the issue at stake. In fact, Stanford confines his answer to a mere hypothesis about the assumption that anyone raising the question above may hold, that is, that we currently occupy a privileged position in the history of science whose methodological assumptions and metaphysical presuppositions will not undergo the same fate of previously

[10] As a particularly instructive discussion of this problem, see Patton's (2011) analysis of Fizeau's classic optical experiments. According to her, there is no unequivocal way in which to account for the results of a certain experiment and thus there is no way in which the latter can be taken as unequivocally given.

discarded ones. That one needs to hold such an assumption to raise the problem of inconceivable unconceived alternatives is false advertising. Trivially, we can peacefully claim that the theory of special relativity could not be a serious scientific alternative for Newtonians, given the background assumptions they subscribed to, while maintaining that the theory of special relativity and everything it implies, from both a metaphysical and a methodological standpoint, will not be our final view on space, time, and matter.

We argued that it is not always the case that the failure to conceive theoretical alternatives happens across slices of history of science where there is a continuity in metaphysical, methodological, and empirical assumptions like the one Stanford envisages between mid-to-late 19th century theorists investigating inheritance and generation and contemporary theorists dealing with genetics and embryology. Even the most committed realists such as Saatsi (2019) concur that typically this continuity does not hold among theory-change. Provided that, also in the historical case Stanford considers, the employed notion of background assumptions remains unduly vague, we took the liberty to distinguish the sense in which Stanford uses it, i.e., as limitations that do not prevent conceiving, from the sense usually ascribed to the concept of background assumptions, unconceived alternative theories. Hence, we introduced the alternative label of "sociocultural limitations", for we think the latter is more consistent with the way Stanford employs the notion of background assumptions—whereas on the standard interpretation the latter would hardly accommodate cases in which radically different and subsequent theories were conceivable, yet the scientific community of that time failed to conceive them.[11]

A further remark concerns the sharp distinction Stanford seems to imply between theories and theorists, which patently clashes with the theory-ladenness of data collected to test theories as well as the empirical results that come out of this process. In section 4, we have given reasons to conclude that Stanford treats conceivability as an atemporal property of theories, rather than as a theorist's epistemic possibility with respect to a theory's content. As long as a theory is equally well supported by evidence as its rival(s), such theory has the ability to be conceived, regardless of whether the contemporary scientific community has the ability to conceive it. Put it otherwise, to assess whether a theory is conceivable we only need to look at its relationship to experience. The implication that conceivability is determined by the theory-evidence relation, with no inclusion of theorists as producers and consumers of theories, is hard to subscribe in general. In particular, it clashes with Stanford's project to redirect the pessimism inductively justified—and the antirealism thereof—to theorists rather than theories.

In the last section, we explored the consequences of accepting the distinction we highlight between conceivable *vs* inconceivable alternatives. As Stanford's main goal was to provide novel theoretical support to broadly anti-realist claims, we emphasized that, whenever a theory is unconceived (also) because of its inconceivability, then there are ways in which a tamed form of realism can be advocated. Still, we believe, the most interesting cases—at least when it comes to evaluate the UA argument—are those in which a certain theory is unconceived despite the absence of theoretical, empirical, and methodological impeding factors. For it is precisely in these cases that the untenability of the distinction

[11] Stanford also adopts the term "conceptual barriers or limitations" (132) to signal what might prevent from conceiving serious scientific alternatives to the accepted ones.

between theories and theorists comes to the forefront, while scaling back the realist *vs* anti-realist debate. In a way, we concur with Stein (1989: 56) when he argues that, though trying to unravel the role of theories in the evolving process of discovery is both intriguing and relevant, the matter in question cannot be simply resolved in terms of the realist *vs* instrumentalist discourse. Rather, we claim, one should focus on the way in which observations, models, predictions, methods, instruments, experiments, and values (Rowbottom 2019) mutually interact in the context of scientific practice and how other factors, such as ontological assumptions, theoretical principles, and standards of evidence play a crucial role in both theory-building and evidence assessments. Our claim is that to properly engage with the way in which scientific practice is carried out, one should primarily target those background assumptions that allow for the individuation of data and their selective arrangement within a specific theoretical context. This is what, in our view, is particularly wanting in Stanford's account and what prompted us to unravel the distinction between conceivable *vs* inconceivable alternatives in the first place.[12]

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