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Creativity, pursuit and epistemic tradition

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A B S T R A C T

This paper revisits the standard definition of scientific creativity in the contemporary philosophical literature. The standard definition of creativity says that there are two necessary, and jointly sufficient, conditions for creativity, novelty and value. This paper proposes to characterize the value condition of creativity in terms of "pursuitworthiness". The notion of pursuitworthiness, adopted from the recent debate on scientific pursuit in philosophy of science, refers to a form of prospective epistemic worth. It indicates that a certain object (such as a scientific hypothesis) is promising or has the potential to be epistemically fertile in the future, if further investigated. To support the claim that creative scientific instances are, qua creative, valuable in the sense of pursuitworthy, three examples of creative hypotheses taken from the history of the geosciences are introduced: MacCulloch's continuity hypothesis in mid-19th-century geology, Baron et al.'s phylogenetic hypothesis in contemporary paleontology, and the widely discussed Anthropocene hypothesis.

1. Introduction

Philosophers have frequently conceptualized creativity as a value, and sometimes even as a virtue (Gaut, 2010; Zagzebski, 1996). Today the most accepted definition of creativity in the philosophical literature is the so-called *standard view* of creativity. The standard view says that there are two necessary, and jointly sufficient, conditions for creativity, namely novelty and value (Paul and Kaufman, 2014, p. 6; Boden, 2004, p. 1; Sternberg & Lubart, 1999, p. 3). That is, creative objects and ideas are both novel and valuable, while creative individuals are those with the ability or disposition to create novel and valuable products.[1]

Advocates of the standard view of creativity sustain that the value condition is indeed fundamental in the definition, because, as a matter of fact, we attribute a form of merit or value to the instances we call creative in everyday situations. When we praise someone (as creative) after having come up with a novel, previously unimagined solution to a difficult task at hand; when we admire an artwork (as creative) for its revealing novel use of colour or form; or when we celebrate a scientific invention (as creative) for the new technical possibilities it advances, we are ascribing some form of value to those people, artworks, and inventions. Without a value condition in the definition of creativity, we wouldn't be able to capture typical distinctions we make between things that are just novel (including those that are novel but in a conventional, uninteresting way) and things that are creative.

Let's assume that we accept the standard view as a valid framework to address the phenomenon of creativity –which is not something that all contemporary philosophers accept, as I will discuss in Section 2 (see Hills & Bird, 2018). A further question that arises in that case, which hasn't been sufficiently addressed yet, is: but what is the *precise type of value* that we are ascribing objects, ideas, and individuals by calling them creative? A mere value condition in the definition of creativity is quite general and vague. Things can be valuable in different ways –epistemically, aesthetically, intellectually, morally. And even when just focusing on a specific type of value (i.e. epistemic), there is a plurality of forms that such value can adopt: an idea can be truthful, sound, precise, coherent, unifying, etc. So what specific value is involved in creativity attributions? Responding to this question is important if philosophers want to advance a comprehensive account of why and under which circumstances epistemic agents endorse creative ideas, and under which other circumstances it might be reasonable to discard them. This is the problem that occupies the present paper.

The domain of science is the specific focus of analysis here, and thus it is scientific creativity. Sometimes in their practice, scientists judge some theories, models, methodologies, and hypotheses as creative. If we accept

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[1] To these two conditions, a third condition of intentionality or motivation is often added, since the standard view tries to avoid that novel and valuable objects that might have been produced by pure luck are included as cases of creativity too. In this article, I will not discuss this third condition of intentionality or motivation, but will focus only on the other two. I take that adding a condition of intentionality to my proposal here is largely unproblematic. For further discussions on intentionality in creativity see Kieran (2014), Gaut (2012), and Stokes (2008).

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the standard view on creativity, then these scientific objects are, qua creative, novel and valuable. But, again, when a scientist considers a certain scientific theory creative, is she also judging it correct or approximately correct –insofar as it is valuable? Or if a scientist thinks that a scientific model is creative, is it implied that, apart from novel, the model is empirically accurate? Or if scientists claim that a certain scientific hypothesis is creative, are they suggesting that it has a high plausibility of being true? Probably most scientists and philosophers alike would be suspicious to respond positively to the previous questions, and would rather give an answer such as "not necessarily", or "that depends on the particular model or hypothesis". If so, the challenge philosophers of creativity face is how to distinguish value attributions applicable to creative instances from those that we should retrain ourselves from making. In other words, philosophers need to spell out the value condition in the definition of creativity in more specific terms, avoiding the vague formulation of the standard view. Without such specification, it will be difficult to appreciate how creative theories, models, methodologies, and hypotheses can actually contribute to scientific research. Moreover, without such specification, we run the risk of expecting creative scientific objects to have merits that they might not possess.

The specific proposal of this paper is that we should spell out the value condition of the definition of creativity as "pursuitworthiness", at least for the case of scientific creativity.[2] A pursuitworthy scientific object (hypothesis, theory, model, methodology) is one that deserves to be explored, scrutinized, developed further. This "deserving to be further pursued", I will defend, is the specific sense in which creative instances are valuable. But why is "deserving to be further pursued" a value at all? The key is that to be pursuitworthy is to be promising, exhibiting a potential, projecting an expectation of epistemic success into the future. So a creative scientific object might not have achieved a form of epistemic success (accuracy, truthfulness, correctness) in its current form yet. But it contains a novel component –with respect to the tradition in which it has been produced– combined with a promise that, if further investigated, it could bring a significant epistemic contribution into future research.

To spell out the implications of characterizing the value of creativity as pursuitworthiness, this paper incorporates insight from the recently revivified debate on *pursuit* in philosophy of science. The concept of pursuit was originally introduced by Laudan (1977) in the philosophy of science, and since then it has been further discussed by Whitt (1990, 1992), Nickles (1980, 2006), Franklin (1993), McKaughan (2008), Seselja and Strasser (2013, 2014), Nyrup (2015), Fleisher (2018), Shan (2020), and Shaw (2022), among others. The shared consensus in this literature is that pursuit refers to a specific form of epistemic appraisal that scientists can adopt, in which they assess whether a hypothesis, theory, model, or methodology is worthy of further investigation. The contrast is usually established between appraisals of pursuit and appraisals of acceptance. In the latter, scientists assess whether a hypothesis should be adopted as a true or well-established piece of knowledge. What is important for the proposal here is that judging a hypothesis pursuitworthy is independent from accepting it as true or as a well-established piece of knowledge: a scientist can do the former without the latter, and the latter without the former.

The examination of historical cases will be central to support the revised definition of the value of creativity proposed in this article. After introducing, in Section 2, the existing problem of how to understand the value condition in the standard view on creativity, I propose, in Section 3, to reformulate such value in terms of pursuitworthiness, drawing on contemporary literature on pursuit in philosophy of science. Then, in Section 4, the suitability of this new formulation is examined in light of three examples of creative scientific hypotheses, selected from the recent history of the geosciences. The first example is taken from 19th-century geology, when John MacCulloch (1831) proposed the hypothesis of the continuity between different rocks of volcanic origin, with interesting consequences for aetiology and geological methodology. The second example is the phylogenetic hypothesis advanced by Baron et al.'s (2017) in contemporary paleontology, which promised to prompt significant changes in the research on dinosaur origins. The third example is the well-known Anthropocene hypothesis, extensively discussed beyond the field of stratigraphy, where it was originally formulated, in recent years.[3] Taken together, these examples offer an illustrative range of circumstances and historical debates around the adoption of creative hypotheses, highlighting the importance of their promising, worth pursuing character. The concluding section sketches some consequences of the present proposal and suggests possible points for further investigation.

2. The epistemic value of creativity

The so-called *standard view* of creativity is, as advanced in the introduction, the most widely accepted definition of creativity in the contemporary philosophical literature. It states that an object or idea is creative if and only if it is novel and valuable. Although the standard view can be traced back to Kant (1790/2000) and his characterization of the creative genius, its more contemporary formulations can be found in Sternberg and Lubart (1999, p. 3), Boden (2004, p. 1), and Paul and Kaufman (2014, p. 6). Other philosophers that endorse the standard view as the basis for their accounts of creativity are Stokes (2008), Gaut (2012), Kronfeldner (2014), and Kieran (2014).

One of the points of consensus among advocates of the standard view is that "being valuable" needs to be included as a necessary condition in the definition of creativity because there are ideas and objects that are novel but we would not normally consider creative. This argument has been also formulated as the "argument against original nonsense" (Kant (1790/2000); Gaut, 2010; Paul and Kaufman, 2014). The idea is that, if the value condition is removed from the definition of creativity, cases of original nonsenses, that is, cases where something novel but completely incomprehensible to us is produced, would be considered cases of creativity too. However, the most shared intuition is that creative ideas need to speak to us at least in some way, be graspable, comprehensive, even in cases when they challenge our usual ways of understanding the world.

Unconvinced by the "argument against original nonsense", some contemporary philosophers, such as Hills and Bird (2018), have tried to show that creative ideas and objects do not need to be valuable. To the contrary, they can be worthless, false, or of wholly negative value (Ibid.: 8). We all know of cases where endorsing a creative idea ends up being a *bad* idea, for instance when the solution it promises to offer is eventually not reached, or when it leads to false claims. So, why insisting on offering a definition of creativity in terms of value? Along these lines of reasoning, Hills and Bird contend that keeping the value condition in the definition of creativity would wrongly invite to adopt an "unreflective approval of creativity", which they consider "both widespread and deeply misguided" (Ibid.: 18).

It is beyond the scope of this article to discuss the specific arguments that Hills and Bird (2018) advance to make their case, and how they have

[2] It remains to be seen if the proposal to spell out the value condition of creativity as pursuitworthiness is also applicable to other contexts outside science. Works like Carroll (2010), discussed in Section 3, invite to think that this would be also the case for creativity in the arts, but further work would need to be developed.

[3] Although the three examples examined here are cases of creative scientific *hypotheses,* the arguments presented below would equally apply to creative scientific theories, models, and methodologies. The decision to pick examples of hypotheses is due to argumentative simplicity, since the distinction between appraisals of pursuit and appraisals of acceptance is more straightforward when it concerns propositional units like hypotheses. That is, non-propositional units such as scientific models cannot be *literally* assessed as true or false in appraisals of acceptance, so additional argumentative steps are required to make the same point.

been recently disputed (see Sánchez-Dorado, 2020). What is important for the aims of this article is that the confrontation between philosophers who endorse the standard view of creativity and those like Hills and Bird (2018), who reject it because they think that keeping the value condition is equivalent to unreflectively accepting the worth of creative ideas and objects, is that there is a manifest lack of clarity about where the value of creativity would exactly lie. In other words, even philosophers endorsing the standard view should be wary that a mere value condition in the definition of creativity is too unspecific, broad, vague, and can invite one to misattribute creative instances merits that they do not possess. In a previous paper, I argued that the most satisfactory response to the challenge that Hills and Bird advance is not to offer an alternative definition of creativity that excludes the value condition from it, as Hills and Bird propose to do, but to try to spell out as much as possible what the value condition of creativity specifically entails in particular epistemic –or aesthetic– contexts (Sánchez-Dorado, 2020).

The aim of this paper, partly building on and partly revising the ideas already sketched in the previous one, is to advance a more informative characterization of the value condition of creativity that can help improve the current formulation of the standard view. The specific proposal here the specific proposal here is that, at least for the domain of science, we should spell out the value condition of (scientific) creativity in terms of "pursuitworthiness". The next section addresses how pursuitworthiness should be understood in this context, but before doing that, a couple of clarifications regarding the standard view need to be introduced in this section.

In most works endorsing the standard view of creativity, the conditions of novelty and value are interpreted as being simply juxtaposed or added to one another (such as in Kieran [2014] and Kronfeldner [2014]). A fundamental assumption in this paper, in contrast, is that the conditions of novelty and value are not just added, but tightly entangled. So even if so far in this article, for the sake of simplicity, I have said that "pursuitworthiness" should substitute the mere value condition in the standard view –and then be added to the novelty condition– what is going on really is an intimate combination of pursuitworthiness and novelty that gives rise to the phenomenon of creativity. In other words, when analyzing creative instances, the novelty and the pursuitworthiness components cannot be disentangled without content loss. This is because 'creativity' functions as a thick epistemic concept, which contains two inseparable components, a descriptive one (novelty, or more precisely, newness[4]), and an evaluative one (pursuitworthiness) (Sánchez-Dorado, 2020; see Roberts [2018], Kyle [2016], Kirchin [2013], and Kotzee and Wanderer [2008] for accounts on thick epistemic concepts in line with my argument).[5] It will become clearer in the next sections why treating the conditions of novelty and pursuitworthiness as entangled has different implications than treating them separately: a scientific hypothesis can be both novel and, in virtue of other epistemic merits, pursuitworthy, without being creative.

A second clarification that needs to be made is that this paper is not the first attempt to articulate the value condition of the standard view of creativity in more specific, informative terms than a mere assertion of value. In an early work, Stein (1953, p. 311) suggested that a creative work is "a novel work that is accepted as *tenable* or *useful* or *satisfying* by a group at some point in time'' (my emphasis; in Klausen, 2010, p. 350). Amabile's (1996, p. 35) version of the standard view says that a creative product is "both a novel and *appropriate, useful, correct* or valuable response to the task at hand" (in Klausen, 2010, p. 350). Sternberg and Lubart (1999, p. 3) describe creative works as those that are both novel and *appropriate*, understanding appropriate as "*useful, adaptive* concerning task constraints" (my emphasis). More recently, Hills and Bird (2018, p. 14) have identified, in their critical article "Against creativity", the value condition in the standard view of creativity with the truthfulness of creative ideas. It is precisely the identification of value with truth which motivates them to reject the standard view and propose an alternative definition of creativity that does not include a value condition.

What these different versions of the standard view have in common in the way they spell out the value condition is that they assume that a creative object, qua creative, can display a form of epistemic success in its current state (Klausen, 2010, pp. 349–350). Being useful, appropriate, correct, and truthful, as Stein (1953), Amabile (1996), Sternberg and Lubart (1999), and Hills and Bird (2018) respectively propose, are ways of being epistemically successful *in the present*, in the *current form* of an object. Yet, I maintain that this is not an adequate way of articulating the value of creativity, as the historical examples of the adoption of creative hypotheses in the geosciences, discussed in Section 4, will illustrate, since it is not uncommon to consider certain ideas creative even if we still don't know if they are correct or not, and even if we assertively know that they are false or inaccurate in their current form (see Hills & Bird, 2018, §3; Sánchez-Dorado, 2020). Instead, articulating the value of creativity as a *prospective* kind of value can capture much more fairly the epistemic merit that creative instances exhibit. The notion of pursuitworthiness precisely helps to mark that prospective kind of value.

3. Pursuitworthiness in scientific practice

The notion of pursuitworthiness that I propose to adopt to characterize the value condition of creativity comes from the contemporary debate on scientific pursuit in philosophy of science. Larry Laudan originally proposed the notion of "context of pursuit" in 1977, with the aim of conceptualizing an observed middle phase –or "nether region"– between the generation and the acceptance of scientific hypotheses (Laudan, 1980, p. 174). Each research tradition in the history of science, Laudan observed, goes through a number of different (and sometimes mutually contradictory) formulations after it has been generated, often extending through a significant period of time (1977, p. 79). The practice of entertaining and reformulating hypotheses was neither captured by the notion of generation nor acceptance, so an additional stage of pursuit needed to be recognized in the analysis of the scientific inquiry. Moreover, identifying a stage of pursuit made manifest the remarkable fact that when a new scientific theory appears, scientists sometimes decide to work on it even if there are alternative, already accepted, theories with fewer anomalies at that time (1977,p. 110; see also Nyrup, 2015, pp. 752–3).

Since its original formulation, the concept of pursuit has been examined by numerous philosophers of science, such as Whitt (1990, 1992), Nickles (1980, 2006), Franklin (1993), and McKaughan (2008). And in the last few years, it has gained central stage in philosophy of science again, especially thanks to the contributions of Seselja and Strasser (2013, 2014), Nyrup (2015), Fleisher (2018), Shan (2020), and Shaw (2022). A shared conceptualization of pursuit that comes through in these works is that one could distinguish between a descriptive and a normative sense of 'pursuit', even when the two are often discussed together. In the descriptive sense, pursuit refers to the temporal stage of inquiry in which hypotheses are under development, and scientists are working on their articulation and refinement. In the normative sense, pursuit refers to a specific form of epistemic appraisal that scientists can adopt, where they ask themselves whether a hypothesis or theory is worthy of further investigation by the community (Seselja & Strasser,

[4] Although the standard view of creativity refers to "novelty" as one of the necessary conditions for creativity, this term already contains an element of positive value. "Newness" would be a more value-neutral term. This by itself is evidence that 'creativity' functions as a thick concept, since its descriptive and evaluative components cannot be easily –if at all– separated.

[5] That creativity attributions have a positive evaluative element attached to its descriptive element of novelty is for instance shown in data gathered by the SenticNet, where the term 'creativity' and derivatives have in everyday talk a positive valence of around 0.45 (see the SenticNet concept search resources in < https://sentic.net/>.

2013, p. 9).[6] The main concern in this paper is with the normative question of which hypotheses are considered pursuitworthy by scientists, so the descriptive sense of the concept will be left aside for now.

A way in which appraisals of pursuit have been typically defined is in contrast to appraisals of acceptance. The idea is that while accepting a hypothesis in science is to regard it as true or a piece of well-established knowledge, pursuing a hypothesis involves investigating it further. A hypothesis that is now worthy of further investigation might be eventually accepted as true in the future. But it doesn't need to be so: it may be also rejected or assessed as inadequate. What matters is that a scientist's decision to accept a certain hypothesis (or theory, model, methodology) does not entail a decision to work on it, nor is it the case that a scientist who decides to further work on a hypothesis has also thereby decided to accept it (Whitt, 1990, p. 471; Franklin, 1993, p. 253; Nyrup, 2015, p. 752). Whitt offers the example of how in the nineteenth century numerous chemists were devoted to the pursuit of the Daltonian theory, while openly declaring to be reluctant or unwilling to accept it as true (1990: 467).

Apart from stating what being pursuitworthy is not, namely, it is not being accepted as true or well-established knowledge, we still need to ask what exactly pursuitworthiness in science entails. Laudan originally proposed that a pursuitworthy scientific hypothesis is one that has a high rate of progress, that is, a hypothesis that is capable of generating solutions to problems at an impressive rate (1977, p. 111). The problem with this definition is that a scientific hypothesis might still be at a very early stage of its development, and therefore have a very poor track record behind it. The hypothesis might also present multiple anomalies in its current formulation. And still, scientists might want to further investigate it if they judge the hypothesis to have a rich programmatic character (Seselja & Strasser, 2014, p. 3131). As Feyerabend criticized of the Laudanian definition of pursuit, the lack of performance and inadequacy has never stopped people from pursuing ideas they regarded as important (Feyerabend, 1981, p. 67).

It seems, therefore, that an adequate definition of pursuitworthiness should not be centered on what a research hypothesis has already achieved or on its current performance, but on its prospects in the field. In appraisals of pursuit, scientists do not judge whether a hypothesis exemplifies particular virtues but whether it could exemplify them in the future if it continued to be investigated (i.e. further developed, revised, refined, tested): they are interested in diachronic or modal properties, not in actual properties (Shaw, 2022, p. 104; Shan, 2020, p. 188). As Seselja and Strasser have clearly explained,

> … when we evaluate whether a theory is worthy of pursuit, instead of focusing on its explanatory anomalies, we are rather interested in its programmatic character which indicates that the investigation can proceed in spite of the current anomalies and towards their resolution. Hence, we are interested in the prospective values, which allow for a prospective assessment, rather than a retrospective one, which is typical for the context of acceptance. (Seselja & Strasser, 2013, p. 10n2)

Accordingly, Seselja and Strasser offer a specific definition of a pursuitworthy hypothesis as a hypothesis that has potential explanatory power, potential inferential density, potential consistency, and a programmatic character (2014, p. 3123). Also in agreement with the diachronic conception of pursuit, McMullin describes pursuitworthy hypotheses as those that can offer a convincing response to the question "what is its research potential for the future?" by "estimating its imaginative resources … for future extension", instead of by tracing the hypothesis' career (1976, p. 400). More specifically, McMullin considers a hypothesis worthy of pursuit if it shows potential to give rise to interesting extensions, handle outstanding problems in the field, unify diverse areas, or open up an entirely new research territory (Ibid., pp. 423–424). Shan has also recently proposed a diachronic characterization of scientific pursuitworthiness –or more precisely, 'promise' in his account– in terms of potential usefulness (Shan, 2020, p. 181; see also Shaw (2022, p. 104) for an exhaustive review of different accounts of pursuitworthiness).

The prospective element in these various definitions of pursuitworthiness, articulated in the language of "potential" or "promise", establishes a clear distinction with the acceptance of a hypothesis. The acceptability of hypotheses does not concern their possible future epistemic developments, but the present estimation of their epistemic success, usually in terms of truthfulness and empirical adequacy (McMullin, 1976, p. 400; Shaw, 2022, p. 104).

I propose to adopt the type of prospective or diachronic definition of pursuitworthiness to define the value condition of creativity, with a particular focus on scientific creativity. Thus, I understand here pursuitworthiness as the *potential of a scientific object (hypothesis, theory, model, methodology) to exhibit certain epistemic benefits in the future.* This is by no means a new definition of pursuitworthiness; to the contrary, it is a minimal way of capturing some of the essential features of pursuitworthiness identified by several philosophers of science in the debate of pursuit (McMullin, 1976; Seselja & Strasser, 2013, 2014; Shan, 2020; Shaw, 2022). This definition is minimal because it does not make any specific commitment about how a pursuitworthy scientific hypothesis, theory, model, or methodology is going to be epistemically beneficial in the future. I take that sometimes they are expected to exhibit explanatory power, inferential density, or consistency in the future (as Seselja and Strasser [2014] propose), other times they are expected to exhibit usefulness (as Shan [2020] proposes), and still other times they are expected to be able to handle outstanding problems and unify diverse areas of research (as McMullin [1976] proposes). Committing to one of these specific definitions of potential epistemic benefit is not required for my proposal about creativity here. Moreover, these definitions don't need to be incompatible with one another, since one could accept a plurality of ways in which hypotheses are judged as worthy of further pursuit (see Shaw, 2022, p. 104). What matters is that, beyond this plurality, pursuitworthiness is understood as a prospective type of epistemic worth.

Following this minimal diachronic definition, I argue that creative scientific hypotheses (and theories, models, methodologies) are pursuitworthy because they exhibit a potential to be epistemically beneficial in the future, that is, in case they were further investigated.[7] But what distinguishes a creative scientific hypothesis from a non-creative hypothesis that might also be pursuitworthy? The pursuitworthiness of creative hypotheses is special because it is closely entangled to an element of novelty –the other necessary condition for creativity identified in the standard view. This novelty is such that calls attention to unexplored aspects of the epistemic tradition in which such hypotheses are embedded, and brings them effectively to the fore of the research.[8] The novelty and pursuitworthiness conditions come together because it is the novel revisitation of topics, approaches, techniques or assumptions of the past of a field what then establishes the programmatic character of

[6] Although the normative sense of pursuit has been sometimes discussed in connection to the problem of theory choice, as in Laudan's (1977) original proposal, several commentators have pointed out that appraisals of pursuit do not necessary involve choosing between competing theories, but the rational assessment of individual theories. For instance, assessing a hypothesis as worthy of further pursuit does not imply rejecting its rivals as unworthy of pursuit. There are situations where it may be rational for a given scientific community to pursue two or more research hypotheses at the same time (Seselja & Strasser, 2014, p. 3121; Shan, 2020, pp. 187–188).

[7] As advanced at the beginning, I am from now on in the article mainly referring to creative scientific *hypotheses* to exemplify my proposal, for the sake of simplicity. But the definitions of pursuitworthiness and creativity adopted here equally apply to scientific theories, models, and methodologies.

[8] With the notion of "epistemic tradition", I am including here the set of previous commitments, techniques, and methodologies of a certain scientific field at a specific historical moment.

creative hypotheses. That is to say, creative hypotheses formulate a promise about how examining unexplored aspects of the epistemic tradition could open up fertile paths of inquiry in the future. Or in other words, creative scientific hypotheses negotiate the transition between the past of an inquiry and the prospects of it more explicitly and richly than non-creative hypotheses.[9]

The proposal to spell out the value condition of creativity as pursuitworthiness, entangled with a novelty condition, is substantially inspired by Noël Carroll's (2010) account of creativity, originally advanced as an account of artistic creativity. For Carroll, a creative object is one that recombines "elements and concerns of the tradition in an especially deft, original, or insightful way", and by doing so it allows us "to see afresh the tradition we thought we knew so well" (Carroll, 2010, p. 70). Carroll also points to the fact that by clarifying a tradition, creative objects formulate a promise into the future. When we call certain objects creative, he says, we are …

> … issuing a promissory note — a bet that they will be fruitful — given the clarity they have already brought to the tradition. We suppose, reasonably, that that clarity will have consequences. Though we may be wrong in this, that expectation is not without grounds (Carroll, 2010, p. 71).

Carroll is certainly alluding here to the prospective judgments involved in creativity attributions, even if he doesn't appeal to the more precise notion of pursuitworthiness that I propose to use here. Epistemic agents judge that it is "reasonable", "not without grounds" to have expectations on how creative ideas develop, since they issue "a promissory note" about the consequences that they might have later (Ibid.).

It should be evident by now why the characterization of the value of creativity as pursuitworthiness differs from proposals that spell it out in terms of current epistemic success (i.e. usefulness, truthfulness, appropriateness) (Stein [1953]; Amabile [1996]; Sternberg and Lubart [1999]; Hills and Bird [2018]; Stein [1953], Amabile [1996] and Sternberg and Lubart [1999], Hills and Bird [2018]). The definition proposed here articulates the value condition of scientific creativity as a prospective type of value, not as a value about the track record or present virtues of scientific objects. Creative scientific hypotheses do not offer reasons, qua creative, for their acceptance. But it is rationally justified to continue the investigation of creative hypotheses given how perspicuously they illuminate parts of the tradition, while showing the potential to enlarge, expand, or revise such parts in the future. If scientists decide to further investigate creative scientific hypotheses, it is because those hypotheses have helped them to see more clearly than before gaps or overlooked ideas of the past of a field that can turn out to be insightful in the future inquiry. The next section presents three historical examples to show how creative scientific hypotheses establish a dialogue between unexplored parts of the epistemic tradition and future expectations about a research program.

4. Three creative scientific hypotheses in the geosciences

Three examples in the recent history of the geosciences are presented in support of the claim that the value condition in the definition of creativity should be spelt out as pursuitworthiness. The first example, MacCulloch's continuity hypothesis, is from mid-19th-century geology; the second, Baron et al.'s phylogenetic hypothesis, is a case in contemporary paleontology; the third, the Anthropocene hypothesis, originally proposed in the field of stratigraphy, is widely discussed today in a range of Earth and social sciences. Each of these creative scientific hypotheses brought into light unexplored aspects of the past of the field in which they were produced. In doing so, they articulated a promise about how further examining such aspects of the past could be beneficial for the future of the inquiry. Thus, taken together, these cases help exemplify how being creative is epistemically valuable insofar as it makes a hypothesis worthy of further pursuit, even if such creativity does not contribute to making the hypothesis more apt to pass appraisals of acceptance.

4.1. *MacCulloch's (1831) continuity hypothesis in geology*

John MacCulloch (1773–1835) was a Scottish geologist, president of the Geological Society of London between 1816 and 1818, who published the first comprehensive account of the geology of the western islands of Scotland in 1819 (in Cumming, 1980, pp. 157, 170).[10] Later, in 1831, he wrote *A system of geology, with a theory of the earth and an explanation of its connection with the sacred records,* winning both acclaim and some critical reviews from the geological community at that time (in Ibid., pp. 171–173). Here, MacCulloch advances the hypothesis, framed within his history of the Earth, of the continuity between various rocks of volcanic origin despite the perceived differences between them (MacCulloch, 1831, v.1, p. 200). From inferences based on the observation of rocks in the field, MacCulloch postulated the existence of a gradual transition from the lavas recently ejected from volcanos to the samples of trap rocks found in the west coasts of Scotland, which had the shape of flight of steps and at first sight resembled stratified rocks (MacCulloch, 1831, v.1, pp. 2–3; see also Whewell, 1840, v.2, p. 565–6). He formulates the continuity hypothesis in these terms:

> If it be said that volcanoes do not produce perfect granite, it must still be recollected that they produce compounds of an analogous nature in every respect. [… It] was also shown that the trap rocks often assumed the characters of perfect granite; so that, by this intermediate step, the several products which are most distant are again associated. Even admitting that the volcanic rocks stood exclusively at one extremity of a scale of chemical compounds, and the granites at the other, the trap rocks, containing examples of both, form the common link by which they are united. (MacCulloch, 1831, v.1, p. 200)

MacCulloch's special attention to trap rocks was motivated by his conjecture that they could offer the link between different rocks of igneous origins. He hypothesized that the probable reason for the differences in the chemical appearances of these rocks was the time through which the fused materials had cooled, as well as the fact that they could have been either formed under water or cooled in the open air (MacCulloch, 1831, v.1, p. 199; Lyell, 1835, v.4, p. 353–4). Until then, many igneous rocks investigated in places like Germany, France, and Scotland had been associated with marine strata, since they were found less porous and more compact than lavas produced in the atmosphere. So, their connection with ordinary volcanic action was overlooked (Lyell, 1835, v.4, p. 353). This had for a long time given support to aqueous or neptunist theories, which claimed that rocks like basalt and granite were the product of sedimentation processes (Hallam, 1983, pp. 23–24). MacCulloch's hypothesis played a role in overturning these arguments. But not only that: the continuity hypothesis articulated a promise about how further pursuing it could help develop a fertile methodological approach to the study of rocks in future geological investigations.

In the decades before the publication of MacCulloch's *System,* there was already vast empirical evidence in favour of the igneous origin of

[9] Being creative is a matter of degree, as being novel and pursuitworthy also are. So I take a creative scientific hypothesis to be one that is considered *significantly* or *substantially* creative by a community of scientists, not one that could be creative in a very minimal sense (in this sense, one could argue that every hypothesis is creative). See Carroll (2010, pp. 70–71), Kieran (2014, pp. 126–128), Hills and Bird (2018, pp. 13, 17), and Sánchez-Dorado (2020, p. 16) for discussions on different degrees of creativity.

[10] That account was in MacCulloch (1819) *A description of the western islands of Scotland including the Isle of Man: comprising an account of their geological structure; with remarks on their agriculture, scenery and antiquities*.

rocks like basalt, collected among others by Raspe, Arduino, Vicentin, Desmarest, Collini, Guettard, and Faujas (Lyell, 1835, v.4, p. 353). But it was partly thanks to the creative continuity hypothesis that MacCulloch advanced that those pieces of evidence were brought together into a unifying postulate, incorporating also theoretical assumptions on the causes that originated those rocks. The creativity of the hypothesis can be spelt out as a combination of novelty and pursuitworthiness that called attention to aspects of the epistemic tradition in geological research that had been overlooked until then. Namely, it called attention to the fact that vulcanists had fixated on classifying individuals rocks as igneous or not, while engaging in bitter confrontations with neptunists for years. Meanwhile, they had overlooked that an element of transition between different rocks, which could offer a bigger picture of the shared origin of rocks, was missing in their account. The value of the continuity hypothesis, qua creative, was such that highlighted these missing elements in the research, while setting expectations about how adopting the hypothesis could have a positive impact on the future methodology of the field. In particular, MacCulloch's continuity hypothesis promised a smooth transition from vulcanist theses to uniformitarianism as a research technique in the geological sciences (Hallam, 1983, p. 29).[11]

MacCulloch was well-aware of the epistemic potential of his creative hypothesis when he sustained that, if other geologists decided "to pursue, unbiassed, the chain of observations which is only here for the first time indicated, more instances of the same nature [i.e. gradual transitions between rocks] will be brought to light" (MacCulloch, 1831, v.1, p. 159). Also, the philosopher and scientist William Whewell, knowledgeable of the recent discoveries in geology at that time, would explicitly recognize the creativity –or "sagacity"– of MacCulloch's hypothesis (Whewell, 1840, v.2, p. 565–6). Whewell judged the continuity hypothesis as worth further pursuing by the community of geologists, first, because it offered a novel grouping of rocks that materially extended the effects ascribable to volcanic agency (Ibid.). Thus, these newly ascribed effects required to be further examined and tested. But more importantly, MacCulloch's idea of a progressive transition between rocks had the potential, for Whewell, to impact upon Aetiology, that is, the investigation of natural causes (Whewell, 1840, p. 565–6, 660; see also: Ruse, 1976, p. 247).

That MacCulloch's hypothesis was considered worth further pursuing by his contemporaries is manifest in the fact that many geologists in the mid-19th century actually adopted it, submitted it to scrutiny, and investigated its consequences further. Evidence is found in Charles Lyell's references to MacCulloch's hypotheses in the *Principles of Geology* (1830–1835), as well as in J. L. Comstock's *Outlines of Geology (1841)*, and James Nicol's *Guide of the Geology in Scotland* (1844) (Cumming, 1980, pp. 172–3). Nonetheless, the decision to pursue MacCulloch's hypothesis did not exactly imply that they judged it to be a valid, true, or a well-established piece of scientific knowledge. As a whole, MacCulloch's account of the Earth's history would be eventually considered flawed and rejected by the community of geologists. Among other shortcomings, his theses did not take into account a large amount of evidence already available at that time, such as that found in fossils. More precisely, MacCulloch's theses did not pass scientists' appraisals of acceptance because they were too "mineralogically biased", that is, because they did not consider important evidence pertaining to organic remains (Cumming, 1980, p. 174; MacCulloch, 1831, v.1, p. 406; Comstock, 1841, p. 241). Still, his continuity hypothesis would pass scientists' appraisals of pursuit for decades, partly because its creativity helped illuminate missing parts of the past of the research that had been overlooked until then in the geological methodology.

4.2. *Baron et al.'s (2017) phylogenetic hypothesis in paleontology*

A second example in the geosciences that illustrates the value characteristic of creative scientific hypotheses, that is, their pursuitworthiness, entangled with an element of novelty, is found in a case in paleontology discussed by Currie (2018). Traditionally in paleontology, early dinosaurs had been grouped, depending on their hip morphology, either as Ornithischia (bird hipped) or as Saurischia (lizard hipped). This division shaped for a long time the formulation of questions regarding the evolution of dinosaurs in later periods (late Triassic and Jurassic), as well as the taxonomic allegiances of fossil data (Currie, 2018, p. 41). In a recent publication, Baron, Norman, and Barrett (2017) proposed to draw basic phylogenetic divisions in a very different way. The new division was not constrained by the stringent traditional classification into two groups based on hip morphology (Baron et al., 2017). Instead, it placed Ornithischia and Theropoda –conventionally integrated within the Saurischia group– together in the newly coined group of *Ornithoscelida*, a term that had been originally proposed by T. H. Huxley in 1870, and that Baron et al. helped revitalize (Baron et al., 2017, p. 502; Huxley, 1870). Baron et al.'s creative reshaping of evidence under a previously unconsidered classification had the potential to prompt significant changes in the research on dinosaur origins, with regards to the study of their anatomy, diet, and geographic and temporal origins (Baron et al., 2017, p. 505).

As Currie argues, proposing a new phylogenetic distinction opened the door to a much wider set of analyses and interpretations in the field, "leading to hotter searches and thus a more creative science of early dinosaurs" (Currie, 2018, p. 41). The new grouping was, qua creative, epistemically valuable in the specific sense that it advanced a promise about a potentially richer understanding of dinosaur origins. That is, the hypothesis displayed a genuine form of pursuitworthiness combined with novelty. The fact that Baron et al. decided to revive a terminology proposed in the 19th century by T. H. Huxley is a sign of how creative hypotheses tend to bring to the fore portions of the past of a discipline that had been overlooked, while revising others that were taken for granted. What had been in the last few decades a widely accepted phylogenetic distinction based on the hip morphology –and for that reason almost invisible for the community, in the sense of integrated in everyday practices and endorsed almost obliviously– is now made manifest by Baron et al.'s creative hypothesis, brought to the fore, and questioned.

As in the previous example, the pursuitworthiness of Baron et al.'s hypothesis is manifest in the fact that since it was proposed, it has been further explored, articulated, refined, and also challenged by the community of paleontologists. Evidence of it is found, for instance, in discussions on it in Holtz (2017, p. 30), Tsai, Middleton, Hutchinson, and Holliday (2020, p. 1659), and Castiglione, Serio, Mondanaro, Melchionna, and Raia (2022, p. 2). Remarkably, even some of the detractors of the hypothesis, such as Langer et al. (2017), had to recognized that Baron et al.'s creative distinction between groups of early dinosaurs "differ so radically from all previous cladistic analyses, and decades of pre-cladistic research, that they *deserve close scrutiny*" (2017, p. E1; my emphasis). Their skepticism towards the new phylogeny did not impede Langer et al. to be "excited about the Ornithoscelida hypothesis, which will certainly reinvigorate the study of dinosaur origins" (Ibid., p. E1-2). This "deserving a closer scrutiny" is a way of articulating the idea that the hypothesis was worth further pursuing, given its potential to "reinvigorate", or positively help advance, research in the field. Like in the case of 19th-century geology, Baron et al.'s phylogenetic hypothesis entails a novel component entangled to a promissory note, by which unexamined parts of the epistemic tradition are bonded to expectations

[11] The term uniformitarianism can refer both to a method or research technique in the geological science and to a theory about Earth systems (Hallam, 1983, p. 24). As a research method, the relevant meaning here, uniformitarianism consists in the study of present-day observed processes as a means of interpreting past events (Ibid.). As an Earth system, Uniformitarianism was a specific theory initially endorsed by James Hutton, and then substantially developed by Charles Lyell. It invoked that the actions of existing processes were sufficient to shape the surface of the Earth when they were acting over long time scales (Baker, 1998, p. 173). Uniformitarianism as an Earth system stood in opposition to catastrophism, principally endorsed by Abraham Werner and his students, but also by William Whewell. Catastrophism assumed that the processes acting on rocks laid down in a primordial ocean were presumably much more intense that those that could be observed today (Ibid.).

about the future advancement of scientists' understanding of early dinosaurs.

4.3. The Anthropocene hypothesis (−2000)

A third example that illustrates how creative scientific hypotheses entail a genuine type of pursuitworthiness is the Anthropocene hypothesis. The Anthropocene hypothesis is an outstanding hypothesis in the current debate in stratigraphy, but way beyond it as well. The idea that the beginning of a new geological epoch has already occurred, whose distinctive feature is the "central role of mankind" in it, was originally proposed by Paul J. Crutzen and Eugene F. Stoermer in 2000 (Crutzen & Stoermer, 2000, p. 17). In their article, they postulate that the "major and still growing impacts of human activities on earth and atmosphere" demand the conceptualization of a new geological time unit, with onset in the 18th-century industrialization period (Ibid.). Crutzen and Stoermer believed that adopting the Anthropocene hypothesis could contribute to better investigate the overwhelming observation that "mankind will remain a major geological force for many millennia, maybe millions of years, to come", as well as to encourage the development of sustainability strategies in the face of human induced stresses to the ecosystems (Ibid., p. 18).

Since Crutzen and Stoermer's article was published, the Anthropocene hypothesis has been not only thoroughly scrutinized by multiple scientific communities, but also adopted both by scientists and laypeople almost "matter-of-factly", that is, "as if it were already part of accepted geological time terminology" (Zalasiewicz, Waters, Williams, & Summerhayes, 2019, p. 2). However, the Anthropocene hypothesis has not been formally accepted by the International Commission on Stratigraphy (ICS), the organism considered the official arbiter for the identification of units of geological time, yet. The Anthropocene Working Group (AWG), within the ICS, is the team in charge of gathering all available empirical evidence to examine, expand, refine, and test the Anthropocene hypothesis. With their conclusions, the ICS will eventually decide whether the Anthropocene hypothesis should be accepted or rejected.[12]

The process by which reasons are being offered for and against the ratification of the Anthropocene hypothesis can be described, using the philosophy terminology here introduced, as appraisals of acceptance. The resolution of these appraisals is still unsettled today. The main reason why this is so is that geological epochs have been traditionally defined based on stratigraphic evidence, that is, traces on the lithosphere, or the rigid rocky outer layer of the Earth. So the ICS would only be willing to accept the Anthropocene hypothesis if comparable stratigraphic evidence to the one employed for the identification of previous geological epochs –like the Pleistocene– is found on rock, glacier ice or marine sediments (Zalasiewicz et al., 2019, p. 3; Lewis & Maslin, 2015, p. 171).[13] However, at present there isn't for the ICS an unmistakable, datable marker documenting a global change that is recognizable in the stratigraphic record (Lewis & Maslin, 2015, p. 173). While many research teams, including the AGW, have advanced arguments for the acceptance of the Anthropocene hypothesis, others have given reasons against it, for instance for being a hypothesis "analytically flawed, as well as inimical to action" (Malm & Hornborg, 2014, p. 62; see also Santana, 2019).

At any rate, that human activity is leaving a pervasive signature on Earth is widely recognized by the scientific community. It is also widely accepted that traces of human activities are found at the interface of atmosphere, hydrosphere, biosphere, cryosphere and pedosphere, if not so clearly on the lithosphere (Bobadilla, 2022; Waters, Zalasiewicz, Summerhayes, & Wolfe, 2016). Whether or not the Anthropocene hypothesis eventually passes the appraisals of acceptance to which it is currently subjected, an issue that appears to be in the hands of the ICS, the hypothesis has undoubtedly passed appraisals of pursuit. Crutzen and Stoermer's hypothesis was still an "improvised proposal" in its original formulation, in words of the AWG (Zalasiewicz et al., 2019, p. 1). Yet, it soon began to be discussed in publications in different fields in the natural and social sciences, and the popular usage of the term rapidly escalated (Ibid.; Lewis & Maslin, 2015, p. 171). Its rapid spread through the ESS (Earth System science) community was especially striking (Steffen, Leinfelder, Zalasiewicz, & Schellnhuber, 2016). In the geological community, it would be the Stratigraphy Commission of the Geological Society of London the first in explicitly suggesting that the hypothesis "had merit and *should be studied further* with respect to any potential formalisation" (Zalasiewicz et al., 2019, p. 2; my emphasis). These evaluations, appealing to the "merit" and the "potential" of studying the Anthropocene hypothesis, can be read as judgments on the pursuitworthiness of the hypothesis. Also, prestigious scientific journals like *Nature* and *Science* have publicly encouraged the examination of the Anthropocene hypothesis (Lewis & Maslin, 2015; Waters et al., 2016; in Bobadilla, 2022), and at least three journals focusing on the topic have launched in the last years: *The Anthropocene*, *The Anthropocene Review* and *Elementa* (Lewis & Maslin, 2015, p. 171).

The Anthropocene hypothesis is a highly creative hypothesis. It is creative insofar as it exhibits a genuine type of pursuitworthy novelty that illuminates overlooked aspects of the epistemic tradition in geological research, and, in virtue of doing so, articulates a promise about the future benefits that adopting it could have for the field. Specifically, it brings to the fore standardized past assumptions on how geological epochs are to be determined, turning them into open questions that could now be either revised or further substantiated. For instance, considering that there is a vast array of available geological signals, the Anthropocene hypothesis calls attention to the way in which GSSPs (Global Boundary Stratotype Section and Points) –the global markers of an event that define the beginning of a geological unit in stratigraphic material – have been delineated until now (Zalasiewicz et al., 2019, p. 34; Lewis & Maslin, 2015, p. 173). An invitation is set now to reflect on the consequences of adopting a more flexible identification of GSSPs, as well as of incorporating new stratigraphy proxies to the investigation (like the suggestion by Gałuszka and Migaszewski [2017] to use industrial glass microspheres as age markers in sediments; in Zalasiewicz et al., 2019, p. 286). Also, the Anthropocene hypothesis has brought to the fore the possibility of joining forces with other research communities for the study of geological phenomena in a way it was not previously done. For example, it has highlighted the prospective benefits of collaborating with the archaeological community, since their artefacts/technofossil techniques could assist in stratigraphic tasks in complementary ways, especially concerning the study of artificial grounds (Zalasiewicz et al., 2019, pp. 35, 286).

At a more historical level, the creativity of the Anthropocene hypothesis can be read as a claim of authority of some portions of the epistemic tradition in the Earth sciences (Carroll, 2010, p. 68). Like in the previous example in paleontology, where Huxley's 19th-century terminology was revivified, the Anthropocene hypothesis revivifies early human-based geological time units proposed since the 18th century. In 1778, Comte de Buffon defined the seventh and last epoch of the history of the Earth as a human epoch, in analogy with the seven-day creation. In 1830, Charles Lyell argued that the "Recent epoch" of the Earth had to be defined on the basis of the emergence of civilization as well as the end of the last glaciation. In 1854, Thomas Jenkyn described a "human epoch" based on the fossil record that would most likely be found in the future (Rudwick, 2005; in Lewis & Maslin, 2015, pp. 172–173; Zalasiewicz et al., 2019, pp. 4–5). More directly, the concept of "Anthropocene" was

[12] See the most up-to-date state of the art on the agreements of the AWG to be presented in front of the ICS on their Website <http://quaternary.stratigraphy.org/working-groups/anthropocene/>. Retrieved on December 2022.

[13] The acceptance of the identification of the start of the Holocene epoch was also a matter of controversy in the geological community. In some proposals, the start of the Holocene corresponds to a transition from a glacial phase into a warming interval, accompanied by sea-level rise, that took place ~11,700 years ago (Waters et al., 2016, pp. 1–2). However, as Walker, Johnsen, Rasmussen, and Schwander (2008, p. 264) argue, "the Pleistocene–Holocene boundary has proved difficult to define in conventional Quaternary depositional sequences [… and a] precise dating of the boundary has also proved to be problematical".

retrieved by Crutzen and Stoermer from the work of the Ukrainian geologist V. I. Vernadsky in the 1920s. Vernadsky's notion of "noösphere", proposed to designate the increasing role played by humankind's brainpower on the environment, was later translated into English as "anthropogene or anthropocene" (Lewis & Maslin, 2015, p. 173; Crutzen & Stoermer, 2000, p. 17). In short, the pursuing of the Anthropocene hypothesis promises the opening of a path of inquiry that would deepen our understanding of humans as non-passive observers of Earth's functioning, as it was already advanced, but not sufficiently spelt out, by several other geoscientists in the past centuries (Lewis & Maslin, 2015, p. 178).

The three historical examples sketched here aimed to illustrate the potential of creative hypotheses to negotiate the transition between some parts of the past of an inquiry and the expected future developments of a research programme. Given such potential, the three hypotheses discussed were considered worth further investigating by their respective scientific communities. For the recent examples –Baron et al.'s hypothesis and the Anthropocene hypothesis–, we cannot assure, though, that these hypotheses will continue to be considered worthy of pursuit some time from now. It is possible that, if they end up being formally accepted by the community of, respectively, paleontologists and stratigraphists, and hence endorsed as well-established pieces of scientific knowledge, the interest in continuing to examine, test, refine them, and explore their consequences decreases. But this doesn't need to be so: not only young or emerging hypotheses can be regarded as worthy of further pursuit. Well-developed, accepted hypotheses can also be pursuitworthy in cases when scientists are interested in their further heuristic capacities (Seselja & Strasser, 2013, p. 9). And even if these hypotheses are eventually rejected, scientists may still want to ask themselves whether certain elements in them could be worthy of further inquiry (Ibid.; see also Chang, 2011).

5. Concluding remarks

In defining the value condition of creativity as pursuitworthiness, this article brings together the contemporary philosophical literatures on creativity and pursuit, developed in a separate manner until now. The consequence of doing so is twofold. One, it helps improve the most common definition of creativity (the standard view) in the current philosophy of creativity, by narrowing down its value condition to only a prospective type of value (i.e., pursuitworthiness). Two, the debate on pursuit in current philosophy of science can be enriched by calling attention to cases of scientific creativity, since these exemplify particularly well how values for pursuit and values for acceptance can come apart in scientific practice (Seselja & Strasser, 2013, 2014). We have seen throughout the paper how being creative might not give scientists reasons to accept a hypothesis as true or a well-established piece of knowledge, but it gives them reasons to think that the further inquiry into it could contribute to making a research program advance in a fertile way.

Indeed, the selected historical examples in this paper are cases of creative hypotheses that were *in fact* pursed by a certain scientific community, at least for some time. This is an important observation, since being worthy of further pursuit is different from being in fact pursued. Being in fact pursued depends on the epistemic qualities exhibited by a hypothesis, but also on the qualities of other hypotheses existing in the field at the same time, and on non-epistemic considerations that scientists need to make regarding the limited resources of the research community at a certain point (of time, money, energy, cognitive capacity) (on this point, see Peirce, 1932–58, p. 5.602; Achinstein 1993, p. 93; McKaughan 2008, p. 457; Nyrup 2015, p. 753).

The proposal in this paper was limited to suggesting that being creative makes a scientific hypothesis worth further pursuing, and also contributes to making the hypothesis more apt to be *in fact* pursued –but it does not guarantee the latter. A further suggestion that arises throughout the article, especially after examining the historical cases, is that in contexts characterized by cognitively stagnant situations or the exhaustion of the most predominant theory or method in a scientific community, creative hypotheses might have better chances to be in fact pursued. In those contexts, seeing anew parts of the epistemic tradition, in a way that affords clarity about how to open up paths of inquiry based on them, can turn out to be particularly beneficial for the advancement of the field.

Creative instances are neither valuable in general nor valuable in the sense of possessing features that make them epistemically successful in their current state (i.e. truthful, useful, appropriate). They are valuable only in the sense of being rationally worthy of further investigation, analysis, and exploration by epistemic agents, because they might at a later stage possess some of those epistemic virtues. The revised version of the standard view of creativity advanced here counteracts the criticism advanced by philosophers like Hills and Bird (2018, p. 18), who argue that including a value condition in the definition of creativity means endorsing an "unreflective approval" of creative instances, as well as accepting their truth or truth-conduciveness. Neither of those assumptions are involved in the definition of creativity proposed here: being creative is being epistemically valuable in a restricted sense, which does not include being (necessarily) truthful nor being (necessarily) accepted by an epistemic community.

It remains to be seen if the revised definition of creativity presented here, focused on scientific creativity, could be expanded to the debate on creativity in aesthetics and other domains. My guess is that, seeing how close Carroll's (2010) account of artistic creativity –discussed in Section 3– is to the proposal on scientific creativity advanced here, there are good prospects for thinking that a more comprehensive definition of creativity, encompassing cases in different domains, where pursuitworthiness is the value occupying central stage, is possible.

Declaration of competing interest

None.

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