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RESEARCH ARTICLE

Breakthrough results in astrobiology: is 'high risk' research needed?

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Abstract
Astrobiology is a scientific endeavour involving great uncertainties. This could justify intellectual risk-taking associated with research that significantly deviates from the mainstream, to explore new avenues. However, little is known regarding the effect of such maverick endeavours. To better understand the need for more or less risk in astrobiology, we investigate to what extent high-risk / high-impact research contributes to breakthrough results in the discipline. We gathered a sample of the most impactful astrobiology papers of the past 20 years and explored the degree of risk of the research projects behind these papers via contact with the corresponding authors. We carried out interviews to explore how attitudes towards risk have played out in their work, and to ascertain their opinions on risk-taking in astrobiology. We show the majority of the selected breakthrough results derive from endeavours considered medium- or high-risk, risk is significantly correlated with impact, and most of the discussed projects adopt exploratory approaches. Overall, the researchers display a distribution of attitudes towards risk from the more cautious to the more audacious, and are divided on the need for more risk-taking in astrobiology. Our findings ultimately support the explicit implementation of a risk-balanced portfolio in astrobiology.

Contents

Introduction

Astrobiology, mostly defined as the study of the origin, evolution, distribution and future of life in the universe (Horneck *et al*., 2015; NASEM, 2019), covers lines of research that have a high degree of uncertainty. The historical origin of life on Earth may remain a complete mystery and extraterrestrial life forms, if they exist, might have nothing to do with what has been imagined so far.[1] Given this context, it is hard to draw a solid paradigm in this still nascent discipline and it is tempting to engage with research that significantly deviates from the scientific mainstream. However, this strategy is associated with a certain risk-taking. Indeed, the number of unknowns in some areas of the field is so large that one might as well wonder whether taking shots in the dark would not be a complete waste of resources without any advancement in knowledge.

In science, the main justification for carrying out such risky research is to aim for high impact, so much that high-risk research often actually stands for high-risk / high-impact (or high-reward) research (abbreviated here HR/HI). In recent years, significant efforts have been made to promote HR/HI research by various governments through funding agencies, with the need to make the criteria for defining, and therefore funding, HR/HI research more explicit. However, the definitions of risk on the one hand, and of impact on the other hand, are rarely explicitly stated. For example, the US 'America COMPETES Act' of 2007, which aims to promote such research, defines it as being able to '(1) meet fundamental technological or scientific challenges; (2) involve multidisciplinary work; and (3) involve a high degree of novelty'. Funding agencies such as the ERC, HFSP or NIH particularly encourage the submission of HR/HI projects but there are tensions with the 'feasibility' of the project, a criterion explicitly apparent for applicants to ERC grants for example. Surprisingly, risk, which naturally translates into a significant probability of failure, is very rarely addressed in this way, with a few exceptions such as the Exploration programme of the Government of Canada, which acknowledges that 'it is expected that a number of funded projects will fail to meet their objectives'.[2] It is joined by the OECD report 'Effective Policies to Foster High-Risk/High-Reward Research' which considers that an essential criterion of such research is that it 'carries a high risk of not realizing its full ambition'.

Recent studies to assess the impact of high-risk research (mainly associated with the number of citations of the related papers) tend to confirm their usefulness on a large scale. For example, (Wang *et al.*, 2017) developed a novelty index, supposedly representative of high-intellectual-risk projects, which they applied to some 800 000 articles in the Web of Science database. They show that highly novel papers are more likely to be among the top-cited papers but that they also are more likely to appear among the least cited ones, which clearly illustrates the ambivalence of such research. Applying the same novelty indicator to articles published between 2005 and 2017 (in the Scopus database), (Machado, 2021) confirms these results and shows that the most novel articles are, nevertheless, on average more cited. To maximize the overall effectiveness of the scientific research, simulations suggest that a mixed population of conservative and maverick researchers, respectively taking few and many risks, would foster major breakthroughs (Weisberg and Muldoon, 2009; Thoma, 2015; Avin, 2019). Different funding agencies have also started to evaluate the impact of the HR/HI projects they fund. For example, through bibliometric analysis and expert reviews of proposals and publications from their funding, the National Institutes of Health (NIH) and the European Research Council (ERC) have found that projects judged to be high-risk are positively associated with greater impact, but caution that these are only preliminary results at present (ACD, 2019; ERC, 2021).

Managing HR/HI research is a major issue in astrobiology as it is an extreme discipline in terms of the costs involved (e.g. space missions), the impact of the discoveries sought (e.g. extraterrestrial life), and the vastness of uncharted waters (at the scale of the universe).[3] Today, there is a disagreement whether HR/HI research is supported enough in the field, and this debate is particularly active at

[1]On the contrary; other lines of research within the scope of astrobiology are now well settled, like the studies of the causes of the Cretaceous-Paleogene extinction event that were the subject of long and intense debates and uncertainties (Vickers, 2022, p. 164).

[2]See https://www.sshrc-crsh.gc.ca/funding-financement/nfrf-fnfr/exploration/2022/competition-concours-eng.aspx, 2022 Exploration Competition, Review Process.

[3]This statement obviously depends on the research objectives and on the different constituent disciplines of the field, but tackling the search for extraterrestrial life, which is the *raison d'être* of astrobiology, certainly implies high cost, impact and uncertainty.

NASA, the first funder of this complex and international field (Persson *et al.*, 2018). In a 2018 presentation, Astrophysics Division Director of NASA Paul Hertz stated that HR/HI projects are under-submitted but selected at a higher rate by NASA than those considered medium or low risk. This finding, based on a survey of the 2017 ROSES call for projects (Research Opportunities in Space and Earth Sciences), is meant to discredit *'the widespread perception that NASA peer review, and possibly all peer review, is hostile to truly innovative, high-risk research and technology development proposals'*[4]. However, Vickers (2020) shows that the result of this survey can be explained by an overly broad definition of risk that would for instance include research with scant preliminary data that are yet very safe, like the observation of any newly detected phenomena. One year after this presentation, the NASA Deputy Associate Administrator for Research, Michael New, reaffirmed the results of the 2017 ROSES survey while maintaining that *'nevertheless, the community believes that NASA peer review is hostile to HR/HI research'*.[5] In order to *'begin to reset this belief'*[6], the testing of a new process is announced where applicants must write a short description of why proposed research would be HR/HI, and may be selected on their own or be given a second chance. This test is still being conducted and NASA HQ continues to actively promote the submission of HR/HI proposals to this day, insisting on the intellectual, even reputational, nature of the risk.[7] The debate within NASA indicates that many administrators and researchers would like to allocate more resources to risky projects, but to our knowledge their impacts in astrobiology have never been studied.

In order to assess the actual contribution of HR/HI research to advances in astrobiology, here we look at the recent history of the discipline and seek to identify whether the breakthroughs come from risky research. By contacting the corresponding authors of a sample of the most cited papers in the field of the past 20 years, we gather the stories of the related research and opinions on risk management in astrobiology. By doing so, we seek to understand if the success stories of astrobiology can shed light on how to manage risk. Here, we are interested in what we could call '*intellectual risk*', where we mean research for which *the probability of meeting intended objectives is unknown, or known to be significantly below average, because of a heterodox approach or an approach that is contrary to prevailing viewpoints.* This makes intellectual risk different from technological, economic or (bio)safety risks otherwise well documented in the context of astrobiology and space exploration (NASA administrator's symposium, 2004; Lorenz, 2019).[8] Thus, this paper doesn't address high-risk research as a whole but some of its facets linked to the degree of remoteness from the existing mainstream schools of thought. Furthermore, we include uncertainty as a component of risk, meaning that we do not subscribe here to the distinction often made in decision theory that uncertainty represents a situation in which probability cannot be measured and risk a situation in which it can (Knight, 1921).

This article is organized as follows. In section 'Do the most impactful astrobiology results come from low, medium, or high-risk research?' we show to what extent the most high-impact scientific papers reporting astrobiology-related results come from low-, medium- or high-risk research, according to contacted corresponding authors. Based on interviews with some of these authors, section 'Characterisation of the intellectual risk behind the breakthroughs' summarizes the main approaches that led to the breakthroughs and refines the concept of intellectual risk. Section 'Attitudes to risk, from the laboratory to the astrobiology community' reports different ways of managing intellectual

[4]Paul Hertz's 22/10/2018 NASA presentation on ROSES 2017, slides 23–25.

[5]Michael New's 06/12/2019 NASA presentation on ROSES 2017, slides 9–10.

[6]Note that this belief is yet shared by NASA's Science Mission Directorate. This is evidenced by Thomas H. Zurbuchen (NASA Associate Administrator) who acknowledges that 'the peer review process used to make investment decisions can inadvertently discourage innovative concepts'. "Explore Science 2020 – 2024 – A vision for Science Excellence", p19, see https://smd-cms.nasa.gov/wp-content/uploads/2023/04/2020_2024_Science.pdf.

[7]AbSciCon 2022 - NASA Town Hall with Dr. Mary Voytek, from 2min42, https://www.youtube.com/watch?v=EHmWbgiPU88.

[8]This specification is important as we are interested here in understanding how researchers try to answer astrobiology-related questions in the face of great uncertainties like the origin of life and its detection in the universe. Including technological risk would deviate this paper from its objective since it would include research that is not tackling astrobiology-related issues but that is tackling technological issues which may help astrobiological research.

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risk and outlines opinions of the interviewees on risk-taking in astrobiology. Section 'Concluding thoughts and recommendation' concludes, drawing lessons which may influence the future directions and methodologies of astrobiology.

Do the most impactful astrobiology results come from low, medium, or high-risk research?

In this section, we seek to understand to what extent the most impactful results in the recent history of astrobiology have come from more or less risky research. We present the results of an email campaign addressed to the corresponding authors of a selection of 55 articles sampling the most impactful results in astrobiology over the last 20 years (see SI for details on the selection). We here associate impact with number of citations. However, we recognize the limits of this criterion (Penfield *et al*., 2014; Serenko and Dumay, 2015; Machado, 2021) which is nonetheless a useful indicator of scientific impact (Sinatra *et al*., 2016).

Once the selection was made, an email was sent to the 55 corresponding authors including the following questions about the selected papers: 'Did you ever consider this research to be "medium risk" or "high risk", in the sense of it being "heterodox", or simply contrary to prevailing viewpoints?'[9] and 'Did your own results take you by surprise?' (see SI for the full email). After a follow-up, we received 40 responses which allowed us to divide the papers into three categories 'low-risk', 'medium-risk' and 'high-risk', spanning the 6 astrobiology topics identified earlier (see Table 1 and SI for details).[10]

In order to compare the impacts between the different articles, we have associated each article with its Field-Weighted Citation Index (FWCI), available within the Scopus database. It corresponds to the ratio of the citations received by an article and the average number of citations received by a paper from a similar publication, in the same year and in the same field of research. FWCI = 1 indicates that the publication is cited like the world average, and FWCI of more or less than 1 respectively indicates it is being more, or less, cited than the world average. We choose this index to compare the impact of papers from different disciplines and years of publication, unlike the absolute numbers of citations.

Based on the answers of the authors, research projects are considered as low-, medium- and high-risk, in almost the same proportions, meaning that around two thirds of them are said to contain some risk-taking (see Fig. 1a). However, it is important to note that this result does not in itself reveal whether or not the distribution of risk depicted in Figure (a) is typical from only *high*-impact research in astrobiology, or is similar to any (random) sample of astrobiological articles. To assess this hypothesis, we did a similar (yet smaller) survey with low-impact astrobiology paper which shows a strong bias towards low-risk (see SI for more details). This is in line with the idea that high-risk research may be positively associated with high-impact papers, which is in line with the recent studies (ACD, 2019; ERC, 2021; Machado, 2021).

This trend is also visible when looking at papers' impacts (based on the FWCI). Indeed, the top 10% most impactful papers come from high-risk research and the more the research is deemed risky, the more the average impact is high: risk and impact are significantly correlated (Fig. 1d). Moreover, the dispersion of papers' impact for each risk category increases with the estimated risk. This shows a relative unpredictability, in terms of impact, of research deemed risky by the authors, which may be typical of high-risk research as shown by (Wang *et al*., 2017; Machado, 2021). And finally, most of the authors were surprised by their results but no significant correlation has been established with risk and impact in the sample (Figures S2 and S3).

The remarkably very similar proportions of research judged to be low, medium and high risk reflect a diversity of risk-taking behind breakthroughs in astrobiology, which probably reflects the range of

[9]Since we exclude technological failures, financial and other extra-scientific issues such as physical damage to humans or equipment in our previously stated definition of intellectual risk, this characterization is a logical outcome.

[10]It is important to note that these results correspond to subjective a posteriori statements of contacted corresponding authors. Given the division of labour in scientific research, these estimates may differ from one individual to another within the same research project. However, since the corresponding authors are usually the principal investigators, we suppose that they are best able to assess the research projects that resulted in the selected papers as a whole.

Table 1. *Summary table of the selected papers for the quantitative and qualitative studies of the paper*

Papers	Risk	FWCI	Results	Author interviewed
Glavin *et al.* (2010)	Low	3,25	Surprising	
Goesmann *et al.* (2015)	Low	8,44	Not surprising	Yes
Belloche *et al.* (2013)	Low	3,23	Not surprising	Yes
Brennecka *et al.* (2011)	Low	2,07	Surprising	
Methé *et al.* (2005)	Low	2,83	Surprising	
Vaidya *et al.* (2012)	Low	4,14	Not surprising	Yes
Pierrehumbert and Gaidos (2011)	Low	3,79	Not surprising	
Segura *et al.* (2010)	Low	4,57	Surprising	
Lammer *et al.* (2003)	Low	2,55	Not surprising	
Hsu *et al.* (2015)	Low	7,21	Surprising	
Sandford *et al.* (2006)	Low	4,76	Surprising	
Altwegg *et al.* (2016)	Low	9,81	Surprising	
Pinheiro *et al.* (2012)	Low	9,13	Surprising	
Raymond *et al.* (2004)	Low	4,35	Surprising	
Lincoln and Joyce (2009)	Medium	7,53	Not surprising	
Love *et al.* (2009)	Medium	5,31	Surprising	
Chan *et al.* (2011)	Medium	8,25	Surprising	
Waite *et al.* (2009)	Medium	3,99	Surprising	
Postberg *et al.* (2009)	Medium	2,77	Surprising	Yes
Kopparapu *et al.* (2013)	Medium	19,97	Surprising	Yes
Alexander *et al.* (2012)	Medium	6,09	Surprising	
López-García *et al.* (2001)	Medium	3,74	Surprising	Yes
Charbonneau *et al.* (2009)	Medium	9,67	Surprising	
Mansy *et al.* (2008)	Medium	5,32	Not surprising	
Öberg *et al.* (2009)	Medium	3,79	Not surprising	
Martín-Torres *et al.* (2015)	Medium	14,28	Surprising	Yes
Chapelle *et al.* (2002)	Medium	2,57	Surprising	
Powner *et al.* (2009)	High	8,31	Surprising	
Callahan *et al.* (2011)	High	4,09	Surprising	
Dodd *et al.* (2017)	High	13,04	Surprising	
Dupont *et al.* (2012)	High	11,69	Surprising	
Borucki *et al.* (2010)	High	19,97	Surprising	Yes
Petigura *et al.* (2013)	High	12,76	Surprising	Yes
Waite *et al.* (2017)	High	10,03	Surprising	
Muñoz Caro *et al.* (2002)	High	3,45	Surprising	
Mumma *et al.* (2009)	High	7,58	Surprising	
Hedges *et al.* (2015)	High	29,58	Surprising	
Beal *et al.* (2009)	High	4,98	Not surprising	
Weiss *et al.* (2016)	High	22,99	Surprising	Yes
Anglada-Escudé *et al.* (2016)	High	23,99	Not surprising	

The levels of risk, associated with research 'being "heterodox", or simply contrary to prevailing viewpoints', and the surprise regarding the results are based on the answers of the corresponding authors to the emailed questions (see SI). The FWCI is a measure of the impact of a paper corresponding to the ratio of the citations received by an article over the average number of citations received by a paper from a publication published in the same year and in the same field of research (obtained via Web of Science). Interviews of the subset of corresponding authors are described in parts 3 and 4.

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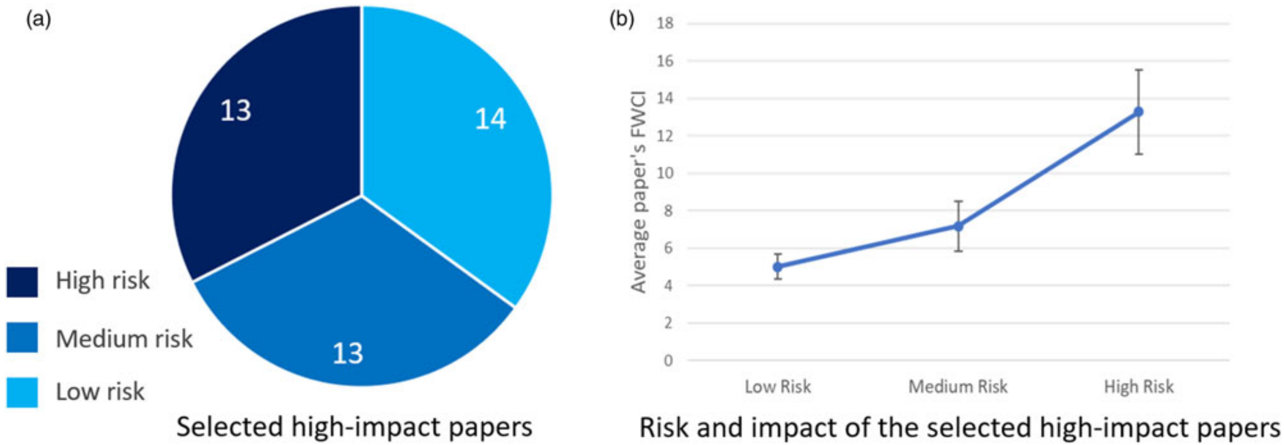

Figure 1. *(a) Distribution of risk assessment among high-impact astrobiology-related papers. Graph showing the proportion of high-impact papers the authors contacted considered to be from low, medium, or high-risk research. (b) Average impact of papers from each risk category. Graph showing the average impact of articles, quantified by their FWCI (Field Weighted Citation Impact), as a function of the author's risk estimate. The error bars correspond to the standard error. Risk and FWCI are correlated, with the correlation coefficient r calculated as* $R^2 = 0.265$ *and the p-value calculated as* 6.92×10^{-4}.

attitudes in the scientific community towards novelty and intellectual risk (Vickers, 2022, p. 113). However, the fact that the majority of research projects (in our sample) are at least medium risk, that riskier research is on average more impactful and that the most impactful papers come from risky research does suggest that high-risk research is particularly suitable to achieve the most groundbreaking advances, possibly not reachable by more conservative routes.

So, does this finding justify advocating more intellectual risk in astrobiology? At first sight, favouring risky projects would increase the number of very high impact results. However, it does not take into account the resources required to set them up. Consequently, these results cannot tell if HR/HI research has a higher or lower scientific impact per dollar (and unit of time) invested. This consideration is important for research policies since expensive HR/HI research would reduce the number of lower risk research projects carried out, thus possibly reducing the overall impact from the field as a whole. Focusing on more high-risk projects can certainly increase the chance of obtaining high-impact results, but it could also reduce the number of lower impact results, which are cumulatively responsible for the sustained incremental advancement of scientific knowledge. Moreover, this analysis does not consider high-risk projects that have not led to any publication due to a lack of results. It is expected that this number would be higher than for lower risk projects. Indeed, the Web of Science database analysis realized by (Wang *et al.*, 2017) shows that highly novel papers (associated with high-risk research) are more likely to be among the least cited papers, thus maybe more likely to have no impact worth mentioning in a paper.

These results show that low-, medium- and high-risk research do all contribute to the most impactful findings in astrobiology of the last 20 years but also that risky research can be more impactful. However, due to the heterogeneity of our sample studied, any speculative generalization to the whole field of astrobiology, and beyond, must be done cautiously. As a reminder, we obtained our results by asking the authors whether they, subjectively and a posteriori, considered the research that led to the selected articles as '"medium risk" or "high risk", in the sense of it being "heterodox", or simply contrary to prevailing viewpoints'. This formulation allows for an emphasis on the intellectual dimension of risk but also leaves room for interpretation. How do researchers interpret the notion of intellectual risk applied to their work? What are the practical differences between more or less risky approaches? Is there a common way of doing things that would explain their success? To better understand the similarities and differences of more or less risky approaches that led to these results, the following section gives a qualitative account of the projects carried out by the corresponding authors of ten selected papers.

Characterization of the intellectual risk behind the breakthroughs

This section is an interview-informed conceptual analysis of intellectual risk and its associated research approaches. Based on qualitative interviews with ten authors, we present here the reasons why they characterize the research projects in question as low, medium or high risk. The authors were chosen among the corresponding authors of the papers from the sample of high-impact papers used in the previous section. They constitute a varied sample in terms of risk level (4, 3 and 3 authors of articles considered low, medium and high risk) and in terms of topics.[11] Each author was asked the same questions, in the same order, but with the option to expand on a particular topic based on their personal experience. The interviews were conducted and recorded via a video communication platform and the developments below (parts 3 and 4) have been constructed from summaries of the interviews and quotes to illustrate some of the opinions.[12] Through the accounts collected, we show which types of approaches are characteristic of higher or lower risk and generate impactful results.

[11]The interviewed authors are respectively 2, 1, 1, 1, 4 and 1 to be associated with topics 1 to 6, based on (NASA, 2015) classification.

[12]For more information on semi-structured interview methods, see (Fylan, 2005; Kallio *et al.*, 2016), and see (Janzwood, 2020) for an example applied to scientific researchers.

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During the interviews, we asked the researchers to summarize the research projects that led to the selected articles, focusing on the intellectual risk taken or not throughout the research process. Papers use bioinformatics to infer the physiology and environment of last universal common ancestor (LUCA) (Weiss *et al*., 2016), or reveal an unsuspected diversity of micro-organisms in Arctic deep waters (López-García *et al*., 2001). Another paper reports the establishment of RNA systems as a possible step in the origin of life (Vaidya *et al*., 2012). The search for liquid water is represented here by articles demonstrating the presence of a liquid water ocean under the ice of Enceladus (Postberg *et al*., 2009) as well as a favourable environment for liquid water on Mars (Martín-Torres *et al*., 2015). Other reported findings are related to the detection of complex organic molecules in an interstellar cloud (Belloche *et al*., 2013) or on a comet (Goesmann *et al*., 2015). In addition, an article reveals a new estimate of the size of habitable zones around stars (Kopparapu *et al*., 2013) and finally, other papers report a diversity of exoplanets (Borucki *et al*., 2010) as well as a prevalence of Earth-size exoplanets in the habitable zone of sun-like stars (Petigura *et al*., 2013).

In what follows, we group the research projects by type of approach, roughly ranging from the least risky to the most. These types of approaches, which may overlap, help to highlight the major causes of the degree of risk involved. As a reminder, only intellectual risk as defined in the introduction is tackled here, unless stated otherwise. Thus we focus on research challenging astrobiology-related mainstreams, putting aside technological risk that is more oriented to engineering issues.

Low risk

Projects were considered low risk for various reasons, the most obvious of which is that (i) a specific outcome is expected with near certainty. However, the presence of an expected specific outcome is not necessary for a project to be considered low risk (ii) when it is a question of exploring an unknown area where the results will most probably be significant, even if not necessarily expected in their specificity.

(i) Specific results expected

The typical case of a low-risk project is one in which researchers have every reason to believe that it will lead to a specific result, that is, a phenomenon beforehand precisely described. This is the case for the development of replicating RNA networks (Vaidya *et al*., 2012) where the formation of the networks was expected given the already discovered characteristics of the RNAs in question. However, according to the interviewee, this project still involved a dimension of high intellectual risk. Indeed, the interpretation made of the results supports a challenging theory that deviates from a dominant line of research in the field of the origin of life. Thus, even if the author considers the overall project as being low risk, writing this paper therefore entails an intellectual risk of a different kind than that of not obtaining interesting results, i.e. the risk of having the main interpretation of the results that make the paper valuable invalidated.

(ii) Exploratory research with expected unspecific results

Three projects can be classified as exploratory research and low risk. They all investigate unexplored areas of interest, and their leaders are convinced they will produce significant results if everything goes well technologically, regardless of their specificities. These projects are guaranteed to find out some of what is hidden in a non- or underexplored area of interest and no *specific* data are needed to gain high scientific significance. Two projects concern the detection of organic molecules on the surface of a comet (Goesmann *et al*., 2015) and in an interstellar cloud (Belloche *et al*., 2013), some of which are of prebiotic interest. The third one concerns the analysis of weather variations on the surface of Mars and its implications in terms of the presence of liquid water (Martín-Torres *et al*., 2015).

The author of (Goesmann *et al*., 2015) is clear about his expectations: *'I don't care if it's life or if it's not, but I want to know what it is*'. The results obtained reveal organic molecules, some of which are of prebiotic interest, but according to the author, they are '*boring*' because they are not very surprising in view of

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the knowledge already established. The reason for the paper's high impact is obvious to him: it is the first mass spectrometry analysis performed *in situ* on the surface of a comet. The mere novelty of the location of the analysis is therefore sufficient to generate high impact. The same remark applies to (Belloche *et al.*, 2013) which stems from the exploration of a new spectral region from an interstellar cloud to identify complex organic molecules. This '*logical next step of an incremental approach*' was not risky. Other organic molecules had already been discovered there and more advanced technology and modelling guaranteed the observation of at least some new complex organic molecules. Moreover, the paper's high impact is also due to its usefulness since it provides a valuable database for future research.

The guarantee of significant results was also assumed for the experiments conducted by the REMS (Rover Environmental Monitoring Station) of the Curiosity mission on Mars (Martín-Torres *et al.*, 2015). One of its objectives was to measure fluctuations in pressure, temperature and humidity at ground level and whatever results were found could only be novel and provide valuable information about the Martian weather. However, as for (Vaidya *et al.*, 2012), the author acknowledges a dimension of high intellectual risk at the level of the *interpretation* of the results.[13] Indeed, he asserts that the discovered environmental conditions are favourable to the presence of transient liquid water, whereas the scientific community is divided about the existence of liquid water on Mars.

Medium risk

This section summarizes the two main approaches that were considered medium risk during the interviews. The first approach consists in (iii) exploratory research with unspecific results moderately expected. The second approach described here consists of (iv) updating a pre-existing result with no guarantee of getting significant changes.

(iii) Exploratory research with moderately expected unspecific results

The main difference between this approach and approach (ii) described above is that the researchers were less confident in getting significant results. Two projects can be identified along these lines.

The first project consists of analysing ice grains from Saturn's rings, supposedly originating from beneath the surface of the icy moon Enceladus, via a mass spectrometer on board the Cassini probe (Postberg *et al.*, 2009). No specific results were expected, as the interviewed author says:

> *'I was investigating the composition of these ice grains from Enceladus (…) just because it was fascinating to look at the composition of material that originates from the subsurface of an icy moon.'*

The 'serendipitous' breakthrough is then due to the presence of a peak not expected in the spectrogram. It corresponded to molecules necessarily originating from a liquid water ocean, a question that wasn't directly addressed by the research project.

In the second project, scientists used technology limited to the study of prokaryotes to assess the diversity of eukaryotic microorganisms in the deep waters of the Arctic Ocean (López-García *et al.*, 2001) previously considered non-existent. The breakthrough, comparable to discovering groups as significant as mammals, is therefore due to the application of a technology from one discipline to address a question from another, just as (Weiss *et al.*, 2016) discussed below.

(iv) Updating a result without guaranteeing significant changes

The objective of the project that led to (Kopparapu *et al.*, 2013) was to update an old estimate of the habitable zone, the concentric zone around stars where liquid water can be stable. This was

[13]Taking into account this second dimension of intellectual risk, the author classified his project as medium risk in the email survey. We classify it here as low risk because we focus on the intellectual risk before the results are obtained.

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considered a medium risk undertaking partly because the new result obtained might not have significantly modified the old estimate, which was the set objective.[14] As a matter of fact, the result of this project greatly surprised the researcher and led him to consider a much smaller habitable zone than previously assumed, with the Earth just on the edge. The model in (Kopparapu *et al*., 2013) is now widely used for determining whether a particular detected exoplanet is in the habitable zone of its planet.

High risk

The three projects identified as high risk in the interviews were related to (v) exploratory research with the establishment of an unconventional methodology, carrying a significant risk of not obtaining usable results.

(v) Exploratory research with unconventional methodology

The concept of the Kepler Planet-Detection Mission (Borucki, 2016) is concerned with this approach, the first results of which are published in the selected article (Borucki *et al*., 2010). This mission was based on sending a space telescope mainly to assess via transit photometry technology the number and diversity of exoplanets. The researcher interviewed, who happens to be the main advocate of the project, had been promoting this project to NASA for some 20 years and had to write six research proposals before it was finally accepted. The community was against it because it was convinced it could not achieve its goal for several reasons, including because the technology might break down (technological risk), but also because the methodology might not be appropriate at all for such an exploration even without the event of a breakdown (intellectual risk). The interviewee considers that this project was 'high-risk in every respect', as he puts it:

> *'It was high risk to me because the community said it wouldn't be done, my supervisor felt uncomfortable. It was high-risk to my career. It was high-risk. The missions might not succeed for technical reasons, programmatic reasons, and science reasons. It was high-risk in every respect.'*

A second high-risk project is also linked to the Kepler mission, but only concerns the analysis of some of the data produced. It consisted of developing a data analysis pipeline, supposedly more efficient than the official Kepler mission pipeline, to detect Earth-sized planets orbiting in the habitable zone of their star in the immensity of the data produced (Petigura *et al*., 2013). There was no guarantee that this exploratory project would succeed in establishing a better pipeline than the one already in use. Eventually, the results obtained make it possible to assert that *more than one in five* Sun-like stars harbours Earth-size planets orbiting in its habitable zone.

Finally, the last paper (Weiss *et al*., 2016) shows key features of the physiology and habitat of LUCA, and the interview with the author highlights a grey zone in the interpretation of the 'high-risk' tag. In the interview, the author was reluctant to tag this research as high-risk because it involved very little time and no cost. However, since this consideration does not affect the level of intellectual risk as defined before, and since the author acknowledges a high level of heterodoxy and a poor confidence in generating any interesting results, we choose to classify this research as high risk, in terms of intellectual risk. Even if it was a '*deceptively simple experiment*', the author is not surprised that no team undertook this test because '*microbiologists have better things to do than to work on origin of life*', highlighting the heterodox aspect of his research. The '*beyond expectation*' result was made possible by combining distant disciplines: the tools of computational phylogeny and an origin-of-life question.

[14]Moreover, putting an important effort to provide this update, without the guarantee that it would be of interest for the scientific community, was also considered to add risk to the endeavour.

Different facets of intellectual risks

The accounts of the above breakthroughs give us a better understanding of which types of approach are more or less likely to be associated with intellectual risk and give us a better understanding of what is at stake.

These examples provide an opportunity to refine the concept of intellectual risk and to understand how it fits into the more general notion of risk in science. The latter has been the subject of a recent literature review which points out that 'The road to the study of risk in science is paved with misconceptions and prone to misunderstanding' (Franzoni and Stephan, 2023). The authors of the article set up a framework to better grasp the complexity of the notion. First, they recall that 'the word risk can be used in a preventive sense, to mean the possibility of a clearly negative event, as well as in a speculative sense, to mean the possibility of events that can be both positive or negative', and state that they subscribe to the latter sense ('positive *or* negative') to characterize risk in science. Secondly, Franzoni and Stephan (2023) notes and endorses the interchangeability of the notions of risk and uncertainty in science, pertaining to a quantifiable or non-quantifiable probability in decision theory, noting that the notion of risk in science refers to both quantifiable and non-quantifiable situations (we have made the same association to define intellectual risk). Finally, they establish three main sources of risk (or uncertainty) in science, namely: (1) uncertainty concerning [the nature of] the outcomes; (2) uncertainty concerning the probability of discovery [of a specific outcome] and (3) uncertainty concerning the value of the findings.

Let us take a closer look at how the examples described above support or do not support these distinctions. We can already see that the speculative understanding of risk wrongly leads to situations being considered risky. For example, the corresponding author of (Goesmann *et al*., 2015) considers his research to be low-risk even though he admits a high degree of uncertainty about the outcomes. Indeed, no matter which molecules are detected on comet Chury, the mere fact that it is 'a first' ensures the value of the results. Intellectual risk is thus understood in the preventive sense of risk, associated with a clearly negative outcome, namely the non-achievement of research objectives.

As for (1) uncertainty about outcomes, this may be the source of some risk, but this is not necessarily the case, as the same example from (Goesmann *et al*., 2015) shows. In fact this may frequently be the case in some so-called exploratory research. In this regard, out of the 10 interviews, 8 authors have acknowledged their project as being exploratory. This approach can be defined as: collection of data to determine whether any interesting a posteriori hypotheses might be generated (Jaeger and Halliday, 1998). However, this precedence is in fact not so clear-cut, and while the exploratory research is not theory-driven, it is nonetheless theory-informed (Waters, 2007). A striking common feature of the exploratory research accounts we have gathered is that no specific result is expected for the project to be successful, which allows room for surprise. Interestingly, these projects were considered as low, medium and high risk (for 3, 3 and 2 respectively) showing a relative independence between this approach and the associated risk. The risk-taking is then linked to the subjective estimation of the capacity of an exploratory project to generate interesting results. Thus, a project considered exploratory is not necessarily considered risky because uncertain or low probability of obtaining a specific result may be mitigated by a reasonable probability of achieving at least some, significant, unexpected results.

On the other hand, (2) uncertainty of probability is associated with the estimate of obtaining an outcome without necessarily taking into account the possibility of other outcomes. It seems clear to us that situations with such uncertainty are inherently risky. For example, at the time of the development of the Kepler mission, it was estimated that the telescope might not be able to significantly estimate the population of Earth-like exoplanets (Borucki, 2016). This was not an uncertainty about the frequency of exoplanets that Kepler could infer, but about its very ability to make such an inference.

Now, consider (3) the uncertainty of value. This consideration is contextual, and the main explanation for the absence of valuable results is that the project only leads to results that are considered uninteresting. It occurs when they deviate from the dominant interests of the disciplines concerned. These include negative results and confirmatory results, both of which are, however, essential for the progress of science. Thus, the projects associated with (López-García *et al*., 2001) and (Kopparapu *et al*., 2013) were exposed to different intellectual risks of generating respectively only negative results for the former and confirmatory results or a slight update for the latter. It may also be that positive results fail to generate interest and impact. But a seemingly insignificant result can have important consequences depending on how it is interpreted. This leads us to a facet of intellectual risk that is not grasped in the (Franzoni and Stephan, 2023) framework.

Finally, several accounts mentioned above display another risk related to intellectual risk that we could call 'post-results intellectual risk'. This consists of putting forward an interpretation, or a generalization, that is not in line with a scientific mainstream about results whose reality is not otherwise questioned. (Vaidya *et al*., 2012), for example, involves this dimension of risk by challenging a dominant theory of the origin of life. Putting forward some interpretations can be risky for the career and reputation of the researchers involved but can also have much greater global repercussions that we might call political risks. For example, asserting that the results presented in (Martín-Torres *et al*., 2015) support the presence of liquid water on Mars, not only goes contrary to the prevailing viewpoint stating its absence, but also put the entire Curiosity mission at risk for planetary protection reasons.

Attitudes to risk, from the laboratory to the astrobiology community

Distribution of attitudes to risk at the level of the scientist's overall research

The described research projects do not come out of nowhere but emanate from broader research. Indeed, scientists interviewed are involved in a multitude of projects of varying degrees of risk that should be representative of their attitudes to risk. This section sketches out how they manage intellectual risk in their laboratory, which is the breeding ground for the breakthroughs described, and ends by transcribing their main opinions on the current risk balance in astrobiology. As in the previous section, we present the approaches from the most risk averse to the least risk averse as reported in the interviews.

Avoiding risk

Half of the researchers explicitly acknowledge aiming at low-risk projects. As they are often the head of a team, they express a moral obligation towards its members to make them generate indispensable results for their career, as metaphorically expressed by one scientist:

> *'Most of my research activity is alimentary papers. (…) If you have lots of students and postdocs, you have to feed them, by feed them I mean help them to publish papers because they need them.'*

Another researcher extends this moral injunction to society as a whole:

> *'I have some responsibility to society. Science is a luxury that society affords itself, they give us money to do what we want and even if I had unlimited resources, I would try not to waste them. I would try to use them in a way to get the greatest advances fastest.'*

Several strategies were mentioned to minimize risk and, interestingly, having a heterodox or unconventional approach was explicitly described by two scientists as a strategy for implementing low-risk/high-reward projects. One of them recalls the advice given to him by his former PhD advisor, which he still follows:

> *'Never do what everybody else is doing, for two reasons. Number one, it becomes a race to see who can do the obvious fastest and that promotes actually misconduct. And number two, you don't need to do it, you'll be able to read the answer.'*

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Thus, this positioning in the academic field allows him not only to have less direct competition but also to be much more efficient in the advancement of knowledge. His heterodoxy also proves to be an advantage with regard to the peer review process. Indeed, he considers reviewers to be particularly severe in the fields they master, a tendency that has received empirical validation (Boudreau *et al*., 2016). Having an unconventional approach would then guarantee some ignorance on the part of the reviewers who might be less able to identify possible flaws in the submitted project. Heterodoxy can therefore be a way of reducing the professional risk of not standing out from the crowd enough, as another author also attests:

'I'm driven towards the questions that are a little different. I feel like it's how I try to make my niche, (…) my own space, I'm not just following the path of somebody else.'

There is, however, an ambivalence to this behaviour, which still might carry some risk. As the same researcher admits, there are perhaps good reasons why so few scientists embark on such lines of research:

'I look for things that would be exciting if they get done and nobody else has done them. So that's kind of walking a line between 'nobody does them because they are ridiculous, and they won't work' or because they haven't thought of putting those two ideas together. I'm hoping I'm landing on the side 'that would be exciting because nobody has thought of it'.'

A second risk minimization strategy outlined by a researcher is to conduct a thought experiment before setting up each project. He thinks about all the possible outcomes and how to interpret them, as a way of sorting out the '*good questions*' from the bad ones. According to the researcher, this prior identification is a way to avoid exploratory projects, or fishing expeditions, of which he is very critical. Nonetheless, half of the interviewees identified the very exploratory nature of their research as a means to lower the risk, because of the supposed richness of the unexplored zone. To illustrate this, two of them use the metaphor of the exploration of unknown jungles by the great explorers of the past, which could only reveal fascinating surprises:

'I often say for myself, for the type of work that we do in our team, that we are naturalists of the microbial world. (…) If you're a naturalist it's very easy, you never take risks. You just go out there and nature provides. So, we are exploring the diversity that is largely unknown. So, it's like we were zoologists or botanists in the 16th century, and we went to the Americas, and we discovered new worlds or whole new forests for which most species are undescribed. We feel exactly the same.'

Balancing risk

Recognizing that riskier projects can potentially bring significant scientific return that cannot be achieved by other safer routes, three researchers report implementing risk-balanced project portfolios. This portfolio can be deployed at the individual level as one of the researchers explains:

'I'm conscious of not giving my students projects that have a high probability of failure. One of the things that I try to do is kind of a balance of what you might call the risk portfolio so, you know, working on a few projects where it's a bit of a shot in the dark, we'll see what happens. But not basing the student's entire thesis around that.'

The (López-García *et al*., 2001) paper showing an unexpected biodiversity of eukaryotes is from such a portfolio, for example. This project, characterized as medium risk, was closely associated with two other sub-projects, low and high risk. The low-risk sub-project consisted in looking for

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the diversity of prokaryotes in the same sample, which must at least yield species or gene discoveries. And the high-risk sub-project was to look for eukaryotes with specific features (without mitochondria) to test a *'heretical'* hypothesis about the origin of eukaryotes. But after a year of work, no such specimens were identified, which confirms the risk undertaken.

Such a balanced portfolio can also be spread over the whole team. For instance, a scientist confides that he spares his doctoral students by assigning them projects that he believes are likely to yield interesting new results. On the other hand, he uses the trainees in his laboratory to carry out much more uncertain projects. In this way, he can obtain clues to some phenomena without risking the careers of the trainees who are primarily in the laboratory to learn and who no one expects to publish results. Moreover, his position as a permanent researcher also allows him to take more risks individually without risking his career too much. He therefore focuses part of his time on a number of collaborative projects with uncertain outcomes without endangering the members of his team.

Other researchers claim to be undertaking a range of projects with varying degrees of risk but admit to having no particular strategy. For them, risk assessment is above all integrated implicitly on a case-by-case basis in the preparation of each project, as the following remark shows:

> *'There's not really a systematic approach that I apply for everything. I don't calculate the risk and the reward, then weigh it against each other. It's more intuitively that I decide between 'well this project it's worth pursuing' or 'this idea is worth to invest a week or two in the lab'. So, it's not the calculation it's more scientific intuition, and so far, I think it went pretty well. Maybe, it was just luck, right?'*

Embracing risk

Finally, the researchers who admit to taking the most risk also claim to have no strategy. Interestingly, they even make it a point of honour to say that they will not actually take the risk into consideration if the potential scientific return is significant and the approach rigorous, as one author interviewed stated:

> *"We are living in the golden era of human civilization where we will find out if we are alone in the Universe or not. (...) If something is scientifically sound, no matter the risk, I will do it."*

As for the promoter of the Kepler project, he compares the research he has carried out to jumping off a cliff with a hang-glider. In such moments, it is not healthy to think about possible dangers: 'Risk? I don't worry about risk', he says in response to a question about risk management. However, opposing the consensus of his discipline for almost 20 years had a strong impact on his career by not being promoted during this period and having scientific journals explicitly asking him to stop sending articles, for instance.

Overall, the interviewed researchers show a variety of attitudes to risk with half of them explicitly seeking to minimize it as much as possible and the other half admitting to taking a greater or lesser proportion. This diversity perhaps reflects a balance within the astrobiological community that is neither entirely conservative nor too marginal, maybe close to an optimum? Although, some people like the philosopher Philip Kitcher say 'it would be highly surprising if the existing social structures of science (…) were to be vindicated by an optimality analysis' (Kitcher, 1990, p. 22), it is not impossible that we indeed end up being highly surprised.

Contrasted opinions on risk-taking in astrobiology

More risk-taking is needed in astrobiology

In order to assess the balance of risk in the astrobiological community as a whole, the following outlines the main opinions of the interviewees on this subject. As a matter of fact, they are divided as to

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whether enough high-risk/high-reward projects are funded, with half considering that more effort needs to be made to implement these projects and the other half considering that there is actually no need.

Those of the opinion that there is an insufficient number of HR/HI projects often refer to a culture of risk aversion despite pro-risk rhetoric that is not followed in practice, notably within NASA as one researcher claims:

> *'NASA may say that they fund high-risk high-reward projects but it's definitely not the main part of its philosophy at all.'*

In his view, there is a clear priority for low-risk science, which he understands as a way to secure results. Therefore, when proposing new projects, any excuse to identify a risk factor would be good for '*killing your proposal*'. Another researcher mentions the pro-risk rhetoric of the European Research Council (ERC) as well, whilst pointing out the contradiction with the reviewing process requiring reviewers to answer questions about the feasibility of the project and the amount of preliminary work that may jeopardize any high-risk project. And for almost half of the researchers, the detection of extraterrestrial life is not sufficiently funded because of the associated risk, although this is the main goal of astrobiology. The research on Mars over the last few decades is taken as a counterexample where, according to one interviewee, too many '*auxiliary*' projects were undertaken to the detriment of research that would take the question of the presence or traces of life forms head on. However, another interviewee opposes this view and argues for fewer expensive projects related to extraterrestrial research and space exploration, and instead for more on Earth, where there is still so much to discover about life.

In order to enable more HR/HI research to be carried out, several suggestions are made. One researcher explicitly mentions the need to establish portfolios of funded projects that are balanced in terms of risk and to ensure that small projects are particularly funded, which are risky but cost very little and are currently underfunded. Another suggests that project leaders should not be required to have significant experience in the field of the project submitted in order to encourage transdisciplinarity and the deployment of unconventional new ideas. And finally, a third argues for the abolition of funding agencies in favour of unconditional funding, as is the case to some extent at the Max Planck Institute in Germany. He claims this assured freedom would allow for more freedom to take healthy risks.

There's no need for more risk-taking in astrobiology

The other half of the researchers interviewed do not consider that the degree of risk in the projects currently funded in astrobiology should be increased. Three interviewees consider that NASA funds enough HR/HI projects and one states that international space agencies have very recently put their pro-risk discourse into practice in a satisfactory manner:

> *'A year ago, I would have said we need more risk. But with the NASA decadal survey and ESA voyage 2050 I think now I'm fine. I think it's the right level.'*

However, three other scientists are much more reserved about the recently funded projects in astrobiology and deplore the promotion of 'overstated claims' from poor quality research. Risk would then be secondary and there is no need for more high-risk projects but more 'good science' regardless of the risk. For instance, the borderline between high-risk and 'foolish' projects would be in some cases totally crossed, as in research about a shadow biosphere (Marcheselli, 2020) associated with a search for 'divine beings and ghosts'.

To conclude this section, there is a clear disagreement among the interviewees concerning the need to take more or less risk in astrobiology and no majority opinion emerges. This finding reflects a diversity of opinion that is in line with the plurality of risk levels in the projects studied and the diversity of ways of managing risk that we have previously reported on. It confirms to some extent disagreements like the ones regarding NASA funding of high-risk research (see part 1) and justifies the type of analysis carried out in this article to assess the actual contribution of intellectual risk in the discipline (see part 2).

Concluding thoughts and recommendation

By detailed examination of a sample of the most impactful papers in astrobiology over the last 20 years, we sought the actual contribution of HR/HI projects. Our survey of authors shows that the selected breakthroughs come from research considered low, medium and high risk in similar proportions, but that the most impactful papers come from high-risk research, and that risk and impact are significantly correlated.

In order to understand qualitatively what types of approaches correspond to more or less intellectually risky research projects, we conducted interviews with 10 authors of articles with varying degrees of risk. These accounts shed light on different facets of intellectual risk. Indeed, it is linked to the risk of not obtaining exploitable results, but also of obtaining only negative or confirmatory results, both of which have less impact. Furthermore, once a result has been obtained, another risk may lie in the interpretation given in the article, which may deviate from the scientific mainstream and run the risk of being refuted. Finally, it is striking that 8 out of 10 projects were deemed exploratory research and whose success does not depend on specifically predetermined results.

As the selected breakthroughs are only single elements of the interviewed scientists' research, we asked them about their attitude to risk in their overall work. Half of them admit to seeking to limit risk as much as possible, primarily for moral reasons towards the team they lead or the society that pays for their research in most cases. Other scientists integrate risky projects into their research by more or less explicitly setting up a balanced portfolio of projects. And finally, two researchers state that they are not affected by risk and are prepared to propose the riskiest projects as long as the outcome is potentially transformative and achievable.

Finally, we asked the respondents their opinion on intellectual risk-taking in contemporary astrobiology and what changes they would advocate. Half consider that not enough HR/HI projects are being conducted, particularly with regard to the direct detection of extraterrestrial life, which is supposed to be at the heart of the discipline's motivations. The other half do not consider that more HR/HI projects are needed in astrobiology, either because the right level has already been reached or, for the most critical of them, because the discipline must first improve the quality of the research carried out before worrying about risk.

Our study supports the idea that a risk-balanced portfolio is already driving recent breakthroughs in astrobiology and does not show that the discipline as a whole is biased towards too much or too little risk. Instead, we show that pursuing risky projects leads to the highest impacts, perhaps unachievable by more conservative routes. To take an extreme case, it is difficult to imagine that the SETI radio project (the search for radio signals from extraterrestrial civilizations) (Billingham, 1990) could be replaced by a low-risk incremental search. By extension, any research that directly tests the hypothesis of extraterrestrial life, which is at the heart of astrobiology, is exposed to significant intellectual risk given the profound uncertainties associated with this hypothesis (Kite *et al.*, 2018). However, the search for extraterrestrial life can also benefit from the contribution of lower-risk exploratory projects whose success is not tied to any particular outcome, as exemplified by several projects discussed above. Such approaches may include the search for anomalies unexplained by non-biological phenomena, the study of which may lead to important discoveries even if extraterrestrial life is ruled out (Cleland, 2019, chap 8).

The results discussed here do not constitute a case for dramatic change in the organization of the astrobiological community vis-à-vis risk, but they do allow us to make the following recommendation:

> *To foster significant breakthroughs, it is important to maintain a large- and medium-scale risk-balanced portfolio with a significant proportion of exploratory projects.*

This call for vigilance is all the more justified as the current academic system tends to hinder intellectually risky (Boudreau *et al*., 2016) and exploratory (Haufe, 2013) research. The concept of the Orbilander mission to Enceladus, which was recently approved (NASEM, 2022), is in our view an example to follow that illustrates our main suggestions. Indeed, the architecture concept which was subject to a cost-benefit analysis (MacKenzie *et al*., 2021) includes a set of instruments associated with exploratory projects of varying degrees of risk, allowing both the characterization of bulk organic molecules and potentially the search for genetic polymers much more specific to life. However, our analysis does not allow us to estimate whether there should be more or less risk in the current portfolio, even though high-risk research seems to be a key lever to generate breakthroughs in the field. Indeed, in order to identify an optimal ratio, it would be necessary to estimate the proportion of risky projects that do not lead to any results and at what cost, which could be the subject of a subsequent study. Such a study could also make it possible to assess the extent to which a subjective assessment of risk by the authors (as in this case) actually corresponds to a greater or lesser probability of success. Furthermore, it would be interesting to study the extent to which the findings established here in astrobiology are valid in other scientific disciplines and can be generalized to the entire structure of scientific research which is regarded to be rather risk-averse (Stanford, 2019). As things stand, it would seem that the implementation of large-scale risk-balanced portfolios is a consensual preferred avenue (OECD, 2021).

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