Learning Curves in Orbit: Progress with AI in Space Science

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Abstract

AI methods are being touted as a powerful new source of scientific progress. Are they? If so, what kind of progress do they facilitate? To find out, we employed qualitative research methods to explore how space scientists conceive of AI. We show that space scientists are mainly concerned with whether AI can help them solve specific problems, and more generally, to extend their abilities in useful ways. This coheres best with a "functional" account of scientific progress (Kuhn 1962; Laudan 1978; Shan 2019, 2022). Despite recent work applying functional accounts to seismology (Miyake 2022) and economics (Boumans and Herfeld 2022), the functional account is still "insufficiently assessed" (Shan 2022, 2). Inspired by our qualitative data, we propose a new type of functional account according to which scientific progress is simply improving scientific abilities.

1. Introduction

Views on scientific progress may diverge over any of the following issues:[1]

(i) who or what makes progress,
(ii) what progress consists in the increase of,
(iii) what counts as an "increase,"
(iv) what the "bearer" or "vehicle" of progress is,
(v) what the right scale of analysis is,
(vi) what kinds of progress there are, and
(vii) how progress is related to other issues in philosophy of science.

For example, concerning (i), we might want to know whether it is the individual scientist, the community of scientists, an entire scientific discipline, or the human species as a whole that makes progress. Concerning (ii), we might want to know whether progress consists in increasing truth, knowledge, understanding, problem-solving power, or something else. Concerning (iii), we might want to know whether "increase" is

[1] For recent discussions about how we should think about the debate on scientific progress, see, e.g., Dellsén (2023, 2025), Dellsén et al. (2022), and Rowbottom (2023).

best understood as being related to, e.g., the *degree* of truthlikeness or justification, the *accuracy* of representations, or the *number* and *significance* of problems that scientists are able to solve. Concerning (iv), we might want to know whether progress is primarily evinced in changes to theories, models, concepts, abilities, or something else. Concerning (v), we might have different views about how to delineate "episodes" of scientific progress, i.e., when an episode of progress begins and ends. Concerning (vi), we might want to distinguish between things which *are* progress and things which *lead to* progress. Concerning (vii), we might want to know how progress relates to, e.g., the aims of science, arguments for and against scientific realism, and the debate about values in science, among other things.

Some of the above-mentioned issues are closely connected, others less so. A position on (ii) will strongly constrain positions on (iii), but a position on (i) need not constrain positions on (ii). Accounts of scientific progress are expected to address several of (i)-(vii), but not all contemporary accounts address all of them. This is fine, because accounts that only disagree about, e.g., (ii) might nevertheless agree about most everything else.

The current debate on scientific progress revolves around four main accounts. The first is the "semantic" account (Bird 2007). On the best developed version of this account, it is the changes to theories that we should pay attention to when evaluating science for progress. And the kind of change that is important is getting closer to the truth. The main proponents of this view are Popper ([1963] 2002, 1972) and Niiniluoto (1987, 2017, 2014). On Niiniluoto's version, Theory A is closer to the truth compared to Theory B if Theory A is more (approximately) true in more of the possible worlds that are close to the actual world. As Norton et al. (forthcoming) put it, the semantic account sees progress as focusing on the balance between approximate truth and informativeness. They give the following example. One theory claims there are 9 billion people currently living on earth. At the time of writing there are only 8 billion, so this is, strictly speaking, false. A second theory claims that there are less than 100 billion people. This is true. The former theory is nevertheless more truthlike as it more closely approximates the actual world and picks out many possible worlds that are closer to the actual world, while the latter theory is less truthlike because, even though it is literally true, it picks out very many worlds which are quite far from ours. According to Bird, this is the "least demanding" of the four accounts of progress (Bird 2022b).

Another account of scientific progress is the "epistemic" account. On this account, science makes progress when knowledge is increased. Knowledge is a cognitive epistemic state, so instead of theories being the primary vehicle of progress, the epistemic account measures progress wherever we find knowledge (e.g., in the minds of individual scientists, or in groups, or perhaps wherever data is stored – see Birch 2025). The main proponent of this account is Bird (2007, 2022a, 2022b). A central motivating intuition for this account is that arriving at a true claim using unreliable methods should not count as progress. In other words, what the semantic account misses is justification. Insofar as we depart from the truth, or lack justification, we move away from progress. This account is more demanding than the semantic account, as it adds another necessary condition (justification).

A third account of scientific progress is the "noetic" account. On this account, science makes progress when understanding is increased. Understanding is a cognitive epistemic state, so progress will be a property of changes to the cognitive-epistemic states of scientists. On the best developed version of this account, science makes progress when scientists grasp models that accurately and comprehensively represent the dependencies in a system. Progress can be increased via *better* grasp of a comprehensive and accurate dependency model, or equal grasp of a *more comprehensive*, or *more accurate*, dependency model (see Dellsén 2016, 2018, 2020, 2022, 2023; see also Norton et al. forthcoming; Dellsén, Lawler, and Norton 2022; Potochnik 2017). Like the epistemic account, the noetic account is more demanding than the semantic account, and depending on how we conceive of the relationship between knowledge and understanding, this account may or may not be more demanding than the epistemic account (Bird 2022b). The three above accounts are "factive" in the sense that truth is required by each of them as a necessary component for progress to take place (Dellsén, forthcoming).

Finally, there are "functional" accounts of scientific progress. The basic idea here is that progress concerns increasing the efficiency or effectiveness of carrying out some particular function. Traditionally, that function has been thought of as problem-solving (Kuhn 1962; Laudan 1978). On this specification of the view, a scientific community makes progress when it gets better at solving problems, and this is measured in terms of changes in the number of significant unsolved problems: Progress takes place when a community solves a significant problem or "downgrades" an unsolved problem from significant to insignificant. Kuhn and Laudan are explicit that progress of this kind is independent of truth and knowledge, at least in the sense that there can be functional progress even under a false background theory.

Until recently, this view has been "taken for granted as indefensible" (Shan 2019, 740). Shan identifies several reasons for this. First, it has a "skeptical" and "antirealist" flavour, as it consciously starts from a position "internal" to scientific practice, such that what counts as a significant problem is relative to a research tradition (Niiniluoto 2014; Dellsén et al. 2022). This leans antirealist because scientific realism involves a commitment to science as a generally progressive enterprise, yet when a paradigm changes, the notion of significance changes, and this makes it difficult at best (impossible at worst) to compare progress across paradigms. A related problem is that even if we could define a cross-paradigm notion of significance, the number of significant problems solved by a brand new paradigm will typically be very low compared to the previous paradigm. This makes paradigm shifts seem anti-progressive by definition, which is counterintuitive. A second problem is that progress is a measure of the *number* of significant problems solved, and it has proven difficult to "count" problems (Collingwood 1946; Kleiner 1993; Rescher 1984). Third, as mentioned above, progress is possible even when the reigning paradigm or background theory are false, and this is thought to be unintuitive (Bird 2007 69-70). Finally, the functional account measures progress in terms of socially recognized solutions to significant problems. This means that solutions that go unrecognized, like Mendel's solution to the problem of the mechanism of inheritance, do not count as progress until they are recognized by the community, and this is also taken to be unintuitive (Shan 2019, 743).

A new version of the functional account has been proposed by Shan (2019, 2022), which attempts to avoid these problems while maintaining the spirit of traditional functional accounts. The initial statement of the account was as follows: "Science progresses if more useful research problems and their corresponding solutions are proposed" (Shan 2019, 744). Shan's motivations for this view are several. First, he agrees with

Kuhn and Laudan that problem solving is a focal point of much scientific activity. However, he recognizes that for any given problem, the way it is defined and framed can change over time (e.g., in response to new experiments, data, concepts/models, techniques, instruments, etc.), and so a better definition and framing for a problem should also count as progress. Finally, Shan focuses not on significant problems, but *useful* problems. These are those which are defined and solved in repeatable ways that suggest reliable and general frameworks for solving analogous problems (2019, 746). Shan's view doesn't measure progress in terms of socially recognized solutions, or socially recognized usefulness, but simply in terms of whether problem definitions and solutions are repeatable and suggest generally reliable frameworks for solving new problems. In this way, it aims to avoid the relativism of the traditional account.

We will not criticize any of the above views. Instead, we present new evidence concerning how AI is changing one corner of science, and extract from this a new account of scientific progress. Our main claim is that our account of progress deserves a place at the table with the others. The evidence is published here for the first time, and was generated using qualitative methods. The motivation for using qualitative methods will be given in the next section. Some final introductory words might still be useful here, however, to motivate the focus on a) the use of AI, and b) the focus on space science.

The reason we are focusing on AI in the context of scientific progress is that very strong claims are being made concerning the potential of AI to affect scientific progress, not just by big tech companies, but also by scientists themselves (Sourati and Evans 2023; Alvarez et al. 2024; Messeri and Crockett 2024). Radical optimists claim that AGI is just around the corner, and it will accelerate innovation in science by producing new, good ideas for how to address global issues, including fusion reactor design, world hunger, and disease. Skeptics demur, and pessimists worry that AI will flood the scientific marketplace with boring useless papers; change incentive structures (e.g., by motivating scientists to work on problems that AI is good for solving rather than problems that really matter), and cause important scientific skills (like conducting a literature review) to atrophy (Cheng and Zhang 2025). While there is growing philosophical interest in the effects of AI on science, there is nothing yet written from a philosophy of science perspective on AI and scientific progress, and this paper aims to help fill that gap. The kind of information we require concerns how AI is already affecting scientific progress, and as there is little information publicly available on this, so we decided to collect the data ourselves. One thing that is important to note at the outset is that we choose not to define progress in advance, but to see what scientists think about it. This creates a risk of confusing "properly epistemic" progress with something more like socio-technological progress. But given the inclusion of the functional account in the debate, and the functional account's inclusion of the latter, we thought this should be acceptable. More on this in section 4.

The reason we are focusing on space science is because achievements there are many and easy to list. Most of us can name the first satellite put in orbit, the first dog in space, the first human in space, the first person on the moon, and at least one of the Mars rovers. In addition to technological achievements, the number of scientific discoveries that any given space mission is responsible for is usually in the hundreds.[2] National and

[2] Some major scientific discoveries made by space science institutions like NASA and ESA concern the existence of Earth's radiation belts, the structure of the sun, the planets and their moons, the existence and features of black holes, steadily burning cool flames, drug discoveries relevant for cancer, gum disease, muscular dystrophy, Alzheimer's,

international space programs like NASA keep close track of the amount and quality of the science they are responsible for, including which programs have led to more progress and why (Pirtle and Moore 2019). Further, space agencies are themselves interested in the potential of AI to accelerate progress. Joe Pellicciotti, NASA's chief engineer, claims that "some of the biggest changes, at least more recently, have been in the digital area and AI world…We've seen advances already, and that's just going to continue to grow. And as it gets bigger and bigger, and we qualify and validate more of these systems…it'll just accelerate exponentially. So, I think there's huge advancements…there's a very positive future in it" (Almeida 2025). Katherine Van Hooser, NASA's deputy chief engineer, says "some of our analytical solutions would take days to run on a supercomputer…And now those same level of solutions, and probably even better ones, are coming from, you know, somebody's laptop that they're, they're running, you know, multiple times in a day. And so, the tools have gotten a lot better, which is great because it lets us explore more problems and find, you know, multiple solutions or options for programs. That's where I think we've gotten a lot stronger…We've got to figure out how to use AI and figure out how to make more of our digital tools work together more efficiently, or else we're going to get left behind" (Almeida 2025, see also Izzo et al. 2022 and Antonsen et al. 2025). Given the intuitive connection between space science and progress, as well as growing interest in AI methods in that field, this seemed like a good case study for better understanding the effects of AI on progress, and perhaps also the nature of progress itself.

2. Methodology

This study employed a qualitative, interview-based approach to explore how individuals within an interdisciplinary AI research team engage with and understand decision-making in the development and deployment of AI systems. It is part of an on-going multi-year qualitative project. Qualitative methods were chosen because they allow for deep, context-rich insights into participants' experiences, values, and interpretive frameworks—factors that are often crucial in scientific decision-making, but difficult to quantify in discussions of technological practice and epistemology.[3]

The core aim of this paper is to understand how practitioners interpret, justify, and navigate the practicalities of working with AI systems. These kinds of questions are not easily answered through surveys or quantitative metrics. Instead, semi-structured interviews provide the flexibility to probe underlying assumptions and the social and organizational dynamics that shape practice.

At the time of writing, twenty-five participants had been interviewed, all of whom were involved in space research within one of several different space organizations. Participants held diverse roles within the field of space science, representing various teams across space organizations and institutes. Our participant group includes 8 project managers, 7 permanent research staff, 4 postdoctoral researchers, and 6 graduate students. To preserve anonymity while allowing the reader to distinguish among perspectives, we use coded identifiers: M1, M2, etc., for project managers; R1, R2, etc., for permanent researchers; P1, P2, etc., for

Parkinson's, asthma and heart disease. A great deal of work is done on Earth's climate from space which is relevant for understanding climate change, including predicting and addressing natural disasters.

[3] The need for sociological investigation in the debate on scientific progress is nicely motivated by Rowbottom (2023, 43).

postdoctoral researchers; and G1, G2, etc., for graduate students. In terms of gender representation, 7 participants identified as female and 18 as male. While this distribution reflects current imbalances in the field, we acknowledge the importance of gender equity and are actively striving toward a more balanced representation in the research.

Participants were recruited via email invitations sent by Winters, who had previously spent time as a guest within one of the space organizations in an ethnographic capacity, and who had also developed connections with individuals from other organizations through her ongoing ethnographic fieldwork. This pre-existing relationship helped facilitate openness and trust during the interviews, which were typically conducted within the organizations themselves. This allowed for context-rich conversations grounded in the participants' everyday professional environments. The interview process aims to ensure a comprehensive representation of perspectives across different roles and teams within space organizations, and to investigate other topics. The long-term nature of the study helps to capture evolving insights and practices within the field.

This paper reports findings from the first twelve months of the study. Follow-up interviews (with M1, G1, G2, P3, P2), were informed by preliminary analysis, and tailored to explore particular themes in greater depth. Participants were affiliated with three distinct space research institutions. Three participants were employed as a professor at a university, one founded an independent space-related foundation, and one started a space-related start-up. The remaining 20 participants were affiliated with a single, large space organization, representing 10 different internal teams. Of these, 11 participants worked within the same team, while the other 9 were part of separate teams within the organization. At the time of writing, five participants are no longer employed by the organization at which they were interviewed, due to the scheduled completion of their research projects. This distribution reflects both the organizational diversity and the transitional nature of careers in space science research, allowing for a nuanced understanding of differences and commonalities in practices, perspectives, and institutional roles.

Each participant took part in one or several semi-structured interviews lasting between 45 and 90 minutes. Interviews were conducted either in person or via video call, depending on availability and location. All interviews were audio-recorded with participant consent and subsequently transcribed. The quotations have been revised with attention to authenticity and diction, to enhance clarity. The research project was approved by the Science-Geo Ethics Review Board of Utrecht University.

Winters conducted the interviews and did the coding and preliminary analysis. The transcript coding proceeded iteratively using a grounded theory approach: initial descriptive codes were developed based on repeated readings of the transcripts, then grouped into higher-level themes, for example, related to epistemic positioning in relation to various cognitive processes, research-related and practical decisions, and different institutional roles. Stuart offered ongoing feedback on the emerging themes, guided the development of the coding approach, and supported the structuring of key quotations and interpretive framing throughout the analysis. This collaborative process ensured analytical rigor while retaining the close empirical grounding characteristic of qualitative research.

This methodological approach enables us to trace how pragmatic reasoning manifests in lived AI research practice, as narrated by practitioners themselves. It offers fine-grained insight into the trade-offs, justifications, and moral vocabularies at play, elements that are often obscured in more formal or abstract accounts of AI development.

3. AI and Progress in Space Science: Results

In general, we were interested in finding out whether, when, and in what senses AI was having an effect on work in space science. Interviews revealed several areas in which AI was having an impact. These include autonomous on-board machine learning (including rover navigation and satellite control), data analysis (both for science and for policy use), medicine in space, mission design, estimating the shapes of comets and asteroids, materials science, and spacecraft design. We have collected participant responses into two main types, concerning a) problem-solving and b) general capacity-building. This is somewhat arbitrary as a thematic separation because uses of AI that solve particular problems typically also build more general capacities on which researchers later draw, and general capacities are built because they are expected to solve problems (among other things). Still, we can also think about the next two sections as being distinguished primarily by the level of detail the participants have access to concerning the challenges that AI is being taken on to address: the first section concerns cases where the problems are known and quite specific, whereas the second concerns more general, anticipated issues.

The fact that the quotations fit entirely into sections only concerning problem-solving and capacity-building is surprising, insofar as we do not find direct mention of other markers of progress that we would expect to find if the semantic, epistemic or noetic accounts were correct. We come back to this in section 4.

3.1 When AI is progressive: Solving specific problems

Most participants discussed AI in the context of specific problems that AI methods could be used to solve, and they criticize AI when it is introduced in the absence of such problems.

We begin with several extended quotations from an interview with M3, who is transitioning from a high-profile innovation-center into a new role within a space agency, focusing on the application of AI and software in space exploration missions. Specifically, their work involves identifying mission-critical challenges—such as rover navigation on the Moon—and determining where AI or advanced software solutions can meaningfully address those challenges. This fact makes M3 a key source of information for how AI is affecting space science.

> Yesterday I had a call with someone from a consultancy - I won't name them, who was really trying hard to sell me his AI algorithms. But he didn't understand what I actually needed them for, or what our challenges in exploration are. He came with a very technical pitch, talking about knowledge graphs and neural networks and I know what those are, but I had to ask: which of my problems is it going to solve?

Often there's a disconnect, because AI is hyped. We get lots of proposals: large language models, neural networks, but they don't really address our needs. I don't want to make a long list of algorithms just so we can say we've used them. What I'm trying to do is identify real problems in our missions, whether it's a rover, a space station, or an astronaut on the Moon without GPS, and then ask: is this a problem that needs software? Or AI? It's not always AI.

So I'm trying to separate the hype from actual use cases, because there are people who want to use AI just so they can say they've used AI, because it sounds cooler. But it's always tricky, working within a hype cycle...

I have to say, I struggle most when others assume AI is needed to solve a problem, whereas I tend to say: we don't know that yet. We know the problem - let's first try to understand it properly, and then identify what technology or software is actually required. I've found that the less someone understands what AI is, the more likely they are to think of it as a kind of magical solution. I always try to carefully question whether we really need AI in a given case. Sometimes, statistics and AI are quite close and for some problems, all you need is a smart Excel sheet with macros or a pivot table. That's not AI, but for someone unfamiliar with Excel, it can still feel like magic...

I think in this hype phase, 90% of people just have this black box phenomenon of not understanding what AI is. Not knowing, but thinking it's intelligent and will solve all the problems. As a cure for everybody. And it's difficult for me because I don't want to demotivate people. It's also nice that they are enthusiastic about the technology, but at the same time it's sometimes dangerous because they think they know what it is and how to use it, but it's not really the case. So it is a tricky balance to navigate and to try to bring reason. Yeah. And what I usually try to do is break up the problem and say, what exactly do you want to use? What kind of AI, what are we talking about? And then, often, they realize they don't actually know...

I think mostly, it depends on who you talk to. But I think in management, where it's considered something that we need to do now because everybody does it, it's like, 'Oh, let's put AI into this.' 'We need something that is with AI.' And then once you start asking, 'What do you have in mind specifically? Was there an algorithm that you want to use here, and what would be the advantage?' I do it without making them completely uncomfortable. I don't want to embarrass them. I know they don't know in detail. So I'm always just like touching a little bit on the subject. But once you start asking questions, they will be like, 'Ah, actually, I'm not the expert, so maybe you should take a look at this'...

This is a situation everyone is dealing with, and honestly, we're probably a bit late in running into this problem. Many companies started looking into AI ten or fifteen years ago. For some, AI is already fully integrated. They use it like a battery or a chip or a laptop, just another great innovation. For them, AI is normal. Then there are others who are either scared or not informed. So we're often caught in the middle, - between enthusiasts who don't understand AI but are eager to use it, and those who are resistant or intimidated by it...

> What I prefer is to focus on the problem - not talk too much about the AI, and then propose a solution, whatever it may be called. That approach comes from my background in university and software development. I'm part of a generation that was really pushed to think about user needs. There's just so much software out there that was built by developers who wanted to build something, but it doesn't meet any user requirements. It's not user-friendly. That has really influenced how I work: I always try to put the user at the center. (M3, 23-08-2024)[4]

M3 is cautious about the tendency to over-apply AI solutions without first adequately determining whether there is a problem it can help solve. They emphasize that this risk stems from a lack of conceptual clarity about what AI actually is, leading to its perception as being a "magical" or catch-all solution. But without potentially solving a specific problem, it's felt that there is no point in using it. In many cases, they note, there is no need for AI. Overall, this suggests that AI is justified when needed, and it is needed when it can solve a specific, existing problem. As we will see, this is not the only justification for introducing AI, but it is the one that occurs first and most powerfully to M3.

Using the above metric to judge whether AI should be used provides a simple way for M3 to navigate AI hype. While AI is acknowledged as potentially productive, M3 warns that excitement can be harmful if it fosters misplaced confidence or uncritical implementation. Implementing AI when a traditional solution exists can be harmful in a number of ways, though mainly in terms of wasting resources and introducing new failure points. As a corrective, they advocate for a problem-first approach, in which the choice of technological tools, including AI, is guided by a clear understanding of the use case. Their method involves breaking down the problem, questioning the available and proposed technological solutions, and making a decision based on what seems most likely to solve the problem in a safe, reliable, and efficient way.

Examples

The following are specific examples where AI was thought to be useful in solving specific problems.

M1's team is working on neuro-ocular syndrome, sometimes referred to as spaceflight-associated neuro-ocular syndrome. This is a condition that affects astronauts during long-duration space missions, e.g., those aboard the International Space Station. There are several problems to solve concerning this syndrome, which M1's team has considered using AI to address.

> A lot of people have been asking: how can we detect it early, and how can we protect against it? Our team took that as a challenge and developed a solution using a mobile phone camera combined with an artificial intelligence system to detect the onset of symptoms.
>
> Essentially, it's possible to use a phone camera to image the back of the eye, the retina. We developed an AI system that analyzes those images and can identify early signs of this condition. To

[4] Following usual social scientific conventions, we cite quotations by reference to the participant and the date of the interview.

> train the AI, we used a large dataset from people here on Earth who have this syndrome. So it's actually medically really robust.
>
> It's not like we're using AI to talk to the astronauts. They don't care about that. They want tools that can help them, you know...they need tools that help them understand what's happening to their bodies, and medical support. This tool is pragmatic and addresses a real need.
>
> We've flown into space. It works. And we're hoping to fly it again in a year's time. And then hopefully we'll turn it into an operational tool. So it'll become part of the inventory of what astronauts have available all the time to check on their health. (M1, 14-08-2024)

Here, M1 distances (in a nuanced manner) their project from those which employ AI for its own sake. In contrast, what they want to emphasize is the system's utility as a practical diagnostic aid rather than a technological showpiece. The goal is not to impress with AI, but to equip astronauts with tools that help them understand what is happening to their bodies in near real-time, thus enabling autonomous health management. In this context, it is particularly important that M1's team maintains direct contact with astronauts (who are literally across the hallway) from whom they receive first-hand feedback while they are developing the tool.

M3, who we quoted from at length above, is also working on several AI applications in the team.

> In space exploration, we now have a new team looking at software and AI, and I'm joining that team to focus on AI. Right now, I'm trying to understand what kind of data we have, how we've used AI in past missions, and how we should use it in the future. I'm also thinking about where AI could become critical to mission development, and what we need to do to stay state-of-the-art, or ideally, to help Europe lead in AI and software development. (M3, 23-08-2024)

One of these projects concerns autonomous navigation for lunar rovers.

> You don't want to get stuck in the shade, and then your battery dies, because there are fourteen days of night on the Moon. So, if you have solar panels, and you get stuck in the shade, that's very bad, because you're dead. So yes, it's critical for rovers on the Moon to navigate quickly into the sun.
>
> We need having a camera that sees what is going on and recognizes there are different rocks, or where the elevation is like this or like this, and the distance is like that, and the sun is here, and I'm here...and how do I get there quickly without getting stuck? This is something, where we probably need better software.
>
> And then my role is to identify scenarios like this where we need better software or where we can use AI in this case. In this case, the AI is part of the vision-based system to do hazard avoidance. So, the AI is used to analyze what the camera feed is showing to automatically recognize and label the rocks and the terrain, and to propose route planning for the rover based on that. And then it's not

> necessarily my job to build the software, but to write the requirements for it based on our mission and find a company who can do it. (M3, 23-08-2024)

The focus on AI as a problem-solving tool is unsurprising, given the quotations from M3 above. This case of using AI to navigate difficult terrain illustrates M3's entire method, from selecting a problem, understanding (defining and framing) the problem, identifying key points in the solution-space, and identifying the best tools (and then creating and refining them) as part of a solution to the problem.

One final quotation comes from R3, who is a mechanical systems engineer with extensive experience in the space sector. Over their career, they have focused particularly on the design and development of large, ultralight deployable antennas and reflectors. This specialization includes work on concepts such as tensegrity-based structures, for which they hold multiple patents. Although they cover a broad range of mechanical aspects related to spacecraft, their primary expertise remains in large deployable reflector technologies, including conceptual design, structural analysis, and prototype testing.

> In the space sector, they are focusing on the management of data and possibly data on board of satellites. So we have already a few cases of small satellites, which bring with them the NVIDIA components for processing of data. And the idea is that on one side, the satellite is capable to detect what is of more interest and focus on that zone in respect to another. So it is a selecting which area to focus on and to process the data in a smart way, so that the limited bandwidth, the channel that we have from satellite to ground, the best picture can be exploited...
>
> So the other huge effort is, making those data accessible to decision makers. If you are a politician who needs to take decisions about climate? Agriculture? About pollution? And which data are they looking for? We're investing a lot in the domain of making Earth observation data usable for the community and for the decision makers.
>
> A third part where we are active is the so-called, 'digital green.' The digital green is another domain where we are extremely active in artificial intelligence. The idea is to make a huge simulation model with the techniques of machine learning, and the model improves itself. Let's say that it's simulating the entire Earth and combining the data from the satellite, combining geographical and social aspects, the populations, and whatever more. This is something that is pursued also at the European Commission level. (R3,24-09-204)

R3 outlines three major areas (other than their own) where machine learning is currently being applied in the space sector. The first is autonomy in satellite data collection. Satellites can only transmit data when they have a line of sight to ground stations that can receive data, and those transmissions have limited bandwidth, so it's important to make sure the satellites are getting and transmitting useful data. Rather than program data collection protocols in advance, or try to react in real-time to new developments, satellites can use AI to make decisions about where to focus their data collection, in a flexible way. Second, R3 describes an institutional shift toward making Earth observation data more accessible to decision-makers. Given the volumes of satellite data generated, the challenge lies in transforming this raw data into actionable insights for policymakers in domains such as climate policy, agriculture, and pollution control. And AI can be useful here, by making patterns of interest cognitively available to those who need them. Third, they highlight the emergence of "digital green" initiatives, which aim to build comprehensive machine-learning simulation models of Earth. These models integrate geospatial, environmental, and social data to simulate the planet's systems dynamically. This effort aligns with broader goals at the European Commission level, to support sustainability, governance, and public utility through AI-enhanced Earth observation. As with M1 and M3, R3's work coheres with the idea that the introduction of AI is justified, and its employment is at least potentially progressive, when it helps scientists generate *useful* data, or make raw data more *usable*, or support sustainability, governance, and public utility by addressing specific needs, in the sense of overcoming specific problems (bandwidth limitations, short signaling windows, and massive and complex datasets).

The work of M1, M3, and R3 exemplifies M3's position on the importance of AI for solving particular problems. In the cases mentioned, participants feel that the question should predate the uptake of a new (AI) tool. The team has specific goals and directives, and putting these into practice leads to challenges, and overcoming those challenges requires solving specific problems. Insofar as AI can be used to help overcome some of these problems, it can be included as a useful or progressive methodology.

Finally, it is important to note that although both M1 and M3 hold managerial roles, they serve different functions. M1 leads a team that conceptualizes and implements new developments internally within the organization, whereas M3 is more closely affiliated with an AI innovation function and liaises with industry. The function of M3 appears to align more closely with management's broader strategic agenda than with a space for open-ended exploration. And yet each of these participants express a similar view about when AI implementation is justified, hinting at a similar view about what progress is, and how AI can help further it.

But this is not the only way we found participants speaking about AI in a positive way.

3.2 When AI is progressive: Improving abilities

In this section, we will see how AI plays an increasingly supportive role in scientific and engineering work, particularly as a tool that can assist in accelerating learning, problem-solving, and creative thinking. AI tools – such as retrieval-augmented generators, computer vision models, graph neural networks, reinforcement learning, and predictive analytics – enhance the above-mentioned processes by enabling faster analysis, generating ideas, and helping scientists mentally probe complex problems. The key benefit lies not in AI offering answers to existing well-defined problems, but in improving the quality (e.g., speed and precision) of the inferential and exploratory processes that researchers already employ. We can think of these processes as related to individual physical or cognitive-epistemic capacities, which might be distributed across individuals and instruments, at the lab or institutional level (Nersessian 2022). At the individual level, AI can serve as a powerful enhancement for those who already have well-developed epistemic capacities (e.g., for analytical or imaginative reasoning). For others, it risks becoming a crutch that may diminish or prevent the development of epistemic capacities if not used reflectively. First, we focus on the positive use of AI to extend epistemic abilities.

R4 is a researcher at a space organization, working on highly interdisciplinary projects that combine numerical computing, physical computing, and physics. Their focus includes integrated computational materials engineering, such as designing and 3D-printing metal components like rocket nozzles based on computer models and adapting complex plasma simulation codes developed by external researchers for use on various computing platforms. The work involves troubleshooting software and hardware issues, rewriting code and equations, and applying DevOps principles for software portability.

> I had a situation with NVIDIA, where they came to me and said, 'I'll start something now, and in five minutes I'll tell you what it is.' After five minutes, they gave a speech and said, 'We've just imagined 140 new COVID-19 vaccines using our computer, without any prior knowledge.' Of course, there's a simulation behind it, but it's not physics-based. It's called Stable Diffusion, which means it just takes particles and tries to arrange them to see if the configuration looks sensible. So this is really imagination, and it plays a certain role with the whole AI topic that you can imagine configurations without knowing whether they will work, without knowing whether they are realistic. And this gives you new design options. Definitely.
>
> For myself, I would say AI enhances imagination, because I had the experience before using the tools. I'm used to imagining things without external help. But I would agree that for people who haven't developed those imaginative skills, it can dull your capabilities. You see an answer and think it's the golden goose, and then you rely on it. But that's not true. You need to challenge the computer. It's just your partner, you still need your own capability to question what it gives you. (R4, 04-10-2024)

R4 presents a balanced perspective: AI can enhance creativity for those with an existing foundation in imaginative thinking, acting as a powerful partner in ideation. Crucially, AI enhances rather than replaces the intuitive and exploratory processes central to scientific inquiry. This introduces a second rationale for the value of AI: its potential to enhance general cognitive capacities, such as abstract modeling, pattern recognition, and adaptive reasoning, that are thought to be indispensable in scientific and technological work. A second reason AI is considered valuable is its ability to enhance general cognitive and epistemic capacities that are expected to be crucial for future scientific and technological progress. Rather than replacing human cognition, AI can accelerate learning, problem-solving, and creative exploration, especially for individuals already skilled in independent reasoning. Through tools like retrieval-augmented generation, AI supports complex tasks in science and engineering by enhancing, rather than automating, intuitive and imaginative processes.

However, there is a cautionary note: Researchers like R4 highlight the need for epistemic scrutiny and emphasize that AI should be treated as a partner in thought – not a substitute for thought – thus reinforcing the importance of reflective and pluralistic scientific practices. Without such engagement, AI may diminish rather than enhance scientific capacities, particularly for individuals lacking prior training or over-relying on a single model, such as ChatGPT. This highlights the importance of epistemic vigilance and methodological pluralism.

Examples

The following are examples where AI was valued for improving general scientific abilities.

P3 is currently engaged in two primary research projects that integrate principles from AI, computational neuroscience, and biomimetic modeling. The first project involves the development of a computational model of the human retina, aiming to simulate how different retinal cell layers and circuits process visual information such as motion, depth, and global features. The objective is to construct a biologically faithful representation that could eventually be implemented in hardware, contributing to more efficient and perceptually grounded machine vision systems. The second project focuses on continual learning—also referred to as lifelong or online learning—which entails developing algorithms capable of adapting to incoming data in real time, without the capacity to store or access the entire dataset retrospectively. This approach mirrors the learning processes of humans and animals and addresses a key challenge in creating adaptive, resilient AI systems.

> Okay, part of the reason why I was working on this continued learning project for the last three years was because I think it would be very relevant for space, and one of the challenges that we have is that space is very inhospitable. So, very likely we're going to have to rely on machines to explore and to send data back that is interesting and so on without being explicitly controlled by humans. And the further away these missions go, the harder it also becomes to control, to communicate, to debug, to, you know, fix problems. And so they have to be entirely on their own. Already the moon is hard enough. Mars now, and other missions to Jupiter, icy moons and so on.
>
> The idea is to send probes even further. We don't know anything about these environments where these probes are being sent. Okay, we have observations and some, you know, very sketchy data, but we don't know what they're going to encounter with these probes. Imagine a probe landing on Titan. We have no idea what it looks like. We can't train a controller or an AI system using data because we don't have the data. It needs to learn on the fly. It needs to be able to somehow adapt to incorporate new knowledge, even after it's deployed. (P3, 04-10-2024)

In this part of the interview, P3 reflects on the importance of continued learning systems in the context of space exploration. As missions venture farther from Earth, the feasibility of human oversight diminishes, and it becomes desirable for AI systems to be capable of operating autonomously and learning in real time. P3 emphasizes that we cannot rely solely on pre-trained models using Earth-based data; instead, onboard systems must be able to update and adapt dynamically after deployment. This capacity for autonomous, situational learning represents one of the most critical and urgent challenges in current and future space robotics.

This line of thinking leads naturally into P3's broader philosophical and methodological approach to AI. For them, the pursuit of adaptive intelligence, whether for use in space or elsewhere, is deeply informed by biology rather than computational efficiency alone. As they explain:

> Most of my inspiration comes from biology. I would, start approaching a problem by thinking about how I, myself, or someone else, or even an animal would approach the solution. How would an animal try to solve this or work around it?
>
> I imagine the process. Sometimes I watch videos of actual animals solving actual problems. I do that often. Especially, the octopus is a fascinating animal. I think it's very understudied and very

> underrated. So, then I tried to look at the models that we have and machine learning solutions or AI solutions and I ask myself what is the difference? So, we're trying to go about this from the machine learning perspective. And how does the animal actually do it? And what is the difference there? And that I try to imagine that difference and then think about how something like the animal's approach could be implemented in a computer or machine. So, in a nutshell, that's kind of the process. (P3, 28-01-2025)

This appeal to biological problem-solving underpins much of P3's work, suggesting a future where machines not only compute but adapt, improvise, and respond—more like animals than algorithms. When approaching a challenge, P3 begins by considering how a human or even a non-human animal might attempt to solve it. This process often includes observing real-life animal behavior.

P3 contrasts this with standard machine learning approaches, reflecting critically on the differences between biological and artificial strategies. It is not merely mimicking animal behavior, but imagining how general strategies might be abstracted, translated, and implemented into computational systems. This suggests a design logic that is grounded in analogical reasoning and embodied cognition, rather than either pure formalism or hands-off machine learning, revealing a creative, cross-domain mode of capacity-building within AI development.

Building on this perspective, P3 turns to the challenges of embodiment, emphasizing that intelligence cannot be understood or designed in isolation from the physical systems through which it perceives and acts. They highlight a core issue in AI and robotics: the limitations posed not only by algorithms, but also by the physical interfaces between machines and their environments. As P3 explains, challenges typically arise even in routine, everyday contexts, and many of the things we do without thinking remain extremely difficult, if not impossible, for AI systems to replicate:

> What I said before about hardware, also extends to our sensors, like biosensors and their equivalents, like cameras and microphones and effectors. So, how do you manipulate your environment? So, we're not there yet. There is nothing that approaches the dexterity of the human hand and the sophistication of the human eye, for example. We're missing a lot of information right there at the interface with the environment.
>
> I imagine a system that doesn't have these limitations. Okay, maybe an amazing android that can walk around, with very sophisticated sensors and defectors and so on. So what would the controller be for that? How would we implement a controller for such a machine? Conversely, consider the opposite scenario, we might have a highly capable controller, but it is connected to only basic, rudimentary sensors and actuators, especially when compared to biological systems. So of course, the ideal situation is to have the amazing Android with the amazing controller, but we never do have both of them. (P3, 28-01-2025)

These quotations demonstrate a sustained attention to a set of cognitive and physical abilities that P3 finds jointly valuable. These abilities might be valuable for solving problems, e.g., providing a specific kind of data. But they might also be valuable even in the absence of a specific problem, e.g., for gathering and interpreting data or navigating new landscapes in an exploratory way. Extending these abilities by developing flexible, robust, reliable, and powerful technological devices is an important goal of these scientists, the achievement of which counts as progress. Future work should clarify whether those abilities are best attributed to the machine itself, to individual humans (e.g., operators, see Vertesi 2012, 2015, 2023), or to human-robot teams (e.g., a lab or company).

4. Discussion

4.1 Little mention of truth, knowledge, or understanding

A notable absence from the interview data was any explicit mention of epistemic good-making features like truth, knowledge, or understanding, and few references to representational accuracy. Instead, the participants mostly spoke about what AI was allowing them to do, including what problems it was enabling them to solve, and which capacities it had improved.

One explanation for this might be that we did not ask our participants what they thought of scientific progress in general. Instead, we asked scientists to define for themselves what was good or bad about recent developments, and we did not impose any restrictions on the kinds of goodness we were interested in (e.g., technological, epistemic, ethical, aesthetic, etc.). Still, we identified specifically epistemic notions of goodness in the transcripts and observations, and then asked follow-up questions to zoom in on those. This produces better data than simply asking directly about epistemic progress, since answers to that kind of question typically result in general mottos that don't necessarily reflect actual positions and practices.

It was surprising that we were able to abstract a notion of scientific "goodness" that was shared across all participants, despite large differences in their perspectives, research topics, and institutional roles. As we have seen, participants tended to frame AI as contributing to scientific success or value through capability-building rather than by producing new truths or instances of knowledge/understanding. We are characterizing "progress" in terms of movement toward that shared notion of goodness.

Still, we might want to distinguish between success and progress, where "success" might refer to the attainment of specific project-bound objectives, and "progress" might refer to more cumulative, general advances. While participants frequently described markers of success, such as implementing a model, achieving technical milestones, or completing project tasks, a broader notion of progress could be abstracted from their responses that transcends individual achievements. They emphasized how AI mediates research practices, opens new methodological avenues, and enables forms of inquiry that extend their epistemic reach. And importantly, progress remains epistemic. For example, P4's work on applying causal inference to telescope data shows how modeling and noise reduction (important successes) contribute to the more general goal of improving the ability to detect exoplanets and interpret astrophysical signals, goals which seem clearly epistemic.

The kind of progress valued by the participants exemplifies Kitcher's idea of "pragmatic progress," i.e., thinking about scientific progress as progress *from* some particular context, instead of progress *to* some antecedently specified goal (Kitcher 2022). Science is not like building a house according to a blueprint, where progress can be tracked against a clear goal and along a timeline with an endpoint. Instead, scientists often only track progress against past performance. In line with this, here is an extended quotation from P1, whose research focuses on using the motion of stars near the center of the galaxy to investigate dark matter. Although the effect of dark matter on stellar orbits is very small, with long-term and highly precise observations, it should be possible to detect its signature. They developed computer models by testing single dark matter profiles, and then attempted to build more flexible models that could account for a wider range of dark matter density shapes. However, this flexibility required more observational data to constrain the many parameters. Their work therefore aimed to see whether it was possible to balance model sophistication and flexibility with realistic limits on observability.

> When we first started working with [AI], we basically began by throwing data at it. Initially, we didn't use real observational data. Instead, we used 'fake,' [synthetic generated] data. These models can be used to create such artificial data, which you can then feed back into the model to test how well it performs. And this is a very nice method to see, okay if I observe ten years longer, can I see any effect?
>
> With having one orbit of data, with the current measurement accuracy, you really hope to be able to constrain the model. And then you do it, and you see, oh, there's no way we can do that. And then you have to say, 'Okay, okay. What if I can improve on the instruments?' Then you narrow down the accuracy on the fake data, and then you realize, okay, you have to narrow it down so much that there is no hope that's achievable in ten years.
>
> And so then you have to, again, you have to find a middle way. So, okay, I say I will observe for 20 years and I will make the accuracy a little bit better. What can I still squeeze out of this model? But when using more flexible models, the trade-off is that they become much harder to constrain with observations.
>
> Our hope was that the model would directly fit the dark matter profile and reveal how it is distributed, whether like this or like that. Depending on the outcome, we could then infer what kind of dark matter it might be, or at least rule out certain possibilities. For example, we wanted the model itself to reveal how dark matter is distributed, and from that distribution we can infer what type of dark matter it might be, or at least rule out models that are clearly incompatible. Unfortunately, we have not yet been able to achieve this.
>
> In the end, it turned out that the results weren't as promising as we had hoped. (P1, 28-06-2024)

This is an episode in which the use of AI was deemed not to be very progressive. The goal is to find out about the nature of dark matter, but the subgoal that was actually pursued was trying to build AI models with certain properties. The negative evaluation has less to do with AI's inability to tell us the truth about dark matter, and more to do with the team's inability to do anything more useful or interesting than what could already be done. And this was measured in terms of ability: AI does not increase our ability to discriminate between models of dark matter given the empirical data that we have, and that is bad.

Prima facie, measuring progress in terms of the improvement of abilities might tell against the semantic, epistemic and noetic accounts. But there is quite a lot of wiggle room. Proponents of those accounts might argue that scientists simply learn to talk in terms of problem-solving and capacity-building, perhaps because of incentive structures, funding mandates, or modesty, while "genuine" progress nevertheless consists in increasing the truthlikeness of theories or the knowledge/understanding of scientists. Or perhaps it could be argued that accounts of progress are purely normative, and therefore if scientists do not speak or act in accordance with some account, so much the worse for science (for responses to these kinds of claims, see Mizrahi 2020 and Rowbottom 2023). Still, we think it is important to mention that these scientists and engineers speak of abilities rather than truth, knowledge, or understanding when evaluating the scientific value of a technological development. We predict that this might resonate with many scientists and engineers who work outside of space science, though given our limited dataset we make no claims to general validity across science.

4.2 Little mention of opacity

Another surprising absence in our interview data was any explicit mention of computational opacity. Given the centrality of this concept for philosophy of AI, one might think that this would be high up on the list of conversation topics. However, at least in the case of space science, this does not seem to be a major concern.

Why might this be? Here is NASA's chief engineer Joe Pellicciotti again: "We've taken the design of a product, and we've used AI to make that design more mass efficient, more stiffness efficient, and so forth. In the end, you know, we can take that design, and we can test it and make sure it still meets all of our qualifications, but that design has…it's taken less time to go through the process. It's more efficient, which lowers cost" (Almeida 2025). Pollicciotti gives a general template for AI-use that we saw many times: Scientists use AI to identify something useful, e.g., a way to make a component lighter, or a flightpath more efficient. They then try out the new idea and verify that it works. For example, they can weigh a model of the component after modifying it, or run a proposed flightpath through the usual calculations to make sure it works.

Here is an extended quotation from P2, who is working on applying graph-structured data and neural networks to novel problems. In this quote, they describe their current work as developing and testing machine learning systems on simplified experimental setups to better understand how well new ideas perform within a system:

> We have certain metrics to measure how well it's working in machine learning. This could be accuracy. Let's say we want to do image recognition. That would be how often the model is correct, in saying what's the content of the image.
>
> What we basically do, is train. In our case, we train small 'cubes' to arrange themselves into a specific configuration on their own. Another way to think of it would be a swarm of satellites: they should autonomously form a certain constellation as fast as possible. The performance metric here is how successfully and how quickly they reach that target configuration.
>
> You don't directly go to the endgame, but you try your idea on something super simple, where it should definitely work. The metric then acts as a signal: it ranges from 0 to 1, with 1 being very

> good. If it's close to 1, say 0.8, that suggests the system is working. But if it's stuck at 0, then something is wrong, either in the concept or in the implementation.
>
> These signals guide the experimentation. You then think about what else you can do to better understand what's happening inside the system. For example, one thing we did recently was rerun the experiment under different noise conditions. Another useful approach is plotting various outputs, visualizing graphs of the system's behavior, and directly observing what it's doing. In our case, that means watching how it reconfigures itself over time. (P2, 09-09-2024)

It doesn't matter that AI is opaque, because there are other methods for verifying the outputs of AI models. The way the scientists experiment with models is less about understanding the computational details of how they work, and more about getting a clear picture of the model's behavioral profile, so they can use it.

What does this mean for the debate about scientific progress via AI? If the epistemic or noetic accounts are correct, opacity should be a concern for scientists, as opacity stands in the way of progress, by standing in the way of justification (a problem for the epistemic account) and grasp (a problem for the noetic account).

A reply from the epistemic account might be that opacity is not a serious concern precisely because the outputs of AI can be justified by other means. However, on Bird's version of the epistemic account, science makes progress when knowledge is increased, even if no human ever notices. AI models might contain a great deal of knowledge that humans cannot currently access. For all we know, it might be making a great deal of progress. Scientists should be interested in finding out whether that is the case, given that they value progress. But they do not seem to be, at least, not in the way we would expect. They are not pursuing algorithmic explainability to find out whether progress is being made by the AI (in the sense of knowledge being produced). Rather, explainability seems more relevant for use and performance. In a recent interview, scientists at Google DeepMind were asked whether drug discovery algorithms needed to be explainable. They replied that "the call for explainability" is another way of signalling that the model isn't working perfectly well and "we need to make this model better." Explainability is for helping us "understand the pathologies that this model has, the biases that it has." The goal is to "get to this point where, yeah, actually, we can just do end-to-end design purely in silico, and maybe do a final round of verification in the lab at the end" (Fry 2025). Creating algorithms like these would enable a real leap in ability, and this seems scientifically desirable not only because of the knowledge produced or contained in the model (which is inaccessible to humans), but mainly for what it enables us to do. This is easy to make sense of on the ability account: the opacity of *human* abilities does not stand in the way of their value, so it should be no surprise that we see the same in the case of algorithmic abilities.

Another reply from the epistemic account might be that AI is only useful in the context of discovery, not the context of justification. Indeed, there are good reasons for thinking that AI is used as a tool of discovery, and does not, on its own, provide full justification for much (Duede 2023). But very little processes do provide complete justification on their own: scientific justification is holistic (Dürr and Dellsén forthcoming; Elgin 2017). A scientific laboratory experiment does not, on its own, justify much: it also requires a justified interpretation and justified inductive inference to extend the findings to the real world and make an impact on theory. Likewise for AI use in space science. Here is an example.

M4 holds a senior leadership role overseeing scientific mission support and operational activities within a major international research organization. M4 also teaches and supervises students, and is involved in research, including studies on the fate of planetary systems after their host stars evolve into white dwarfs. They investigate the elemental composition of planetary debris that accretes onto white dwarfs, enabling reconstruction of the original planets' makeup. This work offers insight into planetary chemistry beyond bulk density, something typically inaccessible in studies of intact exoplanets. M4 describes their process as one that relies heavily on imagination, observation, and connection to other fields of research:

> Astronomy, my field, is a particular kind of science, it's observational. You can't run lab experiments in the traditional sense. If something explodes, you can't rewind and repeat the event. It might be a unique occurrence, and you may need to wait another 100,000 years to see it again, which is, of course, beyond the length of a PhD.
>
> Breakthroughs in nearby fields I would also consider as fun. It's great to see the successes of these people, and it motivates you in your own work. You're curious, you'd like to understand better how the universe works. What is matter? Have we understood physics? We think we have, we can explain almost everything on Earth and in the universe with the laws of chemistry and physics that we have today. But there's still areas where current laws fall short, and we might need entirely new ones. Understanding how things work and interconnect - that, to me, is incredibly rewarding.
>
> I think that imagination has to do with open-mindedness and being able to connect items, ideas and thoughts. To be able to do that, you need exposure to a set of thoughts. It's like assembling with Lego bricks: if you have only one type of Lego bricks, okay, you can do great things, but there's a limit. But if somebody brings in the different shape of Lego bricks because they work in a different field, your options expand. This also includes methods, ways of working, ways of seeing the world, and knowledge of boundaries... Sometimes, knowing where not to go is just as important, because at the end we try to explain the universe and you can also have imagination or creativity, which just creates things which are completely unreal or impossible.
>
> In research, we're not doing something mechanical or repetitive. So everything you do when you do research, I mean, the moment you actually really advance in a field, you do something that nobody has done before. And the moment you do that, you cannot actually build on existing patterns; you need the minimum of imagination to envision the next step. During my PhD, I studied star clusters to understand stellar evolution. But we realized we could use those clusters to study how galaxies formed - a shift that required imagination. We encountered contradictions with existing theories: galaxies seemed younger than the ancient star clusters they contained. Eventually, we realized young stars outshine old ones, masking older structures. At first, we thought other researchers were simply wrong, but then we asked: what if both views are valid? We had to go through a lot of loops of mind theatre, of imagining and perspectives to reconcile what we saw.

As this excerpt shows, it's not always possible to cleanly separate discovery and justification, at least in part because of the meandering, holistic nature of progress. Most discoveries are partial justifications, and most justifications are partial discoveries (Buzzoni 2015). Insofar as AI does play a justificatory role in space science, the epistemic account cannot claim that AI is always and only a tool for discovery, in order to explain away the lack of attention to opacity by space scientists.

Concerning the noetic account, the lack of mention of opacity is surprising because opacity is thought to frustrate grasp (Beisbart 2021; Janvid 2018; Lenhard 2018; Stuart and Nersessian 2019), and on most accounts, grasp is necessary for objectual understanding. Specifically on Dellsén's account, understanding is achieved when a sufficiently comprehensive dependency model is grasped, and understanding can be improved (and progress made) when that grasp is improved. Perhaps there is a way to avoid this seeming contradiction by augmenting the notion of grasp, for example, by weakening it to something that is merely qualitative (Khalili 2024), or to something akin to an ability, skill, or know-how (Belkoniene 2023; Strevens 2024; 2025; Stuart 2025). But in this case, we are coming closer to functional accounts of progress.

4.3 A new proposal: progress as improving abilities

In light of our data, we wish to propose a new type of functional account of scientific progress. We claim that progress in space science, at least in AI-relevant contexts, concerns improving some relevant scientific ability. This is closely connected to the progress-as-problem-solving account, but importantly different. Solving a single problem is good. This is related to what we called "success," above. But gaining an ability typically enables the solving of many problems and contributes more cumulatively to progress.

Importantly, not all scientific abilities concern problem solving: some relevant abilities might be imaginative or exploratory, e.g., finding new problems, or new concepts, or new methods, even in the absence of a specific problem.[5] We therefore claim that the central notion is scientific ability, not problem-solving or problem-defining. Improving scientific abilities can be epistemically progressive by providing a new ability to solve a particular problem, but it can also be progressive simply by building or improving capabilities. Here is the proposal:

> The ability account of scientific progress: Science makes progress when scientific abilities are improved.

Returning to the beginning of the paper, we wish to briefly sketch how this account might address points (i)-(vii). Who makes progress? The relevant abilities might be possessed by individuals, groups, or distributed across individuals and instruments. What does progress improve? The quality of the scientific abilities of an agent or group. What counts as an "improvement"? Improvement includes the development of new abilities, or the improvement of existing abilities. Improvement might be measured in terms of the increase in precision, robustness, reliability, accuracy (etc.) of the actions/processes that result from exercising those abilities. What is the "bearer" or "vehicle" of progress? Abilities become the unit of analysis. This is different from information stored in a theory or model, or the cognitive-epistemic state of scientists.

[5] Of course, with some creativity, it is possible to interpret any ability as a problem-solving ability. E.g., the ability to move one's hands is a solution to the problem of not being able to move one's hands. However, this seems like an ad hoc strategy for saving the problem-solving version of the functionalist account that is not sufficiently well motivated. Insofar as we are concerned with problems, we restrict our focus to problems that are not just the negation of abilities, but problems which are named by scientists as problems that they are trying to solve.

What is the right scale of analysis? Perhaps it is permissible to conceptualize abilities as being possessed by very large scale communities, or even the human species as a whole, developed over millennia. Or perhaps we should focus only on abilities possessed by single individuals, developed over short timespans. We leave this open. What kinds of progress can we track? On the ability account, there can be as many kinds of progress as there are kinds of ability. We might decide which kinds of abilities are relevant to our analysis of scientific progress depending on choices about the scale of our case study, or what kinds of agents are involved, or whether we are focusing on purely theoretical vs. more applied vs. politically sensitive science. E.g., we might distinguish between mathematical abilities and physical abilities, or between aesthetic, epistemic, ethical, political, and practical abilities. However many ability-types we distinguish, there will be that many types of progress. How is progress related to other issues in philosophy of science? We highlight a few directions in the following.

Before that, three caveats. First, we want to address the possibility that science can make progress in different ways (Chang 2004; Goebel 2019; Rowbottom 2023). It could be progressive for science to increase the truthlikeliness of theories, or to have scientists possess more knowledge and understanding, or to solve more problems, or to increase scientific ability. Perhaps all of these changes to science are good in their own, distinct, yet fundamental ways. So far, the debate has mostly assumed monism (Dellsén forthcoming), but whether we are pluralists or monists, we think the ability account can be defended, either as describing one of the fundamental kinds of progress, or as describing the single fundamental kind of progress.

Second, we should distinguish between instrumental and categorical progress (Rowbottom 2023), or between promoting and constituting progress (Dellsén forthcoming). Following Dellsén, we can say that an event promotes progress to the extent that it raises the chance that future events will constitute progress. For example, a new method might promote progress on the semantic view because it increases the chance of more truthlike theories in the future, even though the event does not constitute progress itself (since it is not itself a more truthlike theory). Proponents of factive accounts of progress will allow that improving scientific abilities can promote progress, but would deny that improving abilities could constitute progress. This possibility does not cohere well with our empirical findings. Against this, we claim that improving scientific abilities can either promote progress or constitute progress. Still, it is fair to ask how improving scientific ability could constitute progress. After all, why are abilities valuable, if not for increasing our stock of truth, knowledge and understanding? We worry that the unintuitive nature of our claim might merely be a felt incongruence with long-ingrained habits in philosophy. Thinking about ability as a fundamental kind of good might require effort, but that effort might be worthwhile. And indeed, there are worries about the value of truth, knowledge, and understanding as well. If these are not themselves grounded in abilities, what are they good for? Finally, as we will argue below, on the noetic account, understanding seems to be composed at least partially of abilities, so improving them would be constitutive of progress, if increasing understanding is.

Finally, the ability account does not, on its own, answer the demarcation problem. Stereotypical scientific abilities like generating good hypotheses, or good experimental designs, or identifying real patterns in data, are built up from simpler abilities like vision, imagination, formal reasoning, physical manipulation, and abstraction, and these latter are also important for other disciplines. If this is right, depending on what level of abilities we focus on, our notion of progress will also apply to other fields. This seems to us a happy consequence. Perhaps art makes progress when artists improve their abilities to express ideas, or stimulate a certain emotional response in an audience, or challenge a tradition. Likewise for society, we might make progress when we improve our abilities to understand one another and treat each other fairly. Having a general account does not preclude our specifying it in ways that interest us. We can easily distinguish cognitive epistemic scientific progress from structural ethical scientific progress, and focus on whichever we like.

4.4 Comparison to other accounts

Let's now consider how this account relates to other philosophical accounts of progress. We'll begin with its closest relative: Shan's functional account. Shan claims that "science progresses if and only if more useful exemplary practices are proposed" (Shan 2022, 50). What does it mean for a practice to be useful? According to Shan, practices are useful iff they propose ways of defining and solving problems, where those ways are new, repeatable, fruitful (lead to new problems), and generalizable. We agree that good practices are often those that enable framing and solving problems, and often, those practices are especially good when they are new, repeatable, fruitful, and generalizable.

However, we deny that progress only ever consists in the proposing of such practices. First, proposing is not always enough: progress also requires taking up proposals and acting on them, as well as in achieving our goals by means of them. On our view, the proposal of useful practices counts as progressive when acting on such proposals leads to improved scientific abilities, or when those proposals make clear how to apply existing abilities in a new way to achieve scientific goals. Second, our account can explain why the virtues listed by Shan (novelty, repeatability, fruitfulness and generalizability) are virtues of practices, while at the same time allowing us to capture counterexamples where a practice is progressive despite lacking one or more of those virtues. Repeatability, fruitfulness, and generalizability can be seen as virtues of practices because they describe good-making features of abilities. Consider generalizability. Scientific abilities that are not generalizable are less good than abilities that are generalizable. If an ability can be applied to many systems in many contexts, the agent who possesses that ability is, in a sense, more able. Though, importantly, even non-generalizable abilities can be progressive, as long as they enable the achievement of some scientifically relevant goal. So generalizability, while a good-making feature of abilities, is not necessary for progress. In terms of fruitfulness, most abilities desired by scientists will be desired because they are fruitful. As we will see below, "being able" to do something is often cashed out in terms of reliably managing to achieve some goal by means of intentional action.[6] Abilities that cannot be repeated and do not achieve some useful function would not count as abilities, according to most accounts of what an ability is. Newness, however, is not a necessary virtue for a proposed practice to be progressive. A proposed practice

[6] We might not want to define ability as a success term. For example, imagination is an ability that is "free" by nature, or at least, freer than most other cognitive processes. So an agent might genuinely have the ability to imagine, yet only imagine in ways that lead to useless ideas, as measured by scientific metrics of success. This helps to motivate fruitfulness as a virtue of scientific proposals, because the improvement of abilities should be fruitful in order to say that progress has been made. And indeed, scientists recognize that imagination might be used in non-fruitful ways, but they value it for its fruitfulness (Stuart 2019; 2022).

might be an old one that we forgot about. Proposing it now can still be a good thing, it can still be progressive, insofar as it improves our current set of abilities.

We agree that Shan's definition allows us to capture many genuine instances of progress, and we agree that the case studies he uses to motivate his account refer to genuine examples of progress. But we think the virtues of progressive episodes that he cites are better explained on our account, that is, by focusing on improved abilities rather than on useful proposed practices.

Nevertheless, we take our account, like Shan's, to be a "functional" account, since we take progress to concern what science can do, rather than concerning the quality of the informational content of science, or the quality of the cognitive relation between epistemic agents and such content. And yet it is not the same as Kuhn and Laudan's original functional account either, because instead of talking about the number or significance of problems solved, we focus on improving abilities, which may or may not be problem-solving abilities. Our account therefore will not face all the objections brought against the traditional problem-solving account. Still, it is worth considering how our account might deal with versions of those traditional objections, and other objections.

4.5 Response to objections

Sometimes scientists appear to develop abilities that should not be associated with progress because they are not "genuine" abilities. Examples include manipulating phlogiston, making phrenological/astrological predictions, and creating the alchemist's panacea. If we cannot differentiate genuine from merely apparent abilities, how can we differentiate between genuine and merely apparent progress? There are several ways we might reply. One is to say that scientists can be wrong about what abilities they have. In some cases, scientists merely *thought* they had developed certain abilities. Of course, in most such cases, there usually *are* some genuine abilities that were developed, and therefore genuine progress that was made. Examples include abilities to measure, infer, and manipulate features of some system. Such abilities typically remain even after the removal of the false theory. As Chang has argued (2012, 53-4), phlogiston theorists were able to get quite a lot done, and it was these abilities that scientists built upon (and continue to build on), even after oxygen theory was introduced. For example, it was through Priestly's efforts to "de-phlogisticate" air that chemists gained the ability to make oxygen (Chang 2022, 152). We might therefore say that scientists really were developing abilities, and thus making progress, and notably, those abilities were central in motivating the turn from phlogiston to oxygen. But the scientists were wrong about the nature of their abilities, and thus about the nature of the progress they were making.

The history of science provides some reason to take this proposal seriously. It is said that the 100 most-cited science papers are methods papers, that is, papers that introduce a method. Scientists may stop citing papers about unobservable entities they no longer take to exist, but they don't stop citing papers that introduce techniques that increase our ability to produce, control, predict, intervene, and measure. As Douglas writes,

> Scientific progress can be defined in terms of the increased capacity to predict, control, manipulate, and intervene in various contexts…While paradigm change can create losses in understanding or losses in explanatory unification as clear conceptual structures are swept away, what is not lost is

> the ability to predict phenomena and/or the ability to control aspects of the world…we are hard pressed to think of a predictive or manipulative capacity that has been lost. (2014)[7]

If abilities always improve or at least remain stable across paradigm shifts, this suggests an answer to the question of how we can be confident that a given ability is a genuine ability. We first differentiate between the quality of an ability as opposed to the quality of a *description* of an ability. Focusing first on the latter, what we want is a reason to believe that our descriptions of abilities are sufficiently accurate. The difficulty is that those descriptions can include reference to unobservable or theoretical entities, and in those cases, for the description to be accurate, those entities must exist as described. The pessimistic metainduction makes it difficult to fully justify such claims. Interpreting things this way, we find ourselves re-creating the realism debate. Anti-realists will claim that we ought not commit ourselves to beliefs about having abilities whose descriptions contain reference to unobservable theoretical entities. Realists will attempt to identify the sorts of theoretical posits that can be trusted to survive theory change and which should thus be included in our ability-descriptions. Finally, pragmatists will hold that realism and anti-realism are merely frameworks or stances (as opposed to truth-evaluable claims about the accuracy of ability-representations) which are only better or worse to the extent that they are useful (Boucher and Forbes 2024). Re-framing the debate in terms of abilities rather than theories and models might be fruitful, and the possibility of such a reframing is, on its own, not a problem for our view. The realism debate will continue in some form or another, and having a version of it focused on ability descriptions does not seem like a bad thing.

However, there is another way to investigate whether an ability is genuine. This way does not focus on the accuracy of our ability-descriptions, but on the connection between our abilities and the world. If "ability" is a success term, there is no such thing as an ability that does not allow us to successfully manipulate some (material, theoretical, mathematical, fictional, etc.) system to achieve some goal.[8] Vetter (2024) distinguishes between two senses of ability: *simple* abilities are all those actions which are possible for an agent to do. In this sense, we're able to do whichever action it is possible for us to do, perhaps in the sense that in at least one possible world, we do it. Then there are *robust* abilities, which concern actions that are, in some sense, within our power to do, in the actual world. It is these latter abilities that epistemologists of ability have turned their interest toward. What explains this power we have to act in ways that reliably achieve some end? Traditionally-minded philosophers of science might argue that the answer lies somehow with the possession of knowledge or truth. There are at least two ways to make such a claim: one, via a metaphysical reduction, and another by telling a story about how the epistemic value of an ability is grounded. On the first strategy, the traditionalist will argue that agents are able to, e.g., measure the value of a variable, because they *know how* to do that, and they know how to do that because they *know that* method x is the way to measure the value of that variable. This reduces abilities ontologically to knowledge. Having an ability just is having some know-how, and having know-how is just having propositional knowledge (e.g., about which

[7] Douglas goes on to say that increased abilities to destroy humans should not count as progress. We should add some ethical limits or constraints on the notion of progress. We agree. The improvement and deployment of abilities should be done responsibly. One way to put this is that epistemic abilities must be developed in connection with ethical abilities. Thinking of the ethics of science in this way might open fruitful new theoretical options (e.g., see Brown 2020).

[8] But see footnote 6. If ability is not a success term, then we can focus instead on what makes abilities successful when they are, without requiring that they always be successful.

actions achieve which aims in which circumstances). The second strategy allows that abilities might be ontologically different from states like propositional knowledge, but contends that an epistemic ability is valuable (when it is) only because the agent has knowledge (or true beliefs) about the target system to which the ability will be applied. Thus, a radiologist may be able to see cancer tumours in computed tomography (CT) scans, and that is an epistemically valuable ability which is different from any states of propositional knowledge. But the reason the radiologist's ability is valuable is because it is justified by background knowledge or true beliefs about, e.g., what cancer tumours look like, how the CT process works, etc. The key move is then to claim that the "real" value of the radiologist's ability is wholly to be located outside the ability itself, e.g., in the propositional background knowledge, or in the new knowledge (or new true belief) that the ability produces (e.g., that this patient's x-rays do or do not display signs of cancer).

Whether these arguments can be made to work is an open question, and we won't pursue them further here. Still, we think new research in the epistemology of ability points in interesting non-reductionist directions that justify independent attention to the epistemic features of abilities, like control, adaptability, reliability, and success (Mayr and Vetter 2023). Improving our abilities seems to be at least one kind of scientific progress, and it seems possible to define, discuss, and measure such progress independently of increases in knowledge and truth. If this is the case, then, to come back finally to the question raised at the beginning of this section, genuine abilities will be those that enjoy the right kind of connection with the target systems that they are applied to, and the goals which they facilitate the achievement of. The epistemology of ability is still at an early stage with respect to specifying the options for how these connections might look. Perhaps it will have to do with embodiment, enactivism, embeddedness, affordances, trial and error, evolution, and much else. And we should not necessarily expect a unified account that works for all abilities, goals, and target systems. In the end, then, it might be possible to tell a story that grounds the (epistemic) value of abilities without reducing those abilities to propositional cognitive states or reducing the value of abilities to the value of propositional cognitive states. And we might even go further, and suggest that it might be possible to tell a story that goes in the other direction: that is, reduces other sources of (epistemic) value to ability. We now turn briefly to this possibility.

There are views of knowledge that define knowledge as useful for, or defined in terms of, ability. For example, Chang's view of "active" knowledge claims that knowledge "is a matter of our ability to engage productively with reality" (2022, 119). It is knowledge "as ability" (2022, 18). If scientific progress consists in the increase of knowledge, and if knowledge just is ability, or if knowledge increases only as abilities improve, then the epistemic account of progress can be wholly or partially explained by (or grounded in) the ability account. In other words, the value of new knowledge reduces to (or is best explained by) the value of new abilities.

The noetic view claims that science makes progress when scientific understanding is increased. The most detailed version of this view is Dellsén's (Dellsén 2016; 2021; 2022; Norton et al. forthcoming; Dellsén et al. 2022). On Dellsén's development, there are three "hallmarks" of such understanding: "the ability for successful predictions," "the ability to formulate successful explanations of some target phenomenon," and achieving "conceptual integration" (i.e., creating a coherent network of dependency relations between elements of some domain) (Dürr and Dellsén forthcoming). Two of these hallmarks are abilities. If there is a way to explain the achievement of conceptual integration in terms of abilities, then (the value of) all three hallmarks could be reduced to (the value of) abilities. In addition, as we saw above, insofar as grasp is involved, this also seems like it might be best characterized in terms of abilities. If this is correct, it seems that the noetic account is already quite close to the ability account, with one main difference being that it defines progress in terms of the improvement of a number of quite specific abilities, rather than improvements in ability in general.

By looking at the broader literature on understanding, we can easily imagine other versions of the noetic account, and many of these will also be friendly to the ability account of progress. For example, it is widely agreed that understanding (whether explanatory, objectual or practical/pragmatic) is, or is characteristically exemplified by, some kind of ability, skill, or cognitive mastery (Le Bihan 2017). One kind of understanding, called practical or pragmatic understanding, is argued to simply be an ability (Delarivière and Van Kerkhove 2021; Toon 2015; Currie 2020; Leonelli 2009; Lenhard 2006; 2009; 2019). For Stuart, this kind of understanding is having a praiseworthy skill, and Stuart leaves open the possibility that skills might be developed abilities (2025). If understanding is, centrally involves, or requires having certain abilities, and the value of understanding can be explained wholly or partially by reference to those abilities, then it makes sense to characterize scientific progress fundamentally in terms of the improvement of those abilities. It seems then that the ability account could explain or ground several different versions of the noetic view of scientific progress.[9]

There are difficult issues lurking in the background here about which we do not have space to go into detail, e.g., the nature and types of abilities, demarcating or counting abilities, and the possibility of cases where abilities might conflict, etc. We leave these for future discussion. Our main goal has been to suggest a new kind of functional account of scientific progress, which we claim deserves to be taken seriously. A corollary is that abilities should be more central than they are to debates about scientific progress.

5. Conclusion

The debate about scientific progress mostly draws on historical case studies, conceptual engineering, thought experiments, and other methods typical of traditional philosophy of science. This paper presents new qualitative empirical data and uses this data to inspire a new non-factivist functionalist account of scientific progress for space science: that science makes progress when it improves its abilities. We also suggest that this account might (at least partially) explain or ground some of the other accounts.

We stress again that our qualitative evidence and resulting account only bear on the general debate about scientific progress insofar as space science is a kind of science. We think it is, but we admit that it has peculiarities that make it special. Still, even if those peculiarities stand in the way of generalization, the set of practices we *can* generalize to will still an important set, perhaps including many engineering science projects (including fusion energy projects, particle collider physics, and large telescope astronomy and cosmology), science involving robotics (including automated chemical and pharmaceutical research), and a good deal of medicine. Given that philosophers have already extended at least some of the traditional

[9] If truth(likeness) can be reduced to ability, the semantic account could also be explained by (or reduced to) the ability account. One might think of some potential candidate strategies for such a reduction (e.g., in the American pragmatists), but we do not pursue the idea further here.

accounts to these contexts, there is room for meaningful debate, at least about those contexts (e.g., the noetic account on progress in medicine and cosmology, see Dellsén et al. 2025 and Dürr and Dellsén forthcoming respectively). Further, as the evidence given above shows, space science is not merely technological: it is also centrally concerned with testing hypotheses (e.g., about the distribution of dark matter in our galaxy), analyzing data (e.g., satellite data for climate research), and refining theories (e.g., ΛCDM), all of which are core practices of science.

Our account claims that we make progress in science by improving scientific abilities. We claim that this account is a good start for making sense of the fact that scientists find AI to be a progressive tool in space science when it helps them solve specific problems or improve their abilities to, e.g., solve problems, find patterns, explore difficult and unknown environments, imagine in useful ways, etc. If this is correct, much more work now needs to be done to bring in existing insights on the nature of ability (from philosophy of action, mind, and epistemology, see, e.g., Vetter and Schoonen forthcoming) to philosophy of science, including about how abilities are distributed across tools and social groups (Toon 2015; Nersessian 2022).

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