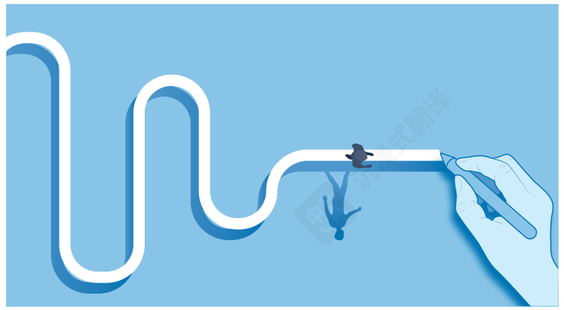
https://doi.org/10.1038/s41562-025-02292-5

How to develop good research questions

Megan A. K. Peters

Check for updates

Designing good research questions goes well beyond the standard definitions of clarity, focus and tractability, and even beyond 'novelty' in the strictest sense. This Comment describes the iterative creative process for designing good research questions, and includes practical suggestions and ways to avoid common traps.



What is a good research question? Ask this question to the internet, a textbook or — heaven forbid — your favourite large language model (LLM), and you will get a standard set of characteristics: clear, focused, relevant, novel and tractable.

But good research questions are not just precise questions that nobody has asked before. They also drive and capture your enthusiasm. They are itchy and persistent, and reflect innovative thinking and novel approaches. Good research questions are often about reframing an unanswered question, challenging previous assumptions or demonstrating how past methods obscured understanding — not just coming up with something focused and new.

Cultivating such questions requires patience, creativity and humility. Here, I will tell you about my process for developing them, including concrete tips and some traps you might encounter (Box 1), to help to equip you to build good research questions yourself.

The phases of development

Phase 1: a self-critical brainstorm. Developing good research questions starts with curiosity. Is there a phenomenon that, upon your learning about it, drove you into a three-hour exploration of the depths of Wikipedia or twenty-five hours of podcasts that you finished within a week? Give yourself permission to be captivated and inspired by something you saw, heard or read.

Next, brainstorm. Find connections without pressure to get it perfect immediately: no idea is 'bad' here. Write down your thoughts, try to be organized and then critically refine to help your initial ideas to mature into good research questions.

That is all much too vague, so here is the expanded, concrete advice.

First, talk to yourself. Yes, literally. And don't just do it sitting on your sofa. Go on a walk or engage in any other 'automatic' activity that you can do without too much linguistic engagement so you can 'tickle' your brain to jumpstart the creative process. My preference is to walk, and to literally talk out loud to myself. Spoken language is more free-form than writing: I can focus on the ideas and connections, and less on pedantically perfecting my terminology or sentence structure. I am not the only one to do this — Dennett used to take long walks and talk aloud to himself as he worked through problems (see page 76 of his memoir, *I've Been Thinking*[1]). If you are worried you will forget all your brilliant ideas as you walk, use your phone to record yourself and transcribe it. My favourite new trick for organizing these stream-of-consciousness transcripts is to use an LLM to summarize them into bullets. But beware: the LLM will often get quite a bit wrong!

Next, critique yourself. Have a (written or spoken) dialogue with your transcript or LLM-created bullet list. Explore flaws in your arguments and ask how others might critique your ideas. If you know who those critics might be, you can look up their work in phase 2. For maximal effectiveness, also talk with people who generally understand your field but who are not necessarily deep topic specialists — you want to be comfortable being wrong, or at least being a non-expert.

A practical way to sharpen these skills is to consistently write down questions and critiques whenever you attend an academic talk or read a paper, going beyond just summarizing to actively engage with your own reactions: What if they had used different controls, asked a slightly different question or tried another analysis? Even if you keep these thoughts private, and even if they are not directly relevant to your immediate questions, developing this habit will improve your critical thinking and questioning skills and make them a natural part of how you engage with science.

Phase 2: building context and connections. Next, dive into the existing body of knowledge. Who has explored similar questions, what were their approaches and what insights were revealed? Your goal is to build a rich, interconnected understanding of your topic.

As you read the literature, do not just summarize; also make explicit links to or notes about other papers or ideas. Try developing your own shorthand or markdown-style tags — for example, I tend to write "#connection" and then a short note to myself. Avoid the temptation to fully rely on automated tools such as ResearchRabbit, Connected Papers or LLMs to summarize or make connections here, because they will not see the connections that you do — so use them sparingly, and only to assist you in curating the growth of connections within your own mind-map. Obsidian is a great, free, cross-platform application for linking ideas and then easily visualizing connections through its built-in graph tool. Also, tag papers as 'primary' (most closely related to your likely question) or 'secondary', including your reasons for doing so; this will help you in phase 3. I also find it helpful

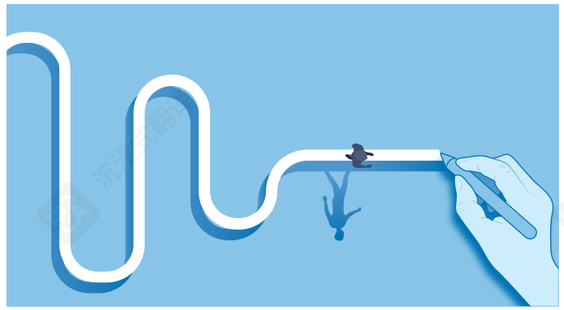
https://doi.org/10.1038/s41562-025-02292-5

如何发展良好的研究问题

Megan A. K. Peters

检查更新

设计好的研究问题远不止清晰、聚焦和可操作性这些标准定义，甚至超越了严格意义上的'新颖性'。本评论描述了设计好的研究问题的迭代创意过程，并包含实用建议和避免常见陷阱的方法。



什么是好的研究问题？向互联网、教科书或——天哪——你最喜欢的语言模型（LLM）提出这个问题，你会得到一套标准的特征：清晰、聚焦、相关、新颖且可操作。

但好的研究问题不仅仅是以前没人问过的问题。它们还能激发并捕捉你的热情。它们令人挠头且坚持不懈，反映了创新思维和新的方法。好的研究问题通常涉及重新定义一个未解决的问题，挑战之前的假设或展示过去的方法如何掩盖了理解——而不仅仅是提出一个聚焦且新颖的东西。

培养这类问题需要耐心、创造力和谦逊。在这里，我将分享我开发它们的过程，包括具体的建议和一些你可能会遇到的陷阱（框1），以帮助你自己构建好的研究问题。

开发阶段

第一阶段：自我批判性头脑风暴。开发好的研究问题始于好奇心。是否存在某个现象，在你了解它之后，驱使你深入探索维基百科的深渊长达三个小时，或者在一周内听完二十五小时的有声读物？允许自己被所见、所闻或所读的事物所吸引和启发。

接下来，进行头脑风暴。寻找联系，无需立即追求完美：在这里，没有"坏"的点子。写下你的想法，尝试保持条理，然后进行批判性完善，帮助你的初步想法成熟为好的研究问题。

那太模糊了，所以这里提供更详细、具体的建议。

首先，要和自己说话。是的，字面意义上的和自己说话。而且不要只是坐在沙发上做这件事。去散步或者进行任何其他'自动'活动，这些活动不需要太多语言参与，这样你就能'刺激'你的大脑，启动创意过程。我的偏好是散步，并且字面意义上大声地和自己说话。口语比写作更自由：我可以专注于想法和联系，而不用过分拘泥于术语或句子结构的完美。我并非唯一这样做的人——丹尼尔·丹尼特过去常常长时间散步，在解决问题时大声自言自语（参见他回忆录第76页，我曾思考[1]）。如果你担心在散步时会忘记所有绝妙的想法，可以用手机录下自己并转录下来。

我组织这些意识流转录的最佳新技巧是使用大型语言模型（LLM）将它们总结成要点。但要注意：LLM经常会犯很多错误！

接下来，自我批评。与你的文本记录或LLM生成的要点列表进行（书面或口头）对话。探索你论证中的缺陷，并询问他人可能会如何批评你的想法。如果你知道这些批评者是谁，你可以在第二阶段查找他们的作品。为了达到最大效果，还要与那些通常了解你的领域但未必是深度主题专家的人交谈——你希望能够在犯错时感到舒适，或者至少能够成为一个非专家。

要在这些技能上精进，一个实用的方法是：每次参加学术讲座或阅读论文时，都要坚持写下问题和评论，而不仅仅是总结，要主动与自己的反应互动：如果他们使用了不同的对照组、问了一个稍微不同的问题或尝试了另一种分析会怎样？即使你将这些想法保密，即使它们与你当前的问题没有直接关系，养成这种习惯也能提升你的批判性思维和提问能力，并使它们成为你参与科学研究的自然部分。

第二阶段：构建背景和联系。接下来，深入研究现有的知识体系。谁探讨了类似的问题？他们的方法是什么？他们得出了哪些见解？你的目标是建立对主题丰富且相互关联的理解。

在阅读文献时，不要仅仅进行总结；还要明确地链接到或记录其他论文或想法。尝试开发你自己的简写或类似Markdown风格的标签——例如，我倾向于写"#连接"，然后附上一则简短的自我笔记。避免完全依赖自动化工具，如ResearchRabbit、Connected Papers或LLMs，来总结或建立联系，因为它们无法看到你所见的联系——因此要谨慎使用它们，仅用于协助你在自己的思维导图中整理联系的生长。Obsidian是一款优秀的、免费的、跨平台的软件，用于链接想法，并通过其内置的图形工具轻松可视化联系。此外，将论文标记为"主要"（与你可能的问题最密切相关）或"次要"，并说明理由；这将有助于你在第3阶段。我也发现这很有帮助

BOX 1

How to develop good research questions

The process:

- Phase 1: a self-critical brainstorm. Let your ideas flow freely, and engage in self-dialogue to identify and work through weaknesses or disconnects.
- Phase 2: building context and connections. Read deeply and widely, both within a closely targeted literature and across disciplines, to identify promising approaches, creative techniques and novel perspectives. Practice critiquing as you go.
- Phase 3: distilling the essence. Hone your possible research questions through critically evaluating the most relevant previous work, bringing perspectives from your wide literature review.
- Phase 4: the final product. Identify a targeted, precise, novel question and the best approaches to answering it.

The traps:

- Trap 1: the 'hypothesis requirement'. Predictions and expectations can be powerful tools, but you do not always need a specific hypothesis; sometimes exploratory or descriptive research is the key to new insight.
- Trap 2: excessive attachment to your question(s). Do not fall prey to a sunk cost fallacy by clinging to your original question if you discover it is not quite right! Sometimes cycling back to an earlier stage is necessary.
- Trap 3: 'oh no, someone already had my idea!' Don't panic: surely, their idea was not exactly the same as yours. Learning to find the small but critical differences between previous work and your own approach is a key component to developing good research questions.
- Trap 4: the 'hammer and nail' trap. Your area of expertise is an important tool and gives you novel perspective, but do not let it stifle the scope of your research questions and approaches.

to keep a separate, 'overview'-type running document for my stream of consciousness to capture nascent thoughts, emerging connections and questions that arise as I engage with new material.

As you do this, resist the temptation to immediately narrow your reading list to only the seemingly most-relevant papers. Papers from outside of your immediate area or even field can be valuable and inspiring. I try to read widely, including in philosophy of mind and philosophy of science in addition to my core foci in metacognition, cognitive science and computational neuroscience. This interdisciplinary background has helped me to spot connections that others might miss, and leads to new questions or innovative solutions. Sometimes, I have discovered problems in distant fields that share the same underlying 'shape' as challenges I am facing, even if the surface details seem different. My group's recent *Science Advances* paper[2], for example, emerged from recognizing commonalities in challenges faced by human microbiome research and computational cognitive neuroscience, despite their 'surface level' dissimilarity.

Phase 3: distilling to the essence. Now, let us refine. Your goal should be to strip away the non-essentials to define a concrete set of circumstances through which your question might be addressed.

To do this, return to and engage more deeply with the papers that are most closely related to your evolving idea – beginning with the ones you marked as 'primary' before. From these, identify any papers that come close to what you envision. Now, read them again, but this time, with a specific goal: actively look for opportunities to ask "What if they had done [some other thing] instead of what they actually did?"

This 'some other thing' can vary widely: a different analytical approach, control condition, stimulus set, data collection technique, target population or task. It could be a subtle tweak or a major departure. Critically, the goal is not to immediately select a single 'other thing', but rather to generate a multitude of alternative explanations or experimental designs that could extend or challenge existing findings. This systematic questioning will help you to identify your unique, refined question.

Phase 4: the final product. Finally, you can ask yourself which of these many options for 'some other thing' will determine your research question, given your resources, expertise and the current state of the field (synthesized in phase 2). As you specify the paradigms, tools and approaches you will use to answer your question, try to also identify all other plausible alternative explanations for what you or others might find. By systematically considering potential confounds and alternative interpretations, you can better design your experiments to rule those out, or at least be clear about them in your future writing.

Et voilà! Now, you have a good research question – one that is not just clear, focused and tractable, but one that also reflects the novelty and creativity that led to its generation and your drive to answer it.

Traps to avoid

In developing (hopefully) good research questions, I have also run into many traps. Here, I'll share a few to help you avoid them.

Trap 1: the 'hypothesis requirement'. A common misconception is that every good research question must be accompanied by a specific, falsifiable hypothesis: for example, that your intervention will produce a particular outcome. Not always true! Yes, you want to have some specific expectations, but they might not take the form of an exact hypothesis about an effect size or comparison against placebo. Sometimes, your goal might simply be description: "What do people do in XYZ situation?" or "What happens if … ?". Alternatively, you may seek a mechanistic explanation such as "How does effect ABC come about?". There are also normative questions such as "Why do organisms do X and not Y?". Tackling these 'what', 'how' and 'why' type questions can provide different, valuable insights, even in the absence of specific, falsifiable hypotheses[3,4].

Trap 2: excessive attachment to your question(s). Developing good research questions takes time, so it is easy to fall into a sunk cost spiral and become overly attached. You might discover late in this process that your question is not as insightful, precise or interesting as you first believed. But after you put in so much time and effort,

框1

如何提出好的研究问题

流程：

- 第一阶段：自我批判性头脑风暴。让你的想法自由流动，并参与自我对话，以识别并解决弱点或脱节之处。
- 第二阶段：构建背景和联系。深入广泛地阅读，既要针对特定文献，也要跨学科，以识别有前景的方法、创造性的技巧和新颖的视角。在实践中练习批判性思考。
- 第三阶段：提炼核心。通过批判性地评估最相关的

  先前工作，结合你广泛文献综述中的视角，来打磨你的可能研究问题。
- 第四阶段：最终成果。确定一个有针对性、精确、新颖的问题，以及回答它的最佳方法。

陷阱：

- 陷阱1：'假设要求'。预测和期望可以是强大的工具，但你并不总是需要一个具体的假设；有时探索性或描述性研究才是获得新见解的关键。
- 陷阱2：过度执着于你的问题。如果你发现它不太对劲，不要陷入沉没成本谬误，坚持原来的问题，如果你

  发现它并不完全正确！有时回到早期阶段是必要的。
- 陷阱3：'哦不，有人已经有了我的想法！'不要恐慌：当然，他们的想法并不完全和你的一样。学会发现先前工作和你自己的方法之间微小但关键的差异，是发展良好研究问题的关键组成部分。

  是发展良好研究问题的关键组成部分。
- 陷阱4：'锤子和钉子'陷阱。你的专业领域是一个重要的工具，能给你新的视角，但不要让它扼杀你的研究问题和方法的范围。

要保留一个独立的、'概述'类型的运行文档，用于捕捉我在接触新材料时产生的直觉想法、涌现的联系和问题。

当你这样做时，要抵制住立即将你的阅读清单缩小到仅限于看似最相关的论文的诱惑。你所在领域之外甚至不同领域的论文也可能是宝贵且富有启发性的。我尽量广泛阅读，除了我元认知、认知科学和计算神经科学的核心关注点之外，还包括心智哲学和科学哲学。这种跨学科背景帮助我发现了别人可能忽略的联系，并引出了新的问题或创新的解决方案。有时，我在遥远领域中发现的问题，尽管表面细节不同，但其潜在"形状"与我所面临的挑战是相同的。例如，我的团队最近发表在《科学进展》上的论文[2]，，就源于认识到人类微生物组研究和计算认知神经科学所面临的挑战中的共性，尽管它们在"表面层面"看起来是不同的。

第三阶段：提炼核心。现在，让我们来精炼。你的目标应该是剥离非本质内容，以定义一组具体的情境，通过这些情境，你的问题或许可以得到解答。

为此，请回到与你不断发展的想法最密切相关的论文中，并更深入地参与其中——从之前标记为"主要"的那些论文开始。从这些论文中，找出那些接近你所构想的论文。现在，再次阅读它们，但这次要有一个明确的目标：主动寻找机会提出"如果他们当时做了[其他事情]而不是他们实际做的事情呢？"

这'其他事物'可能差异很大：不同的分析方法、控制条件、刺激集、数据收集技术、目标人群或任务。它可能是一个微小的调整，也可能是一个重大的偏离。关键在于，目标不是立即选择一个'其他事物'，而是生成多种替代解释或实验设计，以扩展或挑战现有发现。这种系统性的质疑将帮助你确定你独特、精细的问题。

第4阶段：最终成果。最后，你可以问自己，在考虑了你的资源、专业知识和当前领域状态（在第2阶段综合）后，这些许多"其他事情"的选项中，哪一项将决定你的研究问题。当你明确你将用来回答问题的范式、工具和方法时，尝试同时识别所有其他可能的替代解释，即你或他人可能发现的内容。通过系统地考虑潜在的混杂因素和替代解释，你可以更好地设计你的实验来排除它们，或者至少在未来写作中明确说明它们。

好了！现在，你有一个好的研究问题——一个不仅清晰、集中且可操作的问题，而且它也反映了导致其产生以及你回答它的创新性和创造力。

需要避免的陷阱

在发展（希望）好的研究问题的过程中，我也遇到了很多陷阱。在这里，我将分享一些来帮助你避免它们。

陷阱 1：所谓的"假设要求"。一个常见的误解是，每个好的研究问题都必须伴随着一个特定的、可证伪的假设：例如，你的干预措施会产生特定的结果。这不总是真的！是的，你想要有一些具体的期望，但它们可能不是关于效应量或与安慰剂进行比较的确切假设的形式。有时，你的目标可能仅仅是描述："在 XYZ 情况下人们做什么？"或者"如果……会怎样？"。或者，你可能寻求机制性解释，例如"效应 ABC 是如何产生的？""。还有规范性问题，例如"为什么生物体做 X 而不是 Y？""。解决这些"什么"、"如何"和"为什么"类型的问题，即使在没有特定的、可证伪的假设的情况下，也能提供不同、有价值的见解[3,4]。

陷阱2：过度执着于你的问题。好的研究问题需要时间来发展，因此很容易陷入沉没成本螺旋并变得过于执着。你可能会在这个过程的后期发现，你的问题并没有你最初认为的那样有见地、精确或有趣。但当你已经投入了这么多时间和精力之后，

the temptation to persist rather than scrapping and starting over can be very strong.

Try to resist and instead embrace iteration. Maybe you already answered your initial question through extensive reading, and new studies or analyses are no longer necessary. Or maybe your question lacked the necessary precision, and in refining it, you have naturally shifted towards asking something entirely different. Do not be afraid to abandon your question. To avoid feeling like you are totally starting over, return to your critical dialogue from phases 1 and 2.

Trap 3: 'oh no, someone already had my idea!'. If it has not already happened, inevitably at some point in your career you will discover someone has already asked and answered a question that seems identical to yours. Naturally, you will feel disappointed (trust me, I have been there). But try to resist full-on despair: science builds upon others' discoveries, so be proud that your question was actually so good that someone else already explored a similar area.

And it is rare that a question is exactly the same. More often, previous work used different techniques, overlooked alternative explanations or targeted a slightly different phenomenon. There is almost always an opportunity for refinement. As you engage with a paper that 'did exactly what you wanted to do', deploy the critiquing skills you have been honing. Would you have approached it exactly the same way? Probably not. Identify how you would improve upon what was done. I bet you will be able to transform what appears to be a setback into an opportunity.

Trap 4: the 'hammer and nail' trap. As you develop deeper expertise, you will naturally view new problems through the familiar lens of your own niche. But, as the saying goes "If you have a hammer, everything looks like a nail". Your expertise can lead you to rigidly and indiscriminately apply your 'hammer', which can blind you to alternative tools or approaches that might be more suited to your question.

However, instead of completely avoiding applying your 'hammer', reframe this trap as an opportunity. Few others possess your specific specialized knowledge with active self-reflection. Continuously ask: "Is my preferred method truly the best way to approach this specific question, or merely the easiest for me? How can my 'hammer' provide novel insights?". This way you can exploit your strengths while avoiding the 'hammer and nail' trap.

Final thoughts

Developing a good research question is not a flash of genius or insight; it is an ongoing, iterative process fuelled by curiosity, organization and constant self-evaluation. Here I have laid out a multiphase approach that works for me as well as some traps to avoid, moving from big ideas and deep dives into what is already known, to sharpening questions and designing specifically how to answer them. I hope this process works for you too!

Megan A. K. Peters [1,2,3,4,5] ✉

[1]Department of Cognitive Sciences, University of California Irvine, Irvine, CA, USA. [2]Department of Logic & Philosophy of Science, University of California Irvine, Irvine, CA, USA. [3]Center for the Neurobiology of Learning & Memory, University of California Irvine, Irvine, CA, USA. [4]Center for Theoretical Behavioral Sciences, University of California Irvine, Irvine, CA, USA. [5]Program in Brain, Mind, & Consciousness, Canadian Institute for Advanced Research, Toronto, Ontario, Canada.
✉e-mail: megan.peters@uci.edu

Published online: 11 August 2025

References

1. Dennett, D. C. *I've Been Thinking* (WW Norton, 2025).
2. Orouji, S., Liu, M. C., Korem, T. & Peters, M. A. K. *Sci. Adv.* **10**, eadp6040 (2024).
3. Dayan, P. & Abbott, L. F. *Theoretical Neuroscience* (MIT Press, 2005).
4. Neuromatch. Introduction — Neuromatch Academy: Computational Neuroscience. *neuromatch.io* https://compneuro.neuromatch.io/tutorials/intro.html (2020).

Acknowledgements

I thank G. Blohm for helpful comments on early versions of this piece.

Competing interests

The author declares no competing interests.

坚持而不是放弃重来的诱惑可能非常强烈。

尝试抵制诱惑，转而拥抱迭代。也许你早已通过广泛阅读回答了最初的问题，因此新的研究或分析不再必要。或者也许你的问题缺乏必要的精确性，在完善它的过程中，你自然而然地转向了完全不同的问题。不要害怕放弃你的问题。为了避免感觉自己完全要从头开始，回到第1阶段和第2阶段的关键对话。

陷阱3：“哦不，有人已经有了我的想法！”如果还没有发生，那么在你的职业生涯中，你不可避免地会发现有人已经提出了并回答了一个与你似乎完全相同的问题。你自然会感到失望（相信我，我经历过）。但尝试抵制彻底的绝望：科学建立在他人的发现之上，因此为你提出的问题实际上如此优秀而感到自豪，

因为另一个人已经探索了类似的领域。而且问题完全相同的情况很少见。更常见的是，以前的工作使用了不同的技术，忽略了替代解释，或者针对稍微不同的现象。几乎总有机会进行改进。当你阅读一篇“完全符合你想要做的事情”的论文时，运用你一直在磨练的批判性技能。你会以完全相同的方式处理它吗？很可能不会。找出你如何改进已经做过的事情。我敢打赌，你将能够将看似挫折的事情

转化为一个机会。

陷阱4：“锤子与钉子”陷阱。当　　y你发　　p deep展　p专长，你自然会通过自己熟悉的领域视角来看待新问题。但是，俗话说：

“如果你手里有一把锤子，
看起来像钉子。您的专业知识可以引导您严格且不加区分地
　　y apply y我们的‘锤子’，它可能会让人目眩　y转向替代工具或
可能更适合您问题的方法。

然而，与其完全避免使用你的‘锤子’，不如将这个陷阱重新定义为机会。很少有人拥有你独特的专业知识（和思维导图），因此你可以以他人未曾考虑的方式提问和回答问题。所以，将你的专业知识与积极的自我反思相结合。不断自问：‘我的首选方法真的是解决这个具体问题的最佳方式，还是仅仅对我而言最容易？我的‘锤子’如何能提供新颖的见解？’这样你就能发挥你的优势，同时避免‘锤子与钉子’的陷阱。

最后思考

提出一个良好的研究问题不是灵光一闪或洞察力；它是一个由好奇心、组织能力和持续自我评估驱动的持续迭代过程。我在这里概述了一种适用于我的多阶段方法，以及一些需要避免的陷阱，从大的想法和深入已知领域，到打磨问题并设计如何回答它们。我希望这个过程对您也有效！

Megan A. K. Peters [1,2,3,4,5] ✉

[1]认知科学系，加州大学欧文分校，欧文，CA，美国。[2]逻辑与科学哲学系，加州大学欧文分校，欧文，CA，美国。[3]学习与记忆神经生物学中心，加州大学欧文分校，欧文，CA，美国。[4]理论行为科学中心，加州大学欧文分校，欧文，CA，美国。[5]大脑、心智与意识项目，加拿大高级研究所，多伦多，安大略省，加拿大。电子邮件：megan.peters@uci.edu

在线发表：2025年8月11日

参考文献

1. Dennett, D. C.《我一直在思考》（WW Norton，2025）。
2. Orouji, S., 刘明才, Korem, T. & Peters, M. A. K. Sci. Adv. 10, eadp6040 (2024). . . . . .
3 Dayan, P & Abbott, L F *Theoretical Neuroscience* (MIT Press, 2005)
4. Neuromatch. Introduction — Neuromatch Academy: Computational Neuroscience. *neuromatch.io* https://compneuro.neuromatch.io/tutorials/intro.html (2020).

致谢

感谢 G. Blohm 对本作品早期版本提出的宝贵意见。

利益冲突

作者声明无利益冲突。