


# How to develop good research questions

Megan A. K. Peters

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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.

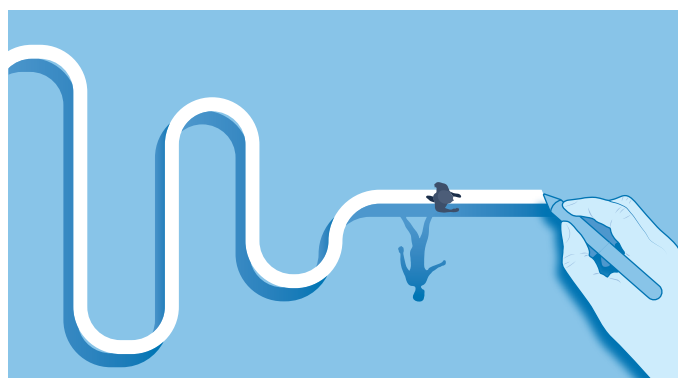
## The phases of development

**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*<sup>1</sup>). 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

## BOX 1

### Development process and traps to avoid

#### 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<sup>2</sup>, 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<sup>3,4</sup>.

**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,

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 (and mind-map), so you can ask and answer questions in ways others might not have considered. So, pair your

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** <sup>1,2,3,4,5</sup> 

<sup>1</sup>Department of Cognitive Sciences, University of California Irvine, Irvine, CA, USA. <sup>2</sup>Department of Logic & Philosophy of Science, University of California Irvine, Irvine, CA, USA. <sup>3</sup>Center for the Neurobiology of Learning & Memory, University of California Irvine, Irvine, CA, USA. <sup>4</sup>Center for Theoretical Behavioral Sciences, University of California Irvine, Irvine, CA, USA. <sup>5</sup>Program in Brain, Mind, & Consciousness, Canadian Institute for Advanced Research, Toronto, Ontario, Canada.

 e-mail: [megan.peters@uci.edu](mailto:megan.peters@uci.edu)

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## Competing interests

The author declares no competing interests.