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Amanda S. Fernandez
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1 Introduction

> If a man will begin with certainties, he shall end in doubts;
> but if he will be content to begin with doubts, he shall end in certainties.
>
> — Francis Bacon

In 1975, George H. Heilmeier stepped into one of the most challenging leadership roles in American research: Director of the Defense Advanced Research Projects Agency (DARPA). The agency was already legendary for funding breakthrough technologies that seemed like science fiction – computer networks that could survive nuclear attacks, artificial intelligence (AI) systems that could reason about complex problems, and computing machines with capabilities that commercial industry couldn't imagine. Yet Heilmeier quickly discovered that, for every transformative success story, there were dozens of projects that consumed enormous resources while producing little meaningful progress.

The pattern that emerged troubled him deeply. Brilliant researchers with access to unlimited funding and cutting-edge equipment would spend years pursuing technically sophisticated approaches that ultimately led nowhere. This was not because they lacked talent or resources, but because they had never clearly articulated what fundamental problem they were trying to solve. Teams would optimize database query performance by fractions of a percent while users struggled with entirely different bottlenecks. Cryptographers would develop mathematically elegant security protocols that protected against threats no one actually faced. Computer scientists would build distributed systems with impressive theoretical properties that failed to address the real-world constraints their users encountered daily. These scientific contributions were

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essential for long-term innovation, but project timelines and uncertain outcomes can appear inefficient through a business lens.

Heilmeier realized that the traditional approach to research proposal evaluation in this context was fundamentally broken. Committees would scrutinize technical methodologies, evaluate the credentials of research teams, and assess the feasibility of proposed implementations, but rarely questioned whether the underlying problem was worth solving in the first place. This led to what he termed "solution-driven research" – work that began with fascinating technical capabilities and then searched for applications, rather than starting with pressing problems and developing appropriate solutions.

The insight that transformed Heilmeier's approach came from observing how successful projects actually worked. The researchers behind the Advanced Research Projects Agency Network (ARPANET) didn't start with a desire to build faster networking protocols; they began with the concrete problem of enabling reliable communication between geographically distributed research teams. The pioneers of computer graphics didn't set out to develop more sophisticated rendering algorithms; they wanted to help engineers visualize complex three-dimensional problems that were impossible to understand through traditional blueprints and equations. In each case, clear problem definition drove methodological choices rather than the reverse.

From this observation, Heilmeier developed what would be known as the Heilmeier Catechism, a deceptively simple set of questions that every research proposal had to answer convincingly:

- What are you trying to do, and why does it matter?
- How do people solve this problem today, and where do current approaches fall short?
- What makes your approach different, and why should anyone believe it will succeed?
- Who will benefit from your solution, and how will you know whether you've succeeded?

These questions seemed almost trivial compared to the sophisticated technical discussions that typically dominated research planning, yet

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they proved remarkably effective at distinguishing meaningful work from elaborate technical exercises.

The power of this framework became evident not in what it approved, but in what it prevented. Projects that couldn't clearly articulate their fundamental purpose were forced to either refocus on genuine problems or abandon their pursuit entirely. Research teams that had been drifting through technical possibilities suddenly found themselves confronting hard questions about impact and validation. The catechism acted as a filter that separated research driven by intellectual curiosity about genuine problems from research driven by fascination with technical capabilities.

More importantly, the process of answering Heilmeier's questions naturally led researchers toward more rigorous methodological approaches. When you understand exactly what problem you're solving and for whom, the appropriate evaluation criteria become obvious. When you can articulate how your approach differs from existing solutions, you can design experiments that test those specific differences rather than generic performance metrics. When you know who cares about your problem and why, you can construct validation studies that demonstrate real-world impact rather than laboratory curiosities.

The methodological implications extended far beyond project selection. Research teams that worked through the catechism found themselves making fundamentally different choices about experimental design, data collection, and result interpretation. Instead of asking "How can we show that our system performs better?" they began asking "How can we demonstrate that our solution addresses the specific limitations that motivated this work?" This shift in perspective led to more focused experiments, more meaningful baselines, and more honest assessment of both successes and failures.

Heilmeier's influence spread far beyond DARPA's walls because his approach addressed challenges that plague research institutions everywhere. Academic researchers face constant pressure to demonstrate novelty and technical sophistication, often at the expense of

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practical significance. Industry researchers must balance intellectual rigor with commercial demands, sometimes losing sight of the fundamental problems that drive business value. In both contexts, the temptation to begin with interesting technical approaches and work backward toward applications creates research that is technically sound but ultimately inconsequential.

The enduring relevance of the Heilmeier Catechism reflects a deeper truth about research methodology in computing: The most sophisticated analytical tools and experimental techniques are worthless if applied to the wrong problems. Modern computing offers researchers an overwhelming array of methodological choices: machine learning frameworks, distributed systems architectures, formal verification techniques, user study designs, and performance evaluation approaches. Without clear problem definition to guide these choices, researchers can easily become lost in methodological complexity, producing work that demonstrates technical competence but lacks coherent purpose. Ultimately, a robust research agenda hinges on a clear articulation of the fundamental challenge being addressed.

For students beginning their research careers, understanding this relationship between problem formulation and methodological design proves essential. The discipline of clearly articulating research questions before selecting analytical approaches develops the kind of critical thinking that distinguishes productive researchers from those who merely apply sophisticated tools to arbitrary problems. More fundamentally, it cultivates the intellectual honesty to distinguish between problems worth solving and problems that are merely solvable, which is a distinction that ultimately determines whether research contributes to genuine understanding or merely adds to the accumulation of technical literature.

This is why research design and methodology cannot be treated as secondary concerns, applied after the "real work" of problem solving begins. They are the foundation upon which meaningful research is built, the lens through which genuine problems are distinguished

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from technical diversions, and the framework that ensures research energy flows toward questions that actually matter.

1.1 WHAT ARE YOU TRYING TO DO, AND WHY DOES IT MATTER?

This book is for anyone who wants to move beyond curiosity and confidently produce impactful research in computing.

Whether you're a student, a professional, or a lifelong learner, this book will be your guide to building a research project from the ground up. We provide a clear framework for tackling complex investigations, from formulating your initial concept to designing, executing, and reflecting on your work.

You'll learn to develop a critical approach to the research process and translate your curiosity into measurable outcomes. The hands-on activities in each chapter will guide you through your chosen interests, revealing existing open problems and demonstrating why solutions to them matter.

We believe that great research starts not with answers, but with the right questions. This book is for those ready to find them.

1.2 AI: YOUR RESEARCH ASSISTANT

As you develop new research skills with each chapter, and exercise the skills through the activities, a section will further recommend practices for leveraging AI. The capabilities of large language models (LLMs) have exploded in recent years, facilitating tasks unimaginable even five years ago. The idea that a neural network could demonstrate creativity, for example, was blatantly discouraged.

However, AI *can* create and is a resource that should not be overlooked in your research. It is not a substitute for the research skills in this book. You will still need to be the trained researcher. But AI can serve as your *research assistant* – so get ready to delegate!

Artificial intelligence models are adept at summarizing large amounts of information visualizing data, and making recommendations. You just need to know *what to ask for (and how to ask for it!).*

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A section in each chapter is dedicated to this end – ensuring that you, the training researcher, makes the most of your (new?) AI assistant, to optimize everything from your writing to your schedule.

Further, you are becoming the expert here, so while AI can supplement your efforts, these models can only go so far. In the absence of a definitive answer, these models *infer* based upon existing knowledge. When this is done, the potentially incorrect assumption made by the model is still presented as absolute fact, and is referred to as "hallucination." Hallucinations are particularly dangerous when taken at face value, so it will be your job as the "lead researcher" in the relationship to verify and take ownership of your research. These sections throughout this book will develop those skills to help you work effectively with your new research collaborator(s).

1.3 ACTIVITY: WHAT INTERESTS YOU?

Now, where to start? This first activity is an exploration of interests. Your interests. Successful exploration of the unknown often starts with genuine curiosity. So, what interests you?

Learning Objectives

- Reflect on your personal interests and align them with computing research themes.
- Learn from active research topics and further hone interests.

1.3.1 Step 1: Getting Started

The flowchart in Figure 1.1 provides initial questions to get you thinking. Based on your responses, fields of study are recommended. Note that these are simply a starting point – we're just at the tip of the iceberg. In addition, you'll likely encounter a question where you aren't clear of your own answer – that's OK! Go through the chart as many times as needed. Remember: Spending time *now* to identify where your interests lie will help *save* you time in your research.

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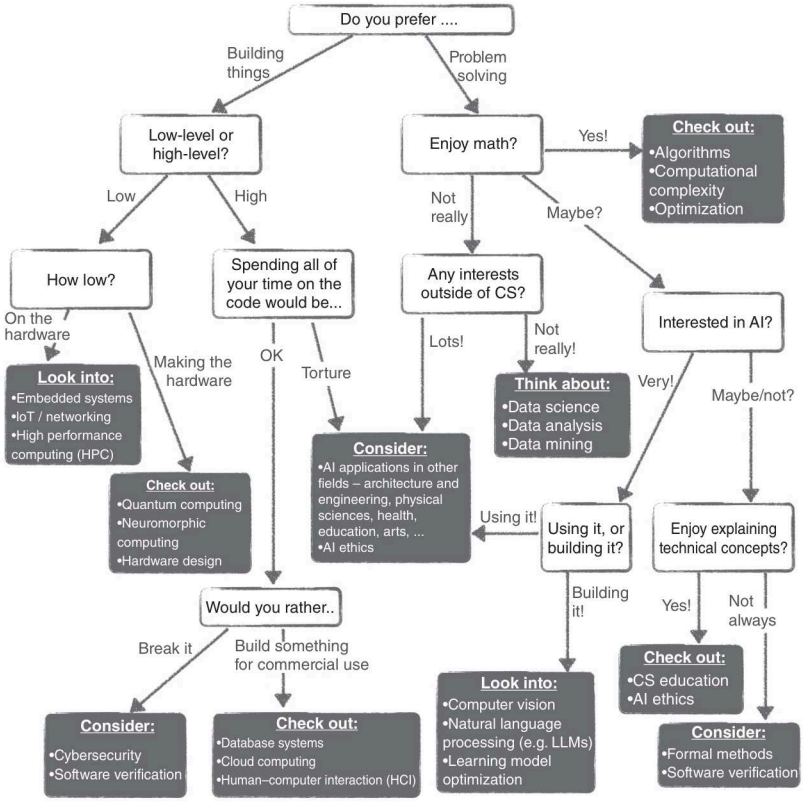

FIGURE 1.1 Activity 1 Step 1: Flowchart for exploring research fields of interest, across fields within computing.

Instructions Beginning at the top, answer the questions (*honestly!*) to discover which computing research fields might be of interest. **Circle** the filled boxes that correspond with a good match to your answers, and **cross out** any that are not of interest. Leave the rest unmarked.

Reflection Complete the flowchart as many times as necessary. Now take a step back – do you see any overall trends? Maybe there are common themes among your circled boxes. Or commonalities amongst those you've crossed out. Knowing what you are *not* interested in is as important as identifying a field of interest.

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Building a secure and scalable decentralized cloud storage system	Designing secure communication protocols that resist vulnerabilities like eavesdropping, data manipulation, and denial-of-service attacks	Toward secure and reliable quantum computing hardware	Constructing consistent digital line segments
Investigating resource sharing and trading mechanisms between edge nodes to optimize utilization	Designing feedback mechanisms that provide meaningful and actionable insights to support student learning and program improvement	Efficiently segmenting images with texture	The computational complexity of quantum annealing
Developing efficient algorithms and hardware architecture for performing machine learning tasks directly on embedded devices	Establishing fairness metrics for AI models	Developing efficient data partitioning and aggregation techniques for distributed ML architectures	Leveraging explainable AI for anomaly detection large datasets
Leveraging explainable AI for real-time medical diagnosis	Building privacy-preserving data mining solutions for bioinformatics	Developing new techniques for visualizing and interacting with real-time data streams in AR/VR environments	Designing distributed and parallel data mining algorithms for processing large datasets on cloud platforms

FIGURE 1.2 Activity 1 Step 2: A grid of sample paper titles across topics in computing.

What topics have you identified a curiosity for? Which have you ruled out? Write down the key words you've identified so far – our goal will be to either connect them to discover a specific niche for your research, or further refine topics to ensure sustainable interest.

1.3.2 Step 2: A Peek into Existing Work

The grid in Figure 1.2 contains titles of research papers across a breadth of computing fields. The premise of this step is simple: If you are uninterested in the title of a paper, it is unlikely you will be interested enough to read through its technical depth. Take your time reading each title, asking yourself if you are *curious* enough to want to learn more.

Circle any titles you might want to read more about right now, and **cross out** any that are not of interest. Leave the rest unmarked.

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Diagram 1.1. *Spatial organization of themes in Figure 1.2.*

Systems	Theory
Applications	Data

Reflection Once you have read each title and made some decisions, come back to this reflection. Once again, can you identify any common themes amongst the titles you've circled, or crossed out?

The grid is organized such that there are spatial themes, as shown in Diagram 1.1.

Now notice, *does your grid have more circles or crosses in a certain quadrant?* More circles in the upper left, for example, suggest you may explore systems-related fields and topics. More crosses in the upper right recommend concepts closer to the theoretical than the applied. Review your responses in light of these general categorizations, and notice any trends.

1.3.3 Step 3: Identify Your Key Words

Which of the previous two steps was more helpful in identifying fields of potential interest to you? *Why?* These exercises will resonate with everyone differently (and with you in various stages of your research experience). Your level of expertise, prior research work, exposure to various fields, and general knowledge will influence your curiosity toward a topic differently over time.

Write down two or three key words or phrases (from steps 1 or 2, or from your own investigations) for topics you might be interested in. These may be specific research topics from the paper titles, high-level fields from the flowchart, or general phrases from your own consideration – for example, maybe you enjoy concepts around cybersecurity, and aren't familiar with "anomaly detection" yet, but are curious enough to want to learn more. You may have a short list of a few phrases; this is an excellent starting point – the goal is not to have circled the entire chart or grid.

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1.3.4 Step 4: Leverage AI

Now that you've identified key words and topics of interest, take time to discuss them with peers, experts, and friends before diving into formal research. Casual conversations can help you articulate why these topics excite you and expose you to different perspectives you hadn't considered. Additionally, leverage AI tools as conversation partners to explore your ideas – ask them to explain current research trends, identify key challenges in your area of interest, or suggest how different topics might intersect. These tools can quickly provide overviews, clarify terminology, and help you understand the research landscape without the commitment of reading dozens of papers upfront. Think of this phase as "research small talk" that builds your intuition and vocabulary before you invest significant time in systematic literature review. Just remember that AI tools should complement, not replace, engagement with actual research papers and your research community – *more on this in Chapter 9*.

In your discussions with AI, ask for links to recent articles on a topic, and discuss why you are/are not interested in that particular article. Pro tip: Starting with magazine articles is an easier lift than full research papers – look to *Communications of the ACM*, *IEEE Explore*, and similar publications which translate recent technical research publications for a broader audience.

Upon completion of this activity, you should have clarified your own interests within computing, and are ready to explore one or more fields more deeply.

1.3.5 Tips for Success

Figures 1.1 and 1.2 are a good *starting point*, but there is no comprehensive resource outlining all research across every field of computing, in a way that is easily understood within a single activity. Take your time with them, but be flexible – if another topic comes to mind, run with that as well!

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