PART I. A SHORT SPEECH BY SARGENT

Introduction to Research and Developing New Ideas

There's a set of questions frequently raised: How do researchers, say in economics, political science, chemistry, or geology, come up with new projects? How do they develop an idea for a paper? How do you come up with something new? This question is central to many researchers.

To answer, consider past scientific discoveries, not merely in terms of outcomes but also in understanding the methods behind them. For instance, think back to how Nicholas Copernicus, Tycho Brahe, and others tackled problems with fresh perspectives. Their methodologies, rooted in curiosity and innovation, offer valuable lessons.

Historical Models and Their Development

- **Ptolemy's Model:** Ptolemy, around 2,000 years ago, wanted a model for planetary movements that predicted positions accurately. He leveraged extensive observational data, placing Earth at the center of the universe and building a complex model using circles and epicycles to represent planetary motion. This model, supported by the Church, was accepted as truth for nearly 1,800 years.
- **Copernicus's Challenge:** About 1,500 years later, Copernicus introduced a heliocentric model, simplifying Ptolemy's elaborate setup to just a few parameters. Though initially met with resistance from the Church, Copernicus's model eventually paved the way for more streamlined theories.
- **Tycho Brahe and Data Refinement:** In the late 16th century, Tycho Brahe recognized inaccuracies in existing data and focused on refining astronomical measurements. His meticulous work highlighted the importance of accurate data as a foundation for developing scientific models.
- **Kepler's Refinement to Ellipses:** Using Tycho's data, Kepler transitioned from circular to elliptical orbits, drastically improving model accuracy. His three laws of planetary motion, though empirically derived, fit the data far better, establishing a simpler, more precise model.

Research Process and Philosophies of Discovery

The methods of Copernicus and Kepler illustrate that scientific progress often stems from a blend of intuition, aesthetics, and mathematical knowledge. As research evolved, Newton introduced calculus to explain Kepler's findings, developing a more profound understanding of underlying forces. The pursuit of simplicity, often linked with beauty in science, drove these innovators to find models that not only fit data but also offered structural clarity.

Applications in Economics and Structural Modeling

In economics, the principles of structural models mirror those in Newtonian mechanics. These models aim to understand the mechanics of economic systems by analyzing forces like supply and demand in a structured manner. The complexity of economic systems, composed of individuals making interdependent choices, creates a need for models that can describe cause and effect—essentially guiding policies based on economic dynamics.

A recent model on data privacy illustrates this application in policy analysis, integrating elements like data buyers, sellers, firms, privacy concerns, and government policies. This structural model exemplifies how economic theories adapt to current issues, linking academic research with practical applications.

PART II Q&A

Q: Is it possible to anticipate the significance of research before it is completed?

A: I don’t think so, and I wouldn’t worry about it. This question is challenging due to the nature of discovery. Many people believe their work is groundbreaking, yet it may not be; meanwhile, others have made crucial contributions without realizing it—sometimes it takes a decade for their work to spark a revolution in the field.

Consider a historical example: in 1900, Henri Poincaré, one of the greatest scientists of his time, was asked what he thought the biggest scientific discoveries of the 20th century would be. He answered, “I have no idea,” because every true innovation is a surprise. By definition, you can’t predict surprises, and that’s what innovations are—they’re statistically unexpected, with an expected value of zero. Poincaré was just five years away from the discovery of relativity, yet he had no idea it was coming. Not even the person who would discover it could have predicted it.

Q: What should we learn from economic history? What are the most crucial takeaways for today’s world?

A: That's a great question, though a tricky one. Do I recommend studying history at length? Not necessarily. Especially if you’re from China—a country with such a long and meaningful history—it can be overwhelming to try to draw specific lessons. But here’s why history is interesting in this context: economics, especially macroeconomics, is largely non-experimental. While there’s a field of experimental economics, the large-scale experiments that macroeconomists would ideally conduct are neither feasible nor desirable. So, we rely on historical insights instead, though interpreting history has its own risks and requires a careful approach.

Take Deng Xiaoping as an example. He was a student of history with a theory rooted in historical understanding, which went something like this: for 30 years, China structured its economy based on principles largely borrowed from the Soviet Union rather than directly from Marx—whose writings didn’t offer practical guidance on economic structure. The goal was a planned economy that eliminated private property. Deng Xiaoping, initially a strong believer in this approach, eventually re-evaluated it. Deng examined cross-country evidence and noticed that places like Singapore, where Chinese immigrants from Guangdong (often from modest backgrounds) had organized their economy differently, were far more prosperous than China. This realization led him to reconsider his lifelong beliefs. He concluded that the data didn’t support the planned approach, so he turned to historical observations, effectively using history to shape policy. For Deng, history was more than a passive lesson; it was an active tool for economic transformation.

This approach is not unique to Deng—other great leaders have recognized that institutions evolve over time and have allowed new evidence to reshape their thinking. But for Deng, having lived through two economic revolutions—the one in 1949 and the reforms that followed—history was a guiding force for change. In this way, history, when used carefully, can provide valuable lessons for policymakers.

Q: Will artificial intelligence change some micro foundation of macro research?

A: There are a lot of questions about artificial intelligence circulating on WeChat and the Internet. Many of these questions are broad, partly because AI itself is often misunderstood. What does AI really mean? At its core, it's just a function—a mapping of X into Y—essentially fitting a function. X can be high-dimensional, and Y can be high-dimensional too, but fundamentally, it's about function fitting.

When it comes to the micro foundations of macro research, AI doesn't fundamentally change them. I could show you cutting-edge machine learning work, but it's still about doing tasks we've always wanted to do, just with larger computers and more data. The algorithms themselves aren't entirely new.

For the true foundations, consider the essential structure defined in foundational economic theory, such as in a paper I saw this morning. It outlines the basics: a collection of individuals with specific goals, constraints, resources, production functions, technologies, and markets. Some markets might be missing, but overall, there's a coherent system of optimization problems that fit together, with a government that can influence the environment. Those are the true foundations. AI, including tools like ChatGPT, isn't going to propose a new foundation that excludes people or governments—and if it does, it's simply hallucinating.

Q: What impact will large language models have on shaping the values of future global politics, economy and culture?

A: It's about defining the direction our country is taking. In response to your question, I honestly don't know. You've raised a legitimate concern, and it's one we're taking seriously. Personally, I don't fully trust large language models. When I first experimented with them, they often provided misleading or outright false information. So, how do I use them now? They're pretty good with tasks like Python or Java, but even then, you need to be an experienced programmer to catch the subtle mistakes they might make. In my experience, they can accelerate a good programmer's work but can't replace them.

As for the broader AI landscape, I wouldn't count on one country staying ahead indefinitely. Large language models, while expensive to build, are scientifically straightforward. At their core, they're about fitting functions to data—a fundamental concept you'd learn in statistics. Think about it: with 50 billion parameters, you need a substantial dataset to estimate each one accurately, typically around 30 data points per parameter. For models with 40, 50, or even 100 billion parameters, that's an enormous data requirement.

I know Chinese companies are also making strides in this field, and access to high-quality chips like Nvidia's could be influential. But with so many companies pouring billions into this race, it raises some big questions. You asked a great question, and it certainly gets one thinking.

Q: Have you ever held a theory you once believed to be true but later realized—perhaps much later—that it was flawed or needed modification? Could you share an example? Specifically, I'd like to know what signals or insights led you to recognize that the theory needed adjustment and how you realized it was wrong.

A: It's pretty straightforward. When you have a quantitative theory, especially in macroeconomics, it's built to explain specific datasets or time series. After working hard to fit the parameters, you move to validation. Often, during this process, the data reveal that the model doesn't work well in certain dimensions. By drilling down, you can identify precisely where it falls short and adjust accordingly. Classic examples include longstanding puzzles like the equity premium puzzle or the volatility puzzle.

Take Robert Shiller’s famous volatility puzzle. It’s been the starting point for decades of research, but recently, someone re-evaluated it, questioning whether the data was constructed in line with the theory’s intended definitions for dividends and returns. Using more refined definitions, they found that a big part of the puzzle disappears. This is a reminder of the risks of accepting data as-is, without re-examining it in light of theory.

Another approach is more intuitive—what Bob Lucas did, for example. He sometimes dismissed elements he found aesthetically unconvincing, saying, "I don’t believe it; it just doesn’t ring true to me." He trusted his instincts, which not everyone gets to do. But that’s what makes economics both challenging and exciting; it combines rigorous data analysis with intuition.

Q: Do you think it’s unwise to rely heavily on intuition, especially when the math alone doesn’t feel fully convincing? Sometimes, using basic economic principles to interpret complex aspects—or depending on a personal, non-mathematical understanding—seems helpful. What are your thoughts on this approach?

A: I often find the term "intuition" vague, though it's commonly used to describe people’s judgment. For example, my colleague Mark Gertler, a respected macroeconomist, often listens to seminar presentations on complex macro models and then asks, "Is this really what’s happening?" His ability to intuitively understand and question models comes from his deep grasp of their structure and math, which ultimately reflect real decisions made by people.

This kind of insight reminds me of historical figures like Marx, who didn’t just focus on isolated choices but tried to capture entire economic systems. Writing during the Industrial Revolution, Marx observed rapid social change and new economic classes emerging, which led him to ask, "How did we get here, and what’s next?" Without formal mathematical tools, he intuitively saw patterns—like the falling rate of profit—that he believed underpinned the business cycle and could lead to crises due to structural problems like overproduction.

Then came Keynes, who took a different approach. Unlike Marx, Keynes didn’t think capitalism had to collapse from business cycles; he believed minimal government intervention could stabilize the economy. He developed a framework of policies that could manage these cycles, a foundation others built upon post-WWII. The evolution from Marx to Keynes illustrates the ongoing effort in economics to align theory with data.

This process isn’t glamorous. It’s slow, deliberate work, like Marx spending days in the British Library reading endlessly, or economists today scrutinizing data for patterns. It’s about wrestling with big ideas step by step—essential, if not always exciting.

Q: Before you became the renowned Sargent, did your ideas come naturally, or did they evolve over time through hard work? Were they the result of intuition and creativity, or did they stem from persistent effort? And how did you know which direction to pursue?

A: It’s simpler than it seems. Don’t get caught up in whether ideas are “great” or “important.” For example, I once got interested in a technical problem through a paper by Milton Friedman. James Tobin and Robert Solow, two economists I admired, wrote mathematical versions of Friedman’s ideas, only to end up disproving them. Their work fascinated me, even though I was just an average student at the time.

While in the Army, I studied time series analysis to understand Solow's paper. After a year, I finally felt ready to apply what I'd learned, and I spotted a flaw in Solow and Tobin's interpretation. I wrote a four-page paper on this, but my first submission was harshly rejected. I shelved it until an older friend shared it with Carl Bruner, who published it in a new journal. Over time, even people like Tobin and Lucas read it. This wasn't a breakthrough; it was just a long journey to understand others' work better. Most discoveries aren't lightning bolts of genius—they're slow, deliberate efforts. If great minds can work this way, so can the rest of us.

Q: Previously, I was used to studying and occasionally conducting research, often with a professor assigning topics or ideas and guiding me. Now, as a Ph.D. student and junior researcher, I need to start conducting research independently. Could you offer some guidance?

A: What does it mean to have an idea? In my case, it wasn't exactly an original idea—I just felt I didn't fully understand something, so my role was to dig deeper into a problem that was already posed.

Another part of this story is the time in between. I wrote that paper in 1971 but started reading Solow's work in 1968. In those years, I was playing football with friends but also learning. In 1967, someone recommended I read John Muth's early papers on rational expectations. They were groundbreaking, but I needed more math and statistics to understand them, so I spent time building those skills.

The line between studying and doing research is thin; what starts as an exercise can become a research project. Trying to "come up with an idea" can feel torturous, like forcing a great play in sports. The goal is to keep learning and let ideas emerge naturally.

Q: I imagine you receive a lot of inquiries from potential researchers, but your time is limited, so you have to choose carefully. Do you have any specific criteria for deciding which projects or people to invest your time in?

A: I like to have fun every day. That's the first criteria. Everyone has their own approach to life, and mine is flexible. I make things up as I go, enjoy the work, and adapt. If I don't get tenure somewhere, I'll try elsewhere, or find a new path. I don't worry much.

Coming from a family where few pursued education or prestigious careers, I never felt pressured to reach a certain status. My parents didn't expect much from me, so by their standards, anything I've done is already a success. That background is liberating—if things hadn't worked out in academia, I would've just moved on to something else.

Q: I often notice that many researchers use advanced statistical methods or machine learning to discover patterns, especially in finance and economics. Is there a preferred sequence between building economic models and applying these advanced techniques? For example, should we be satisfied with just prediction, or should we seek more?

A: Different smart people would answer that question in different ways, which is why I mentioned Kepler and others. Pattern recognition is essential—you can't develop a theory without first identifying a pattern. That's one of the great things about economics. You do have to choose and specialize, but that choice allows for useful work along many different paths.

第一部分：简短演讲

引言：关于研究和新想法的产生

常见的一个问题是：研究人员（无论是在经济学、政治学、化学或地质学领域）是如何提出新的研究项目的？他们是如何构思出一篇论文的？又是如何产生新想法的？这个问题对于很多研究人员来说都是核心所在。

要回答这个问题，我们可以借鉴过去的科学发现，不仅关注结果，还要理解这些发现背后的方法。例如，回顾尼古拉·哥白尼、第谷·布拉赫等人如何用全新的视角来解决问题。他们的方法植根于好奇心和创新，这些都为我们提供了宝贵的启示。

历史模型及其发展

- **托勒密的模型：**大约两千年前，托勒密希望建立一个能准确预测行星位置的模型。他利用了大量的观测数据，将地球置于宇宙的中心，并构建了一个复杂的模型，利用圆形和本轮来表示行星的运动。这个模型在教会的支持下被当作“真理”接受了，持续了近 1800 年。
- **哥白尼的挑战：**大约 1500 年后，哥白尼提出了日心模型，将托勒密复杂的结构简化为更少的参数。尽管教会最初对他的模型持有抵触情绪，但哥白尼的模型最终为更加精简的理论奠定了基础。
- **第谷·布拉赫与数据精化：**在 16 世纪末，第谷·布拉赫意识到现有数据存在不准确之处，便专注于改进天文学的测量工作。他的细致工作突显了准确数据的重要性，这是构建科学模型的基础。
- **开普勒的椭圆轨道改进：**利用第谷的数据，开普勒将圆形轨道改为椭圆，大大提高了模型的准确性。他提出的行星运动三定律，尽管是经验总结，但能更好地符合数据，建立了一个更简单且精确的模型。

研究过程与发现的哲学

哥白尼和开普勒的方法表明，科学进步常常源于直觉、美学和数学知识的结合。随着研究的演进，牛顿引入了微积分来解释开普勒的发现，从而对背后的基本力量有了更深的理解。对简单之美的追求推动了这些创新者找到不仅符合数据且结构清晰的模型。

在经济学中的应用与结构模型

在经济学中，结构模型的原理与牛顿力学相似。通过分析供需等力量，这些模型旨在理解经济系统的“力学”。由个人做出相互关联的决策所组成的经济系统的复杂性，使得模型需要能够描述因果关系，从而根据经济动态指导政策。

一个关于数据隐私的最新模型展示了结构模型在政策分析中的应用，整合了数据买卖方、公司隐私关注及政府政策等元素。该结构模型说明了经济理论如何适应当前问题，将学术研究与实际应用相结合。

第二部分：问答环节

问：在研究完成之前，是否有可能预见其重要性？

答：我认为不可能，而且我也不会为此担心。这个问题的难度在于发现的本质。许多人认为他们的工作是开创性的，但可能并非如此；同时，也有一些人做出了至关重要的贡献却未曾察觉——有时需要十年时间，他们的工作才能在该领域引发变革。

想想历史上的一个例子：1900 年，亨利·庞加莱——当时最伟大的科学家之一，被问及他认为 20 世纪最重大的科学发现会是什么。他回答道：“我不知道。”因为每一个真正的

创新都是一个惊喜。按照定义，你无法预测惊喜，而创新正是如此——它们在统计上是意料之外的，预期值为零。庞加莱距离相对论的发现仅有五年之遥，但他完全不知道它的到来。甚至连即将发现它的人也无法预见它。

问：我们应该从经济史中学到什么？对当今世界最重要的启示是什么？

答：这是个好问题，但也很棘手。我是否建议深入研究历史？不一定。尤其是如果你来自中国——一个拥有如此悠久和深刻历史的国家——试图从中提取具体的教训可能会让人感到不知所措。但历史在这个背景下之所以有趣，是因为经济学，尤其是宏观经济学，很大程度上是非实验性的。虽然有实验经济学这一领域，但宏观经济学家理想中进行的大规模实验既不可行也不合适。因此，我们依赖于历史洞察，尽管解释历史本身也有风险，需要谨慎对待。

以邓小平为例。他是一个历史的研究者，他的理论深深植根于历史的理解，基本上是这样的：三十年来，中国的经济结构主要借鉴了苏联的原则，而不是直接借鉴马克思——后者的著作中并未提供关于经济结构的实际指导。其目标是建立一个消除私有财产的计划经济。邓小平最初是这一方法的坚定信仰者，后来他重新评估了这一思路。他考察了跨国的经验，并注意到像新加坡这样的地方——那里居住着许多来自广东的华人移民（通常出身平凡），他们采用了不同的经济模式，比中国繁荣得多。这一发现促使他重新审视自己的信念。他得出结论，数据并不支持计划经济，因此他转向历史观察，有效地利用历史来塑造政策。对邓而言，历史不仅是被动的教训，而是经济转型的积极工具。

这种方法并非邓独有——其他伟大的领导人也认识到制度会随着时间演变，并允许新的证据重新塑造他们的思维。但对于邓小平而言，经历了两次经济革命——1949 年的革命以及随后的改革——历史是变革的引导力量。这样看来，历史在被谨慎使用时，可以为政策制定者提供宝贵的启示。

问：人工智能会改变宏观研究的某些微观基础吗？

答：关于人工智能的讨论在微信和互联网上非常多。许多这些问题都相对宽泛，部分原因是人们对人工智能的理解常常存在误解。那么，人工智能究竟意味着什么？从本质上讲，它只是一种函数——一个将 X 映射到 Y 的过程——基本上就是拟合一个函数。X 可以是高维的，Y 也可以是高维的，但归根结底，它就是一个函数拟合问题。

当谈到宏观研究的微观基础时，人工智能并没有从根本上改变它们。我可以给你展示最前沿的机器学习工作，但它依然是在做我们一直想要完成的任务，只不过借助更强大的计算机和更多的数据。这些算法本身并不完全是新的。

关于真正的基础，可以参考经济理论中定义的基本结构，比如我今天早上看到的一篇文章，它概述了基本框架：由一群具有特定目标、约束、资源、生产函数、技术和市场的个体组成。有些市场可能是缺失的，但总体而言，这是一套一致的优化问题体系，有一个能够影响环境的政府。这些才是真正的基础。人工智能，包括像 ChatGPT 这样的工具，不会提出一种排除人类或政府的新基础——即使提出了，那也只是“幻觉”而已。

问：大语言模型相对单一的价值观对未来全球政治经济文化的价值观塑造会有哪些影响?

答：这关系到我们国家所走的方向。针对你的问题，老实说，我不知道答案。你提出了一个合理的担忧，我们确实在认真对待。就我个人而言，我对大型语言模型并不完全信任。当我最初尝试使用它们时，它们经常提供误导性甚至错误的信息。那么，我现在是如何使用它们的呢？它们在处理 Python 或 Java 任务上还不错，但即便如此，你也需要是经验丰富的程序员才能发现它们可能犯的细微错误。根据我的经验，它们可以加速优秀程序员的工作，但无法取代他们。

至于更广泛的人工智能领域，我不会指望一个国家能够无限期地保持领先。大型语言模型虽然构建成本高昂，但从科学角度来看并不复杂。它们的核心在于将函数拟合到数据上——这是统计学中会学到的基本概念。想想看：对于 500 亿个参数，你需要一个足够大的数据集来准确估计每一个参数，通常每个参数大约需要 30 个数据点。对于拥有 400 亿、500 亿甚至 1000 亿参数的模型，这需要一个庞大的数据量。

我知道中国公司在这个领域也在取得进展，而像英伟达这样的高质量芯片的可获得性可能会带来影响。但随着那么多公司在这个领域投入数十亿美元，也引发了一些重大问题。你的问题很棒，确实让人深思。

问：你是否曾经坚信某个理论是真实的，但后来——可能是很久以后——发现它存在缺陷或需要修改？你能分享一个例子吗？具体来说，我想知道是什么信号或洞见让你意识到该理论需要调整，以及你是如何意识到它是错误的。

答：这个过程其实很直接。当你有一个定量理论时，尤其是在宏观经济学中，它是为了解释特定的数据集或时间序列而构建的。经过大量工作来拟合参数后，接下来就是验证的步骤。通常在这个过程中，数据会显示模型在某些方面表现不佳。通过深入研究，你可以准确地找出它的不足之处并进行相应调整。经典的例子包括长期存在的难题，比如股权溢价之谜或波动性之谜。

以罗伯特·席勒著名的波动性之谜为例。几十年来，它一直是研究的起点，但最近有人重新评估了这一问题，质疑数据是否按照理论预期的股息和回报定义来构建。使用更精确的定义后，他们发现问题的大部分消失了。这提醒我们，不要仅仅接受现有的数据，而是要在理论的视角下重新审视它。

另一种方法则更加直观——比如鲍勃·卢卡斯的做法。他有时会直接忽视那些他觉得美学上不令人信服的要素，说道："我不相信它；它听起来就不对劲。"他信任自己的直觉，这并非人人都能做到。但正是这种结合了严谨数据分析与直觉的特点，使得经济学既富有挑战性又充满乐趣。

问：你认为过于依赖直觉是否明智，尤其是在数学本身不足以让人完全信服的情况下？有时，用基本的经济学原理来解读复杂问题，或者依赖个人的非数学理解，似乎更有帮助。你怎么看待这种方法？

答：我常觉得"直觉"这个词有些模糊，尽管它经常被用来描述人们的判断。比如，我的同事马克·格特勒是一位受人尊敬的宏观经济学家，他经常在研讨会上听取关于复杂宏观模型的演讲，然后问道："这真的是发生的事情吗？"他能够直观地理解并质疑模型的能力，源于他对模型结构和数学的深刻理解，而这些最终反映了人们所做出的真实决策。

这种洞见让我想起了历史人物，比如马克思。他不仅关注孤立的选择，还试图捕捉整个经济系统的运作。在工业革命期间，马克思观察到社会的快速变化和新经济阶层的出现，这促使他提出了"我们是如何走到这一步的，接下来会怎样？"虽然没有正式的数学工具，但他凭借直觉看到了诸如利润率下降这样的模式，他认为这揭示了商业周期的本质，并可能因生产过剩等结构性问题导致危机。

接着是凯恩斯，他采取了不同的方法。与马克思不同，凯恩斯并不认为资本主义一定会因商业周期而崩溃；他相信适度的政府干预可以稳定经济。他开发了一套可以管理这些周期的政策框架，二战后其他人基于此进一步拓展。经济学从马克思到凯恩斯的发展，展示了不断将理论与数据对齐的努力。

这一过程并不光鲜。它是缓慢而细致的工作，就像马克思在大英图书馆中度过数日不休的阅读，或是今天的经济学家在数据中寻找模式。这是在逐步深入研究大问题——至关重要，

尽管并不总是激动人心。

问：在你成名之前，你的想法是自然而然产生的，还是通过努力逐渐发展出来的？它们是直觉和创造力的结果，还是源于持之以恒的努力？你是如何知道要追求哪个方向的？
答：这比想象中要简单。不要过于纠结于想法是否“伟大”或“重要”。举个例子，有一次我通过米尔顿·弗里德曼的一篇论文对一个技术问题产生了兴趣。我钦佩的两位经济学家詹姆斯·托宾和罗伯特·索洛写了弗里德曼思想的数学版本，结果却证伪了它们。尽管当时我只是个普通学生，但他们的工作深深吸引了我。

在军队服役期间，我学习时间序列分析，以便理解索洛的论文。一年后，我终于觉得自己准备好了可以应用所学内容，并发现了索洛和托宾解释中的一个漏洞。我写了一篇四页的论文，但第一次投稿时被严厉拒绝。我把它搁置了，直到一位年长的朋友把它分享给了卡尔·布鲁纳，他在一个新期刊上发表了它。渐渐地，像托宾和卢卡斯这样的人也读到了这篇文章。这并不是一个突破；而是一个缓慢而漫长的过程，用来更好地理解他人的工作。大多数发现并非灵光乍现的天才火花——它们是缓慢而细致的努力。如果伟大的头脑可以这样工作，那么我们其他人也可以。

问：之前我习惯于学习以及偶尔进行研究，通常是由教授指定主题或想法并给予指导。现在，作为一名博士生和初级研究员，我需要开始独立开展研究。您能提供一些指导吗？
答：“拥有一个想法”意味着什么？对我来说，它并不完全是一个原创的想法——只是我感觉自己对某些问题还没有完全理解，因此我的角色是深入挖掘一个已被提出的问题。

这个故事的另一部分是期间的时间积累。我在 1971 年写了那篇论文，但早在 1968 年就开始阅读索洛的作品。那些年里，我一边和朋友踢足球，一边学习。1967 年，有人建议我阅读约翰·穆斯关于理性预期的早期论文。这些论文非常具有突破性，但我需要更多的数学和统计知识来理解它们，于是花时间去提升这些技能。

学习与研究之间的界限很模糊；从一项练习开始的内容可能最终变成一个研究项目。强迫自己“想出一个想法”可能会让人感觉痛苦，就像在运动中勉强做出精彩表现一样。目标是持续学习，让想法自然而然地浮现。

问：我想您会收到很多来自潜在研究人员的询问，但由于时间有限，您必须仔细选择。您在决定投入时间的项目或人选时有具体的标准吗？
答：我喜欢每天都过得愉快，这是第一标准。每个人都有自己的生活方式，而我的方式比较灵活。我即兴发挥，享受工作，随时适应变化。如果在某个地方得不到终身教职，我会去别的地方，或找到新的道路。我不怎么担心这些。

我的家人中很少有人追求教育或名望事业，所以我从未感到需要达到某种地位的压力。我的父母对我没有太多期望，所以按他们的标准来看，我做的任何事情都已经算是成功了。这样的背景让我感觉自由——如果学术界的路行不通，我也会去做其他的事情。

问：我经常注意到，许多研究人员使用高级统计方法或机器学习来发现模式，尤其是在金融和经济领域。在构建经济模型和应用这些高级技术之间是否有一个优先顺序？例如，我们是否应该满足于仅仅做出预测，还是应该追求更多？
答：不同的聪明人会以不同方式回答这个问题，这也是我提到开普勒等人的原因。模式识别是至关重要的——在没有首先识别出模式的情况下，你无法发展出理论。这是经济学的一个伟大之处。你确实需要选择并专注于某个方向，但这个选择允许在许多不同的路径上进行有价值的工作。