

Becoming a scientist: A journey toward lasting value

Jun Zhong^{1,*}

¹Changping Laboratory, Beijing 102206, China

*Correspondence: junzhong@cpl.ac.cn

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What does it really mean to become a scientist? I used to think it was simple: do research and make new discoveries. But over the years, I have come to realize that making a discovery is only part of the journey. The harder question is deciding what is worth discovering, how deeply to pursue it, and whether that knowledge will remain valuable beyond the moment of discovery.

I am Jun Zhong (<https://junzhong.cc/>), with over 17 years of experience in bioinformatics and genomics research. As the lead bioinformatician for PanScan IV—the largest international pancreatic cancer genetics consortium, encompassing approximately 2 million individuals from more than 20 countries. I have identified novel pancreatic cancer risk genes and non-coding driver mutations, as well as elucidated their underlying mechanisms. These discoveries provide a foundation for identifying individuals at high risk of pancreatic cancer and potential new therapeutic targets.

I received my Ph.D. in bioinformatics from the Beijing Institute of Genomics (China National Center for Bioinformation), University of Chinese Academy of Sciences (UCAS), in 2014. At that time, like many young researchers, I was proud of completing my degree, but I had not yet realized that the far greater challenge was deciding which questions were truly worth a lifetime of research.

A LESSON I DID NOT UNDERSTAND AT FIRST

One of the most important influences on my scientific development was my doctoral advisor, Professor Jun Yu (Figure 1). Professor Yu is a pioneer of genomics in China. He has been dedicated to genomics and bioinformatics research for over three decades, promoting the development and application of sequencing technology. As one of the founders of Beijing Genomics Institute (BGI) and Beijing Institute of Genomics, Chinese Academy of Sciences, Professor Yu served in leadership positions in major international and national genome research projects in China, including the Human Genome Project and Superhybrid Rice Genome Project. His mentorship went beyond experimental design or methodology. He repeatedly pushed us to think about what would remain after a project was finished.

I still remember a routine laboratory meeting during my Ph.D. training. We were discussing preliminary analyses, technical details, and what we might do next. After listening for a while, Professor Yu said, “**We should always strive to transform our findings into knowledge with lasting value.**” At the time, I understood this as a general academic ideal. Only years later did I grasp its practical meaning: a scientist has to think not only about whether a result is innovative and correct, but also about whether the question is important enough to earn a significant place in the long-term life science ecosystem.

FROM CLUES TO DEEPER MECHANISTIC INSIGHTS

In 2017, I went to the United States to join the National Cancer Institute (NCI) at the National Institutes of Health as a Postdoctoral Fellow. Senior Investigator Laufey T. Amundadottir, who co-leads the Pancreatic Cancer Cohort Consortium (PanScan), was my mentor. My early work in her laboratory focused on finding genetic signals associated with pancreatic cancer. In a 2020 study, I applied transcriptome-wide association analysis to identify cancer candidate susceptibility genes. The study was successful in one important sense: it showed how large genetic datasets could reveal signals that were difficult to see from individual studies alone. But it also left me with a question I could not easily ignore. If a statistical association points to a gene, does that tell us what is actually happening in a cell? The answer is often no. That limitation changed the way I approached subsequent projects.

The next step was therefore to move from clues (association) toward mechanism. In a subsequent study, we investigated a pancreatic cancer risk

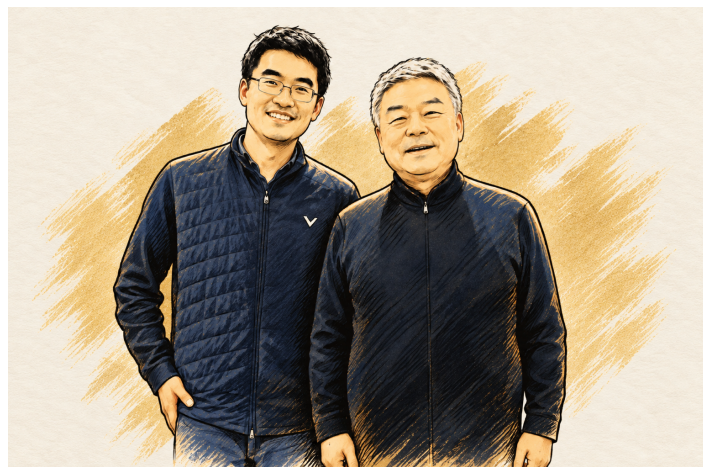


Figure 1. Jun Zhong (left) with Professor Jun Yu (right), March 8, 2026

signal at 16q23.1 and identified a 584-bp deletion affecting CTRB2 protein. The work showed how a genetic variant could alter splicing and protein function, providing a concrete biological explanation for a statistical risk signal. For me, the important takeaway was not simply the specific variant. It was a fundamental shift in the question: instead of stopping at “where is the risk signal?”, I began asking “how does the signal actually work?”

FROM ONE LAYER TO A BIGGER PICTURE

I was promoted to the position of Research Fellow at NCI in 2022. Over time, I came to realize that important biological questions rarely exist within a single layer of information. My later work therefore evolved toward integrating different layers of biological evidence. In a large-scale multi-omic pancreatic cancer study, we combined genomic, transcriptomic, epigenomic, and chromatin-interaction information to investigate non-coding somatic driver mutations. The goal was not simply to add more data types. The goal was to use complementary evidence to answer a question that one data layer alone could not resolve: which non-coding mutations are biologically meaningful, and how they affect target genes to promote cancer.

This experience also taught me a practical limitation of multi-omics research. More layers do not automatically produce better science. Each additional dataset may introduce new technical noise and conflicting signals. The challenge is therefore not data accumulation but evidence integration. The strongest conclusions emerge when different kinds of evidence converge on the same biological explanation.

FROM WHAT IS NEW TO WHAT TRULY MATTERS

These experiences changed how I think about choosing a research direction. Early in my career, I often viewed the choice as a competition between established fields and new fields: established areas offered clearer questions and stronger foundations, while emerging areas offered more room for discovery. I now think that framing is too simple. My own career has been an example. Pancreatic cancer genetics was not an empty field when I entered it. Yet within an established area, there were still unanswered questions: which genes actually drive cancer susceptibility, which variants are functional, why do some signals remain unexplained, and how can we move from statistical evidence to biological mechanism? The real opportunity was not simply to chase a trendy topic, but to find a meaningful gap within a field.

That distinction has become increasingly important to me. I prefer not to select a direction merely because it is crowded or uncrowded, nor because a

technology is new. Instead, I want to work where an important biological question meets a tractable opportunity for discovery. Sometimes that means pushing deeper into an established question, while other times it means exploring a new area. The common requirement is that the question should matter after the current technology cycle has passed.

WHAT I NOW UNDERSTAND ABOUT LASTING VALUE?

Looking back, I think I am beginning to understand what my Ph.D. advisor meant by “lasting value.” For me, it comes down to three things: **first, asking questions that remain meaningful** as the field moves forward, rather than simply making a new discovery, finding a more interesting variant, or developing a more powerful technology; **second, building enough evidence to truly understand the answer**, as my own research journey has taken me from looking for genetic clues, to asking how those clues actually work, and eventually to integrating different layers of biological information to see a bigger picture; and **third, leaving knowledge that others can build on**. I have come to believe that valuable research is not necessarily the most novel at the moment it is published, but rather research that leaves behind a clearer understanding of biology—something that others can use, build upon, and carry forward long after the original study is finished.

WHERE SHOULD MY NEXT CHAPTER BEGIN?

After more than eight years in the United States, I have also had to think seriously about where I want the next stage of my scientific career to take

place. Remaining abroad offers an established research environment and access to strong international collaborations. For me, this is not about which environment is better, it is a question of where I can make the most meaningful contribution in the next stage of my career. Therefore, I have returned to China, my home country. I aim to bring together the strengths I have gained from large-scale international collaboration, computational genomics, and mechanistic cancer research, while building new national and international collaborations.

Recently, I decided to join Changping Laboratory in Beijing. My research in cancer genetics and multi-omics closely aligns with Changping Laboratory's major research focus on cancer and aging-related diseases. I am particularly attracted by its strong interdisciplinary environment and translational focus, which provide an excellent platform to turn genomic discoveries into clinically meaningful applications. As I design new cancer projects at Changping Laboratory, I often think back to Professor Yu's words during that lab meeting over a decade ago: “We should always strive to transform our findings into knowledge with lasting value.” These words now have a different meaning for me. I am not only thinking about what we can discover, but also which questions are worth pursuing over the long term, and what knowledge we can leave behind for others to build on.

DECLARATION OF INTERESTS

The author declares no competing interests.