SNU GRAND QUEST

A GUIDE FOR GRAND QUEST RESEARCHERS

A Space for the Questions in Your Drawer

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Launching the SNU Grand Quest

Seoul National University embarks on a new challenge.

Over the past half century, Korea has traveled a remarkable journey.
Rising from the ashes of war to become a major pillar of the global economy, this achievement is nearly without parallel in modern history. Seoul National University has grown alongside the nation, at the very center of that journey. Through every phase of industrialization and democratization, the information revolution and globalization, our researchers have fulfilled the roles their times demanded.
That accumulated capacity has shaped the standing of Korean scholarship today.

Now we stand at a new threshold. In many domains, we have reached a point where there are no longer precedents to follow. When the footprints of those ahead are nowhere to be seen, what is needed is not faster pursuit but a new sense of direction.

We have excelled at answering the questions the world posed to us.
The time has come to pose questions of our own. The power to find answers and the power to frame questions are not in opposition. They require each other. Seoul National University aspires to be a university that commands both.

The SNU Grand Quest Program is an expression of this commitment.
Seoul National University is creating, on its own initiative, a space where questions no one has yet asked, challenges that defy conventional frameworks, can be taken seriously. Beyond excellence on well-trodden paths, this is a declaration that we will also support the courage to forge paths that do not yet exist.

We hope this challenge will mark the starting point of a new chapter in Korean scholarship.

We look forward to the participation of Seoul National University's research community.

Hong-rim Ryu
President, Seoul National University

A Guide for Grand Quest Researchers

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Prologue

The Questions in Your Drawer

Every researcher has questions tucked away in a drawer.

Some are old questions. Questions that took shape during doctoral studies but were never fully explored. A doubt discovered in the gaps of existing theory. An unexplained phenomenon encountered in the field. A moment of pause before a single sentence in a textbook.

Some are questions that emerged in the course of research. After years of immersion in a field, patterns begin to appear that mainstream methodologies cannot explain. Data that recur but do not fit existing theories. Limitations that colleagues acknowledge in private but never address in public. The deeper one's experience, the more such questions accumulate. There are also questions that arise from encounters with other disciplines. At conferences, in collaborative research, in conversations with colleagues from different fields, there are moments when the assumptions of one's own discipline suddenly seem unfamiliar. The moment when "the way things are" becomes "why are things this way."

The questions in the drawer do not form once early in a career and then remain frozen. As long as research continues, new questions emerge, existing questions deepen, and sometimes questions from entirely different directions are added. The questions in the drawer grow, accumulate, and evolve.

Yet these questions seldom make it out of the drawer. In the daily demands of running a lab, mentoring students, and securing funding, proven topics are the safer choice. They are set aside because they seem difficult to explain to colleagues, unlikely to pass a funding review. Questions postponed in this way are rarely taken out again. But they do not disappear, either.

At the 1900 International Congress of Mathematicians in Paris, David Hilbert presented not answers but 23 questions. By publicly posing questions that even he could not solve, he opened the direction mathematics would follow for more than a century. What Hilbert demonstrated was neither a specific solution nor a finished body of research. It was the power of questions to determine the orientation of an entire scholarly community.

Prologue

The Questions in Your Drawer

Throughout the history of scholarship, it has always been such questions that reshaped the landscape. Not questions that seek more refined answers within existing frameworks, but questions that compel us to redraw the frameworks themselves. Questions that seem reckless at first, with an overwhelming probability of failure. Yet it was precisely upon such questions that new fields of scholarship began. The "Grand Quest" this guide speaks of is exactly that kind of question. It will likely be somewhat rough at first, with only a faint sense of direction. But if the question unsettles prevailing assumptions and opens a new direction of inquiry, it already carries sufficient scholarly meaning.

The SNU Grand Quest Program (Grand Quest Program hereinafter) is an attempt to create a space where those questions can be taken out of the drawer. Within existing research support systems, daring questions have been at a disadvantage. Evaluation criteria that demand proven topics and predictable outcomes are rational in themselves, but they have not served research that seeks to explore uncharted directions. The Grand Quest Program is a playing field for such challenges.

This story is addressed to every researcher who carries questions in a drawer. Whether it is a question you have held since your doctoral years, one that emerged after decades of research, or one that arose from an encounter with another discipline, you need not keep it in the drawer any longer. A space to bring that question out is here. It does not matter if the question is still unpolished, or if there is no answer in sight. Someone will set out upon that question, and that departure may become the seed of a new field of scholarship.

Let us now begin the journey toward the Grand Quest.

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Chapter 1.

Chapter 1.
Where Outstanding Scholars Diverge

"The One with a Map
and the One with a Compass"

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Chapter 1.
Where Outstanding Scholars Diverge

"The One with a Map and the One with a Compass"

Competition on the Roadmap: How Excellence Is Built

Researchers face a set of choices. There are questions that the scholarly community has agreed upon as important. There are research programs pioneered by senior researchers and refined by peers. Proving one's capability on this path is the surest starting point. Mastering established methodologies, advancing existing theories with greater precision, accumulating measurable results. This is the path to becoming an excellent researcher.

This path is by no means easy. But what needs to be done is relatively clear. The miniaturization process in memory semiconductors exemplifies this kind of research. Moore's Law, articulated by Gordon Moore in 1965, went beyond a simple observation to serve as a roadmap for the entire semiconductor industry and academic community. The projection that transistor density would double at regular intervals made clear "what must be done at the next stage." Researchers competed intensely around the problems of process scaling, device architecture improvement, and materials optimization. Formidable challenges such as extreme ultraviolet (EUV) lithography, high-k dielectrics, and leakage current suppression were solved one by one along this roadmap.

This competition is arduous, but not disorienting. It is a race on a map with a clear route. It is precisely this capacity, accumulated by Seoul National University's researchers, that has built Korean scholarship to where it stands today, and it will remain a core asset going forward.

Yet among researchers walking this path, a similar experience is sometimes

shared. Research progresses well, publications accumulate steadily, and the lab runs on a stable footing. Still, at some point, something stirs in a corner of the mind. A question held when first entering the world of scholarship may still be sitting in the drawer, and new doubts that emerged over years of research may have piled up beside it. Even while knowing that one's current research is meaningful enough, the questions tucked away in the drawer keep circling in the mind.

The foundation built by research on the roadmap is legitimate and solid. Yet it is precisely because one stands on that foundation that certain things come into view. Taking a first step toward a direction that is not on the map. Redrawing the roadmap itself. A vague pull toward that work holds the researcher's attention.

The Path Opened by Questions in the Drawer

There are scholars who start in the same field yet, with time, end up standing in entirely different places. This difference cannot be explained by talent or effort alone. The decisive difference lies in what questions they chose to ask and pursue.

The research trajectory of Katalin Karikó, who received the 2023 Nobel Prize in Physiology or Medicine, illustrates this dramatically (see case study). For decades, Karikó held on to the question: 'Could mRNA, which the academic community had deemed unsuitable, be engineered and used

as an informational molecule?' Her funding applications were rejected, she was demoted from her faculty position, and colleagues left. Yet that question ultimately opened a new chapter in medicine: the mRNA vaccine.

The choice of question defines a researcher's career and scholarly position. A stonemason follows the sculptor's blueprint, but the sculptor does not follow someone else's blueprint when standing before a block of stone. The sculptor imagines, in a way that is uniquely one's own, the form hidden within the stone, and draws up a plan to bring it out.

What Reshapes the Landscape of Scholarship Is Not Speed but Direction

What happens when the questions in the drawer finally come out? Sometimes, nothing happens. Most daring questions fade away quietly. But occasionally, one researcher's question becomes the starting line for others.

The research of Professor Taeghwan Hyeon of Seoul National University illustrates this process. For a long time in materials science and chemistry, nanoparticles were understood as objects that formed more or less by chance under given conditions. The research roadmap was oriented toward analyzing the properties of particles that had already been made and improving their performance.

Professor Hyeon had a different question. 'What if nanoparticles could be designed intentionally, rather than left to chance?' The moment nanoparticles

were seen not as ‘objects to be discovered’ but as ‘objects to be designed,’ the direction of research changed entirely. The ‘Heat-Up Process’ developed from this question opened a way to produce uniform nanoparticles in large quantities without a separation step. It was not a faster separation technique but an approach that made separation itself unnecessary. This principle was subsequently extended to diverse domains including catalysis, energy, electronic materials, and biomedical applications, and nanoparticle synthesis established itself as a research field united by shared design principles.

What this case demonstrates is clear. A new question creates a new roadmap, and upon that roadmap, research begins to build new foundations. Research that opens new paths and research that builds foundations on those paths need each other. Without the former, the latter has no path to walk. Without the latter, the path opened by the former returns to overgrown fields. Now we seek to begin, alongside the work of building foundations on existing paths, the work of opening new paths altogether.

‘The one with a map and the one with a compass.’

A map organizes the known routes described by those who came before. A compass is what you need when searching for a path that does not yet exist. In scholarship, there is the work of following a defined route swiftly and accurately with a map in hand, and there is the work of setting out with a compass to find a path that does not yet exist. Both are necessary. But the

support required for these two kinds of work is different.

For researchers carrying a map, a well-functioning support system already exists. For researchers carrying a compass, a different kind of space is needed. This guide calls such fundamental questions, ones that seek to draw an entirely new roadmap, ‘Grand Quests.’ And the Grand Quest Program is a space where researchers who carry such questions can attempt to pursue them.

But there is a problem. The Grand Quest is far harder to find than one might expect. Why is that?

Closing Chapter 1

In scholarship, there is the work of carrying a map and the work of carrying a compass.
Both are necessary, and both are difficult.
It is simply that the space needed for researchers carrying a compass has not been sufficient.

[Case Study] Katalin Karikó: A Question the System Could Not Accommodate

In the 1980s, the prevailing roadmap for gene therapy centered on delivering DNA directly. mRNA was too unstable and triggered strong immune responses in the body. The academic community's judgment was rational: mRNA is useful as a research tool but unsuitable as a therapeutic agent.

Katalin Karikó held a different question. 'Could mRNA be used as medicine?' If DNA operates permanently, mRNA operates temporarily and then disappears. Could this very property become an advantage? But this question was not on the roadmap of the time. Karikó's grant applications were repeatedly rejected. In 1995, the University of Pennsylvania notified her that she would be demoted from the tenure track if she failed to secure funding. Karikó accepted the demotion and the pay cut, and stayed. She could not let go of the question.

This was not a problem unique to Karikó. Questions outside the roadmap were structurally difficult to support within the existing system. In 1997, a chance encounter with immunologist Drew Weissman became a turning point. The two discovered that modifying the nucleosides of mRNA could evade the immune response. They published their findings in 2005, but the reception was cold. In 2013, Karikó left the University of Pennsylvania and joined BioNTech in Germany. When the COVID-19 pandemic struck in 2020, BioNTech and Moderna developed the first large-scale commercially deployed mRNA vaccines within a single year, built on this technology. In 2023, Karikó and Weissman were awarded the Nobel Prize in Physiology or Medicine.

What made Karikó's question a reality was the accumulated foundation of immunology and molecular biology. The problem was not Karikó's ability. It was the fact that, for 30 years, there was nowhere for a question outside the roadmap to reside.

Chapter 2.

Chapter 2.
Why the Grand Quest Is Hard to See at First

"The Lens Called Success"

GRAND QUEST

Chapter 2.
Why the Grand Quest Is Hard to See at First

"The Lens Called Success"

The Invisible Wall Built by Success

The questions in the drawer, discussed in Chapter 1, did not end up there by deliberate decision. Most of them drifted in naturally. Not because the questions were wrong. Rather, because it was difficult to determine whether they were right or wrong in the first place. They did not fit well with existing research programs, or were hard to explain to colleagues, or seemed unlikely to survive a funding review. Bringing them up felt likely to invite the response: 'Why would you ask something like that?'

This is not a problem caused by a closed-minded scholarly community. It is the exact opposite. It is a problem that arises because the scholarly community functions successfully. Thomas Kuhn called this 'normal science.' Once a shared framework of theories, methodologies, and problem definitions is established, what Kuhn called a 'paradigm,' researchers conduct their work like solving puzzles within that framework. This framework determines what counts as an important question, what evidence is valid, and what methods are appropriate. And research within this framework is genuinely powerful. With textbook theories, standard methodologies, and proven research programs in place, research proceeds efficiently and results accumulate rapidly. One can start from the foundation laid by predecessors.

The problem lies precisely in this efficiency. Successful assumptions are no longer subject to doubt. They become starting points, background conditions, things as taken for granted as the air we breathe. The more firmly held the assumption, the later its basis is examined. Questions that do not align with

such assumptions become ‘strange questions.’ It becomes only natural for strange questions to end up in the drawer.

The history of the computing industry illustrates this. For a long time, the CPU stood at the center of general-purpose computation. The GPU was understood as an auxiliary device for graphics processing. This assumption was rational. CPU-centric architecture had driven the computing industry for decades and enabled countless innovations. Yet this assumption became a limitation in emerging domains such as artificial intelligence, which required massive parallel computation. When Jensen Huang of NVIDIA sought to redefine the GPU as a general-purpose computing platform, many were skeptical. Not because the CPU-centric paradigm was ‘wrong.’ It was so successful, so familiar, that other possibilities were simply hard to see.

This is why the Grand Quest is hard to see. It is a question pushed out of view by the success of the existing paradigm, by the lens that success has created.

The Familiar Becomes Transparent

It is difficult to explain water to a fish. For a fish born in water and living in water, water is not ‘an environment’ but ‘the world itself.’ Something similar happens in scholarship. Certain modes of explanation and problem-setting, through repeated success, harden into what feels like common sense or fact. Sentences that open with ‘It is well known that…’ in the introductions

of papers, passages in textbooks preceded by ‘generally…’ are exactly these. They are no longer asserted or defended. They simply operate as assumed premises. They become transparent and invisible.

In Kuhn’s terms, this is both the strength and the limitation of normal science. A paradigm provides researchers with a lens through which to see the world, but at the same time it leaves what cannot be seen through that lens unseen. Researchers solve the problems the paradigm illuminates, use the methods the paradigm endorses, and publish the results the paradigm deems valid. This process itself is productive. But once inside this process, questions that lie beyond the boundaries of the paradigm structurally disappear from view.

The questions in the drawer are usually connected to these transparent assumptions. They are questions that ask ‘Is that really so?’ about things accepted as ‘just the way it is.’ That is why they are hard to see. The very instrument of seeing filters them out.

In psychology, the experience of the familiar suddenly appearing unfamiliar is called “jamais vu.” If déjà vu is the phenomenon of the unfamiliar feeling familiar, jamais vu is its opposite. It is the moment when a road walked every day suddenly feels as if seen for the first time, or a word used daily suddenly looks strange.

This was precisely what Hannah Arendt experienced. In 1961, Arendt traveled to Jerusalem to cover the trial of Nazi war criminal Adolf Eichmann. She was about to face the man who had carried out the murder of millions.

Arendt expected to encounter a figure who was, in some form, demonic. A fanatic's gaze, a cruel temperament, anti-Semitic zealotry. But the Eichmann she faced in the courtroom was astonishingly ordinary. He felt no guilt, but not because he was evil. Simply because he had 'never thought about it.' Following orders from above, obeying the law, performing his role within the organization. For him, mass murder was administrative work.

At that moment, the familiar concept of 'evil' suddenly appeared unfamiliar to Arendt. We are accustomed to linking evil with monstrous will and malicious intent. But none of that was visible in Eichmann. What was visible instead was 'thoughtlessness.' 'Could evil arise not from monstrosity but from the uncritical obedience of ordinary people, from the absence of thinking?' This question developed into the concept of the 'banality of evil' and opened new fields of inquiry: the study of totalitarianism, bureaucratic ethics, and moral responsibility within organizations.

The assumption that 'evil is monstrous' had been transparent until Arendt encountered Eichmann. It was not an object of doubt but the very way of understanding the world. Arendt's question was born at the moment that transparent assumption was rendered opaque. But such moments do not arrive on their own. In most cases, transparent assumptions remain transparent, and questions that do not fit those assumptions stay unseen, resting in the drawer.

The Structures That Make Things Invisible

The reason Grand Quests are invisible is not merely a matter of perception. The very structures that a scholarly community has built to function successfully push such questions out of view.

First, the lens of peer review.

Scholarship operates through peer review. Publication, funding, and scholarly reputation all depend on the evaluation of fellow researchers. This system is an essential mechanism for maintaining the quality of scholarship. However, peer review presupposes an implicit consensus on ‘what constitutes a meaningful question in this field.’ This is the sharing of paradigms that Kuhn described. Evaluation works when evaluator and evaluated are within the same paradigm. But questions that cross the boundaries of a paradigm fall outside this consensus, making evaluation itself difficult. The response ‘Why would you ask that?’ is not hostility. It means that the existing lens cannot discern the significance of that question. When this is internalized, it becomes self-censorship. A researcher holds a question yet puts it back in the drawer before ever taking it out.

Second, the inertia of research optimization.

As a lab stabilizes and results accumulate, research becomes progressively optimized. It becomes clear which topics yield publications, which methodologies are efficient, and which journals will accept the work. This optimization is the productivity of normal science itself. And the higher this productivity, the more any question that deviates from that trajectory feels like a cost. The questions in the drawer are inherently uncertain. How to solve them is unknown, whether results will follow is unclear, and they are unlikely to produce papers in the short term. The better a lab works, the more invisible those questions become. Even if they are visible, they become harder to take out.

Third, the standard of completeness. ‘There is no concrete methodology.’ ‘The expected outcomes are unclear.’ ‘How can I propose such a vague question?’ This hesitation arises because Grand Quests are being judged by the criteria of normal science. In normal science, good research is research where the problem is well defined, the method is proven, and the results are predictable. A Grand Quest cannot meet these criteria. It is the situation of having to describe a roadmap that does not yet exist in the language of an existing roadmap. The more dominant this standard is, the more the Grand Quest appears incomplete, and what appears incomplete vanishes from view.

Peer review, research optimization, and the standard of completeness are structures essential to making scholarship efficient. They are the very mechanisms that make normal science strong. But the more powerfully

these mechanisms operate, the more structurally invisible the Grand Quest becomes. It is not for lack of individual courage. It is because the system does not possess a lens capable of seeing that question.

Learning the Textbook and Rewriting the Textbook

Entering a scholarly community is a process of learning the textbook. One learns what counts as an important question, what methods are standard, and what results are meaningful. In Kuhn's terms, it is the process of initiation into a paradigm. This is an essential process. Thanks to the knowledge and methodologies accumulated by senior researchers, we can start from a further point.

Yet looking at the history of scholarship, there are moments when the textbook is rewritten. This is what Kuhn called a 'scientific revolution.' It is the moment when anomalies that the existing paradigm cannot explain accumulate, someone re-examines the assumptions written in the textbook, reframes the problem in a different way, and proposes a new starting point.

The story of AlphaGo illustrates the process by which a textbook is rewritten (see case study). For 20 years, artificial intelligence textbooks stated that 'problems requiring intuition are difficult to approach with current technology.' This was not incorrect, and countless researchers had built important foundations upon this textbook. The research team at DeepMind posed a different question. 'If intuition is the compression of vast experience, could it not be replicated through sufficient data and learning?'

When AlphaGo defeated Lee Sedol in 2016, the sentences in the textbook were rewritten. Upon this shift, research such as AlphaFold (protein structure prediction) and AlphaGeometry (mathematical proof) became possible.

AlphaGo was made possible not by DeepMind's question alone. It was because 20 years of accumulated foundations in neural network theory, reinforcement learning algorithms, and large-scale computation had been built upon that textbook. Learning the textbook and rewriting the textbook are not separate undertakings. The accumulation of the former makes the latter possible, and when the latter produces a new textbook, the former begins again upon it.

For this cycle to work, however, the question that seeks to rewrite the textbook must be visible. And as we have seen, that question is hard to see because of the success of the existing paradigm. The perceptual wall created by success, the transparent assumptions, the structural filters of peer review, optimization, and completeness. These layers overlap to place the Grand Quest out of sight.

Because the perceptual wall and the structural wall are layered together, it is difficult to take questions out of the drawer. Then what kind of question can become a Grand Quest worth bringing out? We will examine this in the next chapter.

Closing Chapter 2

The perceptual wall, where successful assumptions become transparent, and the structural wall, created by peer review and the pressure of productivity, are layered together.
To take a question out of the drawer is to cross both walls at once.

[Case Study] AlphaGo: The Moment the Textbook Was Rewritten

In 1997, Deep Blue defeated the world chess champion. But artificial intelligence researchers simultaneously confirmed a limitation. The approach of rapidly searching through possible moves does not work in Go. The number of possible positions in Go is 10 to the power of 170, more than the number of atoms in the universe. For the next 20 years, artificial intelligence textbooks read as follows: 'Go is a representative case demonstrating the limits of search-based approaches.' 'Problems requiring intuition are difficult to approach with current technology.' These were not incorrect statements. Countless researchers had advanced neural network theory, reinforcement learning algorithms, and large-scale computation upon this textbook.

Demis Hassabis of DeepMind had a different question. Having studied both neuroscience and artificial intelligence, intuition for him was not a mysterious faculty but a mechanism by which the brain compresses vast experience to make rapid judgments. 'If intuition is the compression of experience, could it not be replicated through sufficient data and learning?'

AlphaGo learned patterns not through explicit rules but through millions of game records and self-play. When AlphaGo defeated Lee Sedol in their 2016 match, the sentences in the textbook were rewritten. 'Problems requiring intuition are difficult to approach' became 'Intuition, too, can become the subject of learning.'

AlphaGo was made possible because of the foundation accumulated over 20 years upon the textbook. The starting point of AlphaGo was not a faster computer built on that accumulated foundation, but a gaze that sought to see afresh what had become so familiar as to be transparent.

Chapter 3.

Chapter 3.
What Kind of Question Becomes a Grand Quest

"Beyond Critique,
Toward a New Framework"

GRAND QUEST

Chapter 3.
What Kind of Question Becomes a Grand Quest

"Beyond Critique, Toward a New Framework"

Questions That Go Beyond Critique to Establish Alternatives

A Grand Quest refers to 'a fundamental question that dismantles a prevailing assumption that has operated as a dominant premise in a given field, proposes a new conceptual framework to replace it, and thereby carries the potential for the formation of an independent research domain or scholarly genre distinct from what existed before.'

Not every question in the drawer becomes a Grand Quest, however. Some questions fill gaps in the existing roadmap. They are important and meaningful, but they do not change the roadmap itself. Other questions raise doubts about existing assumptions but fail to offer an alternative. They stop at critique. A Grand Quest differs from both. It re-examines existing assumptions, and in their place, proposes a new starting point.

The research of Professor V. Narry Kim of Seoul National University and her colleagues illustrates this. For a long time in molecular biology, RNA was understood as a messenger that relays information from DNA to proteins. DNA and proteins stood at the center of research, while RNA was a secondary element shuttling between the two. This framework had operated effectively for a long time.

Professor Kim and her colleagues had a different question. 'What if RNA were viewed not as a mere messenger but as an active agent that regulates gene expression?' This question was not an attempt to revise the details of existing theory. It was an attempt to reset what the central axis organizing biological phenomena should be.

What matters is that this question did not stop at critiquing the existing framework. It proposed a new regulatory concept with RNA at the center. As the mechanisms by which microRNA regulates gene expression were revealed, the focus of life sciences shifted from a unidirectional, protein-centered flow to a complex regulatory network. Today, RNA biology has grown into an independent field, and the development of RNA therapeutics is actively underway. It goes without saying that RNA biology emerged by standing on the achievements of molecular biology. New questions are not born in a vacuum. They can only be tested upon existing foundations.

A similar turn occurred in the history of art. In 1917, Marcel Duchamp took a urinal purchased from a hardware store, turned it upside down, titled it 'Fountain,' and submitted it to an exhibition. The exhibition committee rejected it. For thousands of years, art had been something made by the artist's skill and hand. What Duchamp did was not mere provocation. 'What if the essence of art lies not in the act of making but in the act of selection and the shift of context?' This question dismantled the existing framework of technical production and erected in its place a new framework of conceptual selection. Conceptual art, installation art, and performance grew upon this foundation in the years that followed.

The Solution May Be Unknown, but the Direction Must Be Present

When writing a research proposal, we formulate hypotheses, present methodologies, and describe expected outcomes. This predictability guarantees the credibility of research, and the research support system is calibrated for this kind of work. A Grand Quest, however, cannot easily follow these rules. It is a question for which no proven path yet exists. How to solve it, what methodology to use, what to count as a result: all of these are unclear at the outset.

When Karikó asked ‘Could mRNA be used as medicine?’, the solution of pseudouridine substitution was discovered much later. What she had at the beginning was only a direction: ‘mRNA as an informational molecule.’ The same was true when DeepMind asked ‘Could Go be approached as a learning problem?’ The precise algorithm was unknown. But the shift in direction from rule-based to learning-based was clear.

A Grand Quest offers not a solution but a direction. A compass does not reveal the destination. It merely points the way. Yet direction alone is sufficient. Because upon that direction, trial and error begin to acquire meaning.

In the existing research support system, ‘I do not know how to solve it’ is a fatal weakness. In the Grand Quest, however, it comes closer to evidence that the question is sufficiently fundamental. If a solution can be readily guessed, the question is likely an extension of the existing roadmap.

Not a Single Paper but a New Starting Line

Conventional research has a relatively clear beginning and end. A problem is defined, a method is applied, results are derived, and a paper brings it to a close. This is the basic cycle of research and the structure that sustains scholarship. A Grand Quest does not close within this cycle. Instead, it creates a new starting line from which multiple researchers can explore in their own ways.

SpaceX's reusable rocket illustrates this. For half a century, the assumption that rockets are single-use was the standard of space development. Under this assumption, weight reduction was the supreme value, and extreme mass savings were the goal of design. Space programs including NASA had operated successfully upon this standard.

The question SpaceX posed was simple but fundamental. 'What if rockets were reused like airplanes?' The moment this question was raised, what counted as an important problem changed. The core challenges became not weight but structural reliability that could withstand repeated use, durability against heat and impact, and precision re-landing control.

This question did not end as a single technical achievement. Research problems that had previously been secondary or nonexistent emerged as central topics. Retro-propulsive landing, recovery trajectory control, repeated use of thermal protection systems. A new roadmap was created, and upon it, numerous researchers began building foundations. Naturally, this was possible because of the foundations in rocket engineering, control systems,

and materials science accumulated over half a century under the single-use rocket paradigm.

When thinking about a Grand Quest, one can ask this question: does this question help solve existing problems better, or does it generate an entirely new kind of problem? If the latter, it is not a single research topic but a new starting line.

To summarize, a Grand Quest has three characteristics. First, it re-examines existing assumptions and erects a new framework in their place. Second, it offers not a specific solution but a direction of inquiry. Third, it does not end as a single outcome but becomes a new starting line.

Then what process does such a question go through to grow into a new field of scholarship? We will examine this in the next chapter.

Closing Chapter 3

A Grand Quest has three characteristics. It re-examines existing assumptions and erects a new framework. It offers not a solution but a direction. It does not end as a single outcome but becomes a new starting line.

[Case Study] Behavioral Economics: Re-examining the Premise and Establishing a New Starting Line

From the mid-twentieth century onward, economics achieved great success upon a framework that assumed humans to be rational agents. Individuals maximize utility, and markets are explained as aggregates of rational choices. This premise provided powerful explanatory force for growth, market efficiency, and policy analysis.

Daniel Kahneman and Amos Tversky had a different question. 'Do humans actually judge and choose that rationally?' This question was not an attempt to revise the details of existing theory. It was a question that re-examined, at the most fundamental level, how economics had assumed human behavior to work. Through experiments, the two researchers demonstrated that people systematically distort probabilities, perceive losses as larger than equivalent gains, and arrive at entirely different decisions for the same choice problem depending on how it is presented. What mattered was that this irrationality had predictable patterns.

This question was not welcomed at the center of economics at first. But it did not stop at critique. It proposed a new framework: predictable irrationality. Researchers gradually gathered, and new branches of inquiry emerged, including experimental economics, behavioral finance, and nudge theory. In 2002, Kahneman was awarded the Nobel Prize in Economics.

Behavioral economics was made possible because of the foundations of economics accumulated upon rational choice theory. But when a question does not stop at critique and erects a new framework, that question becomes a new starting line.

Chapter 4.

Chapter 4.
From a Question in the Drawer to a Field of Scholarship

"Small Attempts, Failure, and Accumulation"

GRAND QUEST

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Incompleteness at the Start Is Inevitable

What happens when a question is taken out of the drawer? In most cases, the question does not come in the form of a completed research plan. There is a direction, but the supporting evidence is scarce. No proven method exists yet, and explaining the question to colleagues is not easy. The hypothesis is rough, the experimental design is unstable, and the prospect of results is uncertain. Such questions are likely to receive the evaluation 'insufficiently prepared' in a funding review. The problem definition appears too broad or vague, and it seems difficult to produce results in the short term.

But this is not because the question is immature. It is because the language, methods, and evaluation systems needed to address that question have not yet been formed. A question that seeks to draw a new roadmap is difficult to explain in the language of the existing roadmap. In the history of scholarship, most important turning points began from just such incomplete starting points.

What matters is not the current state of the research but the direction the question points toward. If the question re-examines existing assumptions and points toward new possibilities, that very incompleteness is the margin for future growth.

Starting with Small Attempts

Taking a question out of the drawer does not mean one can march straight toward an answer. No proven path exists yet. What is needed at this point is small attempts. The reason many researchers hesitate before a new question is the pressure to have everything properly in place before starting. Sufficient preliminary data, a refined experimental design, a clear prospect of success. But for a question that seeks to draw a new roadmap, these conditions cannot be met from the outset. It is a path no one has walked before.

Small attempts begin with setting down that burden. Rather than perfect preparation, what matters is trying one small experiment in the direction the question points toward. It is fine if that experiment fails. What matters is that the failure reveals the direction for the next attempt. Hypotheses should be broken into small pieces and tested one by one. There is no need to draw up a grand plan to prove everything at once.

Shinya Yamanaka's research on induced pluripotent stem cells (iPS cells) illustrates this. Upon the foundations accumulated over decades in stem cell biology, transcription factor research, and cell culture technology, he posed a provocative question: 'Could differentiated somatic cells be returned to their initial state?' At the time, this was not a mainstream question. He did not begin with a perfect experimental design. His first attempt was a somewhat reckless, simple experiment: introducing 24 transcription factors all at once. Most cells failed to reprogram.

But this failure became the starting point for the next attempt. It became

clear which factors were unnecessary, and the 24 factors were gradually reduced until they converged on four. This was not a single stroke of genius but a result produced by the accumulation of small attempts. iPS cells subsequently expanded into regenerative medicine, disease modeling, and drug screening, and today they have established themselves as an independent field of research.

Questions Grow Through the Accumulation of Trial and Learning

Something important happens in the process of repeating small attempts. The question itself evolves. The question first taken out of the drawer does not retain its original form. Through small attempts, the question gradually becomes more refined, and at times it is fundamentally reconstructed. A vague direction transforms into a concrete path of exploration, and above the possibilities eliminated through failure, new directions emerge.

We commonly use the expression ‘trial and error.’ But this expression presupposes that a correct answer exists. In the attempt to draw a new roadmap, there is no correct answer. On a path no one has walked, it is impossible to know in advance what constitutes error and what does not. A more accurate expression, therefore, is ‘trial and learning.’ Trial and error presupposes a correct answer; trial and learning presupposes a direction. When walking a path no one has traveled with a compass in hand, what one can do is simply take a step. That step may lead to a dead end. But the

information gained from that step enables the correction of the next course.

The history of the lithium-ion battery illustrates this. In the 1970s, the first question was simple. ‘Could a rechargeable battery be made using lithium?’ Stanley Whittingham, noting that lithium ions could move back and forth between layered materials, built a battery. It worked, but the charge capacity was poor and the voltage was low. It was an incomplete success.

This failure became the starting point for the next question. John Goodenough asked: ‘What if lithium ions were released from the cathode rather than the anode?’ He achieved higher voltage, but safety problems remained. Akira Yoshino posed the third question. ‘If lithium ions alone could be made to move without using lithium metal itself, could the safety problem be solved?’ A carbon-based anode was the answer. The three shared the 2019 Nobel Prize in Chemistry.

What this case demonstrates is that a question does not evolve within one person alone. The vague question ‘Is a lithium battery possible?’ was refined through ‘What cathode structure is effective?’ to ‘Is it possible to design a system that moves only ions without lithium metal?’ What the history of the lithium-ion battery reveals is a genealogy of questions. One researcher’s incomplete success became the starting point for the next researcher, and the evolution of questions across three generations created a single new roadmap. Upon that roadmap, countless researchers continue to build foundations today, working on electrode material improvement, electrolyte optimization, and safety enhancement.

Taking a question out of the drawer is only the beginning. That question is explored through small attempts, refined through failure, and becomes a field of scholarship through accumulation. The Grand Quest Program is a space for this process. Incomplete beginnings, unpredictable paths, accumulation that takes time. It is a space that supports this process, one that does not fit existing evaluation systems, as meaningful trial and learning.

Then how can one discover the questions in the drawer within one's own field? We will examine this in the next chapter.

Closing Chapter 4

Incomplete beginning, small attempts, the evolution of the question through the accumulation of trial and learning. This is the path by which a question in the drawer becomes a field of scholarship.

[Case Study] Geoffrey Hinton: The Time Spent Holding On to a Question

The question Geoffrey Hinton posed was simple. ‘Can a machine learn representations on its own through data?’ At a time when rule-based artificial intelligence was the mainstream, this question belonged to the periphery. Artificial neural networks were evaluated as computationally expensive and inefficient.

During the AI winter that stretched from the late 1970s to the early 2000s, neural network research could not occupy a central place in the academic world. Hinton held on to the question. In 1986, he showed that multi-layer neural networks could be trained using the backpropagation algorithm. However, in deeper layers, the problem of vanishing gradients arose. This failure became the starting point for the next question. In 2006, he circumvented the problem by pre-training each layer individually before fine-tuning the whole.

In this process, the question evolved as well. ‘Can a machine learn on its own?’ was refined into ‘Does learning work in deep architectures?’, and then into ‘What becomes possible when combined with large-scale data?’ When Hinton’s research team overwhelmed existing methods at the ImageNet competition in 2012, it was the moment this long accumulation of trial and learning received visible proof.

What makes Hinton’s case exceptional is the length of time. From the late 1970s to 2012, he held on to the question for roughly 30 years. What he endured during the AI winter was not the price of personal stubbornness. It was the misalignment between the time a question needs and the time evaluation systems allow.

Chapter 5.

Chapter 5. When Are the Questions in the Drawer Formed?

"The Moment When the Obvious Ceases to Be Obvious"

GRAND QUEST

Chapter 5.
When Are the Questions in the Drawer Formed?

"The Moment When the Obvious Ceases to Be Obvious"

Two Moments When Questions Are Born

If you carry a question in your drawer, when did it take shaped? There are usually two kinds of moments. One is the moment when something taken for granted suddenly ceases to look self-evident. The other is the moment when something unexplained keeps catching your eye.

The first moment arrives when a prevailing assumption is seen anew. Every field of scholarship has assumptions that are no longer subject to debate. The basic premises written in the opening chapter of a textbook. The methodological consensus shared by most researchers. The things that appear in the introductions of papers with phrases like 'generally' or 'as is well known.' These became assumptions precisely because they had operated successfully for a long time. And it is precisely because of that success that questioning the assumptions themselves becomes difficult.

Yet occasionally, there are moments when these assumptions suddenly look unfamiliar. The moment when 'the way things are' becomes 'why are things this way?' When did this assumption come to be taken for granted? In what context was it formed? What did it make possible, and what did it render invisible? The moment such questions arise, the seed of a question in the drawer is formed.

The second moment arrives when something unexplained keeps catching your eye. In the course of research, one sometimes encounters data that existing theories cannot explain, phenomena that do not match predictions. The scholarly community typically handles such things as measurement error,

noise, or exceptional cases. This is a rational response. If every exception were accommodated, the theory itself would collapse.

Yet occasionally, that noise keeps catching your eye. Data that recur but cannot be explained. Phenomena that are reproducible yet do not fit the theory. Territories that researchers avoid. When these things continue to nag, the seed of a question in the drawer is formed.

What is interesting is that such doubts rarely surface in formal settings. In papers and conference presentations, only what has been organized according to mainstream theory appears. But at dinner after a conference, in hallway conversations, in candid exchanges within the lab, different stories circulate. 'To be honest, I always thought that assumption was strange too.' 'Data that this methodology cannot explain keep coming up.' It is in this gap that the seeds of questions are found.

Where Different Perspectives Meet

There is yet another important occasion on which questions in the drawer are formed. It is when one encounters the perspective of another field. When an assumption taken for granted in one field meets the gaze of another discipline, what was 'obvious' can be revealed as 'not obvious at all.' A question that no one asks in my field may look strange to a colleague from a different discipline. That strangeness becomes the seed of a question.

The birth of cognitive science illustrates this (see case study). Until the

1950s, psychologists studied behavior, linguists analyzed grammar, and computer scientists explored computation, each in their own language. When these fields began to meet, the assumptions each had taken for granted became objects of questioning. The question 'Could the mind be understood as an information-processing system?' was one that could not have emerged from within any single field.

Such encounters, however, do not happen easily. Within the same campus, sometimes within the same building, it is not uncommon for researchers to spend years unaware of each other's work. The walls between departments are often thicker than physical distance. What is more regrettable is when this distance goes beyond mutual indifference to mutual dismissal. Unfamiliar questions are treated as 'unimportant questions,' and the methodologies of other fields are readily excluded on the grounds that they are 'not rigorous.' Some even dismiss an entire field with a single remark: 'I know perfectly well what they do.' In such an atmosphere, researchers retreat behind the fence of their home department. Exploring unfamiliar territory feels riskier than efficiently solving problems already agreed upon.

Looking at the history of scholarship, the most dynamic research domains were mostly formed at the intersections of existing disciplines. The departments that exist today were themselves fields that, at some point in the past, originated from someone's new question. The landscape of scholarship is not fixed. Whether the walls between disciplines become barriers or windows into other worlds is a choice to be made. Individual curiosity and

openness matter, but if questions that cross boundaries are left solely to individual courage, they will remain exceptions. For questions to persist and expand, institutional spaces that can receive them are needed.

Now, finally, let us examine why Seoul National University is creating a space for these questions, and why now.

Closing Chapter 5

The questions in the drawer begin to sprout at the moment when assumptions appear unfamiliar, when the unexplained catches one's eye, and when perspectives from different fields meet. Especially where boundaries converge, questions that could never have emerged from within any single field are born.

[Case Study] The Birth of Cognitive Science: When Different Perspectives Met

Until the mid-1950s, the study of the human mind was the exclusive territory of individual fields. Psychologists observed behavior, linguists analyzed grammar, and computer scientists explored mechanical computation. Each field was operating productively upon its own assumptions and methodologies.

When these fields began to meet, however, the assumptions each had taken for granted began to look unfamiliar. At the 1956 Dartmouth Conference and interdisciplinary gatherings at MIT, when Noam Chomsky criticized behaviorist theories of language and argued that the human mind possesses an internal structure that generates language, this shook the behaviorist assumptions of psychology as well. When Herbert Simon and Allen Newell proposed that the human problem-solving process can be simulated by a computer program, this provided a new language for philosophy's mind-body problem.

Questions that could not have emerged from within any single field arose at the point where boundaries met. 'Could the mind be understood as an information-processing system?' This question belonged to no single field. Cognitive science was born in this way. Cognitive science was made possible by the achievements that psychology, linguistics, philosophy, and computer science had each built independently. Had those achievements remained within the walls of their respective disciplines, however, this question would never have been born.

Chapter 6.

Chapter 6.
A Space for the Questions in the Drawer

"Why, Now, Seoul National University"

GRAND QUEST

Chapter 6.
A Space for the Questions in the Drawer

"Why, Now, Seoul National University"

What Korea Has Built and the New Challenge Ahead

What Korea has achieved over the past half century is an accomplishment nearly without parallel in world history. Rising from postwar ruins to become a major pillar of the global economy, the journey was in itself a civilizational event. What Korea demonstrated in semiconductors and shipbuilding, steel and automobiles was an extraordinary capacity for implementation. Learning the paths pioneered by advanced nations, yet reconstituting them to fit the context, sometimes implementing them more precisely than the originators. This was not mere imitation. It was the art of compression and leapfrogging, the art of implementation.

The same is true in the domain of scholarship. Korean researchers rapidly joined the front lines of the global academic community and have accumulated independent achievements. The contributions of Korean scholars in every field are now recognized anywhere in the world.

This success is opening a new phase. In many domains, Korea now stands at a point where there are no longer precedents to follow. In memory semiconductors, displays, batteries, and shipbuilding, Korea is already at or near the highest level in the world. At the same time, the landscape has changed. Countries that once trailed behind are closing in rapidly. This is not a narrative of crisis. It is rather a sign of maturity. The question is what coordinates to set in this new situation.

Here we return to what this guide has been discussing. In scholarship, there are two kinds of work. Building foundations upon a roadmap already drawn,

and drawing a new roadmap. Both are necessary. Korea has demonstrated outstanding capability in the former. That capability will remain Korea's core asset going forward.

Whether there has been sufficient space for the latter, however, is worth reconsidering. Whether there has been enough space for questions that seek to draw new roadmaps, for research that explores directions not yet proven, for bringing out the questions that have been kept in the drawer. This is not a denial of the value of existing research. It is a question of whether support for these two kinds of work has been in balance.

The Role of Seoul National University

The history of Seoul National University has been intertwined with the modern history of Korea. In the era of industrialization, Seoul National University provided the intellectual foundations and human resources needed for national development. During the process of democratization, it served as a base for critical reflection. In the phases of the information revolution and globalization, it elevated the standing of Korean scholarship by building world-class research capabilities. This contribution will continue. But a new dimension needs to be added.

Why Seoul National University?

First, Seoul National University is the most comprehensive university in Korea, where the widest range of academic disciplines coexist in a single space. Natural sciences and engineering, humanities and social sciences, medicine and the arts share one campus. As we saw in Chapter 5, questions in the drawer are often formed when they encounter the perspectives of other fields. Questions are born where boundaries meet. This condition exists at Seoul National University.

Second, Seoul National University is an institution entrusted with substantial resources by the nation. This trust carries the responsibility to pursue long-term value beyond short-term results. Research that opens future possibilities beyond immediate utility. Research that undertakes meaningful challenges beyond assured success. Seoul National University bears the responsibility to lead such research. The reason Korean society entrusts resources to Seoul National University is the expectation that the university will do on society's behalf what society cannot do for itself in the near term.

Third, Seoul National University brings together researchers who represent Korea in their respective fields. Each of them carries insights and doubts accumulated over long years in their disciplines. There are questions that have not yet been put into words but have been held close. When these questions are brought out, refined, and shared, the entire Korean academic community and research ecosystem can advance to a new stage. When these researchers take on challenges first, undergo trial and learning first, and share that experience,

it becomes the most valuable intellectual asset for their peers and for the researchers who follow.

The Aspirations of the Grand Quest Program

The Grand Quest Program is an attempt to fill an empty space in Korea's research ecosystem. As we have seen, the very structures that sustain the efficiency of scholarship become a deep crevasse for questions that seek to explore new directions. This is why questions whose direction is clear even though their solutions are unknown, and attempts to open territories not yet proven, remain in the drawer.

The Grand Quest Program is a space for this gap. The Program aspires to three things.

Posing questions that no one else asks, and encouraging trial and learning.

Fundamental questions that dismantle dominant assumptions and propose new conceptual frameworks. Questions that appear heretical at first but ultimately become the new orthodoxy. Such questions have always existed in the history of scholarship. Doubts formed through long years of research experience. Questions that the explanations in textbooks cannot resolve. Intuitions that mainstream scholarship turns away from but that cannot be abandoned. These are the seeds of the Grand Quest.

To pose a daring question also means accepting the possibility of failure. Not every daring question leads to success. Most, in fact, fail. But when what was learned from that failure and how the direction of the next attempt was adjusted are recorded and shared, it becomes an asset of the scholarly community. This is the trial and learning discussed in Chapter 4. The Grand Quest Program respects and supports such daring failures.

Asking first about the future of the community.

In the history of scholarship, the most far-reaching questions have usually arisen from the deepest sense of public purpose. They did not begin with the question ‘What research should I do to gain recognition?’ but with the question ‘What is the question this world most needs?’ When oneself is placed at the center, questions become small. When the community is placed at the center, questions become large.

The advent of the age of artificial intelligence, the future of life, the challenge of sustainability. These problems are difficult to approach within any single existing scholarly framework alone. New concepts, new methods, and new convergences are needed. As a national university, Seoul National University bears the obligation to think preemptively about the problems facing the community called Korea. The Grand Quest Program elicits questions that answer this public obligation, and supports the researchers who take on those questions.

Raising the base camp for the next generation.

What the Grand Quest Program pursues is not only immediate results. What matters more is laying stepping stones so that the next generation of researchers can pose even bolder questions. The challenges and failures of senior researchers become the starting points for those who follow. What was attempted, what did not work, and what directions remain open. This experiential knowledge is an asset of the scholarly community, transmitted across generations. The trial and learning we accumulate now will become the shoulders upon which the next generation stands.

This is deeply connected to the future of education in the age of artificial intelligence. We have entered an era in which artificial intelligence can deliver the knowledge organized in textbooks. But what only people can do in education is this: showing the act of hesitating before a question that has no answer yet and still taking a step forward, demonstrating the process of recording failure and finding a new direction. Students learn by reading their professors' papers, but they learn more deeply by watching their professors ask questions and take on challenges. A professor poses a question, and a student, watching the process, creates an even larger question. This is an education more powerful than any textbook, and it is the most essential role a university can play in the age of artificial intelligence. It is also the vision of campus life that the Grand Quest Program aspires to.

Structural Features of the Program

To realize these aspirations, the Grand Quest Program is organized as follows.

Grand Quest Agora: A Public Square for Bringing Out Questions

The Program begins by gathering questions. Just as the ancient agora was a public square where citizens assembled to deliberate on public affairs, the Grand Quest Agora is an intellectual square where researchers bring out the questions from their drawers and share them. Researchers from all fields can propose the questions they have carried. Departmental boundaries are irrelevant. There are no strict requirements on the completeness of a question. What matters is whether the question re-examines existing assumptions and points toward new possibilities. Selected questions from among those proposed receive incentive grants. It is a space where the act of posing a question, not providing a solution, can earn recognition.

Presenting Grand Quests and Soliciting Solution Proposals

By synthesizing the questions presented at the Agora and discussions among experts across fields, “SNU Grand Quests” are presented. An open call for solution proposals follows for the presented Quests. The evaluation criterion is not the likelihood of success but the potential of a new approach and the possibility of learning through the attempt.

Research Support Where Failures Accumulate

The most distinctive feature of this research support is that it does not divide outcomes into success and failure. What matters is not the failure itself but what was learned from it, and how that learning was recorded and shared.

Another distinctive feature is the freedom to change the research path. A new discovery may demand a shift in direction, and an unexpected obstacle may require a detour. What matters is not adherence to the plan but the evolution of the question.

Sharing the Process

Not only successful results are presented. The entire process of attempts, failures, and changes of course is shared with fellow researchers and subsequent generations. This helps prevent the repetition of the same failures, and one researcher's failure can provide new inspiration for another. The culture of sharing failure itself contributes to transforming the culture of research.

The Grand Quest Program does not seek to replace existing research. Research that builds foundations upon the roadmap will remain the backbone of scholarship. It simply creates, alongside it, a space for the challenge of questions that seek to draw new roadmaps. When both kinds of work happen together, scholarship can reach further.

Closing Chapter 6

Seoul National University is launching this Program for three reasons. A space where questions no one else asks can be attempted. The public obligation to ask first about the future of the community. And the work of raising the base camp so that the next generation can pose even larger questions. These three are the aspirations of the Grand Quest Program.

Epilogue

If You Have a Question in Your Drawer

In a researcher's drawer, there are strata of time.

At the very bottom lies the question from the days of first entering scholarship.
A question from a time when nothing had yet been proven
but something was already sensed. Above it, daring questions pushed
aside during the period of first grants and the pressure to produce results
accumulate. Then come the questions set aside with the judgment 'not now' as
the lab found its footing. A question folded away because of a single line from a
reviewer. A question shared with a colleague in a conference hallway, laughed
off together, and let go. A question that surfaced while reading
a book outside one's field but had nowhere to return to.

These strata grow thicker as a career deepens.
And the only person who knows their weight is the one who carries them.

What is interesting is that these questions do not sit still inside the drawer.
What began as no more than a rough intuition changes shape as the seasons
of research pass. Some merge with other questions.
As the landscape of a field shifts,
what was once premature finally finds its moment.
The questions in the drawer are not asleep.
They are ripening alongside the researcher's own time.

The most important turns in the history of scholarship happened when
a question that had long ripened in someone's drawer finally came out.
And the reason that question had to remain in the drawer for so long was not a
flaw in the question. It was a gap in the system.

If there is a question ripening in your drawer,
now may be the time to bring it out.

A space for that question is here.

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For details on the Program schedule, submission formats, and evaluation procedures, including the Grand Quest open call and Challenge proposals, please visit the Grand Quest website (grandquest.snu.ac.kr).

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