APRE

Agenzia per la Promozione
della Ricerca Europea

PREPARING AN ERC PROJECT FROM RESEARCH IDEA TO PROPOSAL WRITING

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INTRODUCTION

The European Research Council (ERC) is widely recognised as one of the most prestigious and competitive funding programmes for frontier research. Every year, thousands of researchers submit proposals of outstanding scientific quality, yet only a small proportion are ultimately funded. The difference between a competitive proposal and a successful one often lies not only in the quality of the science, but also in the ability to communicate a compelling scientific vision that resonates with evaluators.

This guide has been developed by the Italian ERC team in APRE, the Agency for the Promotion of European Research, to support researchers preparing an ERC proposal by explaining not only what should be written, but also why certain elements are important from the perspective of the evaluation process. While the ERC provides comprehensive official documentation describing the rules of participation, the proposal template and the evaluation procedure, applicants often ask a different set of questions: *What are evaluators really looking for? How should I present my idea? What distinguishes an excellent proposal from a merely good one?* The purpose of this handbook is to address those questions.

Rather than providing a section-by-section commentary on the proposal template alone, this guide aims to explain the principles that underpin successful ERC applications. Throughout the chapters, readers will find practical advice, strategic considerations, annotated proposal excerpts, discipline-specific case studies, examples of common pitfalls, and reflections from the evaluator's perspective. The intention is to help applicants understand the logic of the ERC evaluation process and to develop proposals that are scientifically ambitious, intellectually coherent and convincingly argued.

The handbook follows the natural journey of an ERC application. It begins by introducing the philosophy of the ERC and the concept of frontier research before examining how excellence is evaluated and how applicants should develop an ambitious research idea. It then explores the preparation of the proposal itself, including Part B1, Part B2, the curriculum vitae and track record, scientific risk, resources and budget. The final chapters address strategic decisions that applicants increasingly face, such as choosing between Starting and Consolidator Grants under the new eligibility windows, understanding when an ERC Plus Grant is the appropriate funding instrument.

This guide is intended for researchers applying to any individual ERC grant scheme—Starting, Consolidator, Advanced or ERC Plus. It is also designed to support research managers, grant advisors, mentors and research support offices involved in assisting ERC applicants.

No guide can guarantee success. Every proposal is unique, every scientific field has its own characteristics, and every evaluation panel exercises independent scientific judgement. Consequently, the recommendations presented in this handbook should not be interpreted as prescriptive rules or a formula for success. Instead, they should be viewed as practical guidance intended to help applicants make informed decisions while developing their own scientific narrative.

Ultimately, the most successful ERC proposals are not those that simply follow a template. They are those that present an original scientific vision, demonstrate intellectual courage, and convince evaluators that the proposed research has the potential to change the way a field thinks.

DISCLAIMER

This handbook has been prepared by the Italian National Contact Points (NCPs) for the European Research Council (ERC) based in the Agency for the Promotion of European Research – APRE. The aim is to support researchers in the preparation of ERC proposals. It is an unofficial guidance document and has been produced solely for information and training purposes. It does not replace or supersede the official ERC Work Programme, Information for Applicants, proposal templates, evaluation forms or any other official documentation published by the European Research Council Executive Agency (ERCEA) or the European Commission. The views, interpretations and recommendations presented in this guide reflect the professional experience of the authors in supporting ERC applicants and should not be regarded as official interpretations of ERC rules or evaluation procedures. In the event of any discrepancy between this handbook and the official ERC documentation, the latter shall always prevail. Applicants are therefore strongly encouraged to consult the most recent official ERC documents applicable to the call to which they intend to apply.

List of Acronyms

ACRONYM	MEANING
APRE	Agency for the Promotion of European Research
B1	Part B1 of the ERC Proposal (Extended Synopsis, Curriculum Vitae and Track Record)
B2	Part B2 of the ERC Proposal (Scientific Proposal)
CV	Curriculum Vitae
EC	European Commission
EIC	European Innovation Council
ERC	European Research Council
ERCEA	European Research Council Executive Agency
ERC AdG	ERC Advanced Grant
ERC CoG	ERC Consolidator Grant
ERC PoC	ERC Proof of Concept Grant
ERC Plus	ERC Plus Grant
ERC StG	ERC Starting Grant
ERE	European Research Executive (where applicable in future references)
EU	European Union
F&T Portal	Funding & Tenders Portal

HE	Horizon Europe
HI	Host Institution
NCP	National Contact Point
PI	Principal Investigator
PoC	Proof of Concept
R&I	Research and Innovation
TR	Track Record
TRL	Technology Readiness Level
WP	Work Package

Note: Throughout this handbook, the term **ERC** refers to the European Research Council as a funding body, while **ERCEA** refers to the European Research Council Executive Agency, which manages the implementation of the ERC funding programmes on behalf of the European Commission. Unless otherwise specified, the term **Principal Investigator (PI)** refers to the lead applicant responsible for the scientific direction and implementation of the proposed project.

1. UNDERSTANDING THE EUROPEAN RESEARCH COUNCIL

1.1 Why the ERC Exists

The European Research Council (ERC) was established in 2007 as the flagship programme for frontier research within the European Union. Unlike most funding programmes, which aim to address predefined societal challenges or industrial priorities, the ERC follows a radically different philosophy: excellent researchers should be free to pursue their most ambitious scientific ideas, wherever these may lead. This seemingly simple principle has profound implications for proposal writing. Many applicants approach the ERC with the mindset acquired through collaborative projects, national funding schemes or mission-oriented programmes. They focus on demonstrating impact, delivering milestones, building large consortia or addressing policy priorities. While these aspects are valuable in other contexts, they are not the driving force behind ERC evaluation. Instead, the ERC seeks projects that have the potential to transform scientific understanding. The programme is based on the conviction that major scientific breakthroughs rarely emerge from incremental advances. They often originate from researchers willing to challenge existing paradigms, question established assumptions and explore high-risk directions. This philosophy explains why proposals that appear "safe" or "well planned" are often less competitive than proposals that tackle genuinely difficult scientific questions.

NCP'S TIP

Do not ask yourself: *Is my project scientifically good?*

Ask instead: *If successful, would my project change the way researchers think about this problem?*

That is the question many ERC panel members implicitly ask while reading the first page of your proposal.

1.2 Frontier Research

One of the most misunderstood concepts in ERC proposals is *frontier research*. The term does not refer to a particular discipline. Nor does it imply basic research in opposition to applied research. Instead, frontier research occupies the boundaries of current scientific knowledge. A frontier research project is characterised by one or more of the following features:

- → it addresses questions that have never been answered before;

- → it challenges existing theories;
- → it combines disciplines in an unconventional manner;
- → it develops entirely new methodologies;
- → it opens previously unexplored scientific directions.

Importantly, frontier research is not synonymous with interdisciplinarity. Many excellent ERC projects are entirely disciplinary. Conversely, interdisciplinary projects are not automatically frontier research. The defining feature is ambition.

Table 1 Incremental vs Frontier Research

Incremental Research	Frontier Research
Extends existing knowledge	Challenges existing knowledge
Improves current methods	Invents new methods
Predictable outcomes	Uncertain outcomes
Low scientific risk	High scientific risk
Solves known problems	Formulates new scientific questions
Confirms hypotheses	May overturn hypotheses

1.3 Excellence as the Sole Evaluation Criterion

Perhaps the most distinctive feature of the ERC is its exclusive focus on excellence.

Unlike many other funding programmes, the ERC does not evaluate:

- → geographical balance;
- → gender quotas;
- → industrial participation;
- → policy relevance;
- → expected economic impact;
- → stakeholder engagement.

These aspects may strengthen certain parts of a proposal, but they are never evaluated independently. The evaluation rests entirely on scientific excellence. However, excellence has two inseparable dimensions:

- → the excellence of the Principal Investigator;
- → the excellence of the proposed research.

Neither dimension alone is sufficient. An outstanding scientist proposing an ordinary project is unlikely to succeed. Similarly, an extraordinary idea presented by a PI who cannot convincingly demonstrate the ability to deliver it will face significant difficulties. The evaluation therefore focuses on the interaction between the project and the researcher.

1.4 The Role of the Principal Investigator

Perhaps the biggest conceptual difference between ERC proposals and collaborative Horizon Europe projects concerns the role of the Principal Investigator (PI). In collaborative projects, emphasis is placed on the consortium. In ERC proposals, the PI is the project. Everything revolves around demonstrating that this individual possesses:

- → scientific vision;
- → intellectual leadership;
- → independence;
- → creativity;
- → capacity to manage uncertainty;
- → ability to build a research team.

The proposal is therefore not only an evaluation of a scientific idea but also an evaluation of scientific leadership.

➔ Common Mistake

Many applicants write their proposal as if it were an anonymous scientific paper. ERC proposals should instead demonstrate why **you** are uniquely positioned to conduct this research. The panel must finish reading the proposal convinced that: *There is no better person to carry out this project.*

1.5 Why Excellent Proposals Are Rejected

One of the most surprising aspects of ERC evaluation is that many rejected proposals are scientifically excellent. Rejection does not necessarily indicate poor science. Instead, proposals often fail because they do not sufficiently convince evaluators in one or more of the following areas:

- → the scientific question is not ambitious enough;
- → the proposal appears incremental;

- → the novelty is hidden rather than explicit;
- → objectives are descriptive rather than transformative;
- → the methodology dominates the narrative;
- → the proposal resembles a conventional research grant;
- → the PI's leadership is not evident;
- → risks are either underestimated or not balanced by potential gains.

In many cases, science is sound, but the proposal does not communicate its transformative potential. This distinction is fundamental. ERC evaluators rarely ask: "Is this good science?" More often they ask: "Is this the kind of science that deserves ERC funding?"

1.6 Thinking Like an ERC Evaluator

Panel members typically begin forming an opinion within the first few pages of the proposal. Although they carefully read the full application, experienced evaluators unconsciously search for signals that indicate whether the proposal belongs to the small fraction of projects that deserve funding. Among the questions they continuously ask themselves are:

- → What is genuinely new here?
- → Why has nobody attempted this before?
- → What could fundamentally change if this project succeeds?
- → Why is this PI uniquely qualified?
- → Is the ambition proportional to the requested budget?
- → Does the proposal excite me intellectually?

Applicants often underestimate the importance of this last question. ERC panel members are scientists. Like all scientists, they are naturally attracted to ideas that surprise them. Curiosity remains one of the strongest predictors of a successful ERC proposal.

NCP'S TIP

A useful exercise before submitting your proposal is to ask a colleague to read only the first two pages. Then ask a single question: **"What do you think is the revolutionary idea?"** If the answer differs from what you intended, your proposal probably needs revision.

2. THE ERC EVALUATION PHILOSOPHY

2.1 Looking Beyond the Rules

Applicants often spend considerable time studying the ERC Work Programme, the Information for Applicants, and the proposal template. While these documents are essential, they do not fully explain how proposals are evaluated. Understanding the philosophy behind the evaluation process is just as important as understanding the formal criteria. The ERC does not attempt to identify the "best" project in an absolute sense. Such a goal would be impossible. Instead, the evaluation process seeks to answer a more practical question:

Among all proposals submitted to this panel, which ones are most likely to produce transformative scientific advances if funded?

This distinction is subtle but crucial. Panel members are not searching for perfection. They are searching for potential. Consequently, proposals are evaluated comparatively rather than absolutely. Every proposal is judged in relation to the other applications under consideration within the same panel. A proposal that would easily receive funding in many national programmes may still fail if competing against exceptionally ambitious ERC projects.

2.2 The Philosophy of Scientific Excellence

Unlike many research funding schemes, the ERC deliberately avoids prescribing what excellent science should look like.

There are no predefined thematic priorities. There are no mandatory methodologies. There are no preferred technologies. There are no required impacts beyond scientific advancement. This openness reflects an important philosophical assumption: Scientists themselves are best placed to identify the most promising research directions. The ERC therefore delegates scientific judgement almost entirely to the research community through peer review. For applicants, this has an important consequence. Your proposal should not attempt to demonstrate that your research aligns with European priorities. Instead, it should demonstrate that your research addresses an important scientific question whose answer could significantly advance knowledge.

NCP'S TIP

A surprisingly common mistake is to justify the project primarily through societal relevance. For example: "Cancer is one of the leading causes of death worldwide." Every evaluator already knows this. What they want to know is:

Why is your scientific question fundamentally different from all previous attempts? Scientific novelty—not societal importance—is the starting point of ERC evaluation.

2.3 The Two Dimensions of Excellence

The ERC evaluates two inseparable dimensions:

➔ 1. The Rese+arch Project

Evaluators ask:

- → Is the research question important?
- → Is it original?
- → Does it challenge existing paradigms?
- → Is it intellectually ambitious?
- → Is the methodology appropriate?
- → Are the risks balanced by potentially transformative gains?

➔ 2. The Principal Investigator

Evaluators also ask:

- → Has the PI demonstrated creativity?
- → Does the PI show scientific independence?
- → Has the PI already influenced the field?
- → Does the PI have the vision required to lead such a project?
- → Can the PI assemble and lead an outstanding research team?

The evaluation is therefore not additive. A proposal is not simply: Excellent project + Excellent CV = Fundable. Instead, panel members constantly ask:

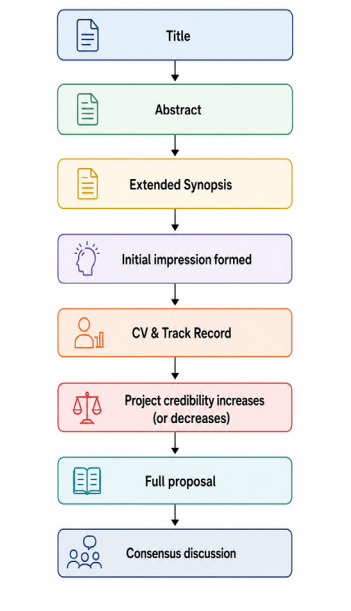
Is this the right person to conduct this particular research?

2.4 Why the First Pages Matter So Much

Although evaluators read the entire proposal, psychological research consistently shows that first impressions influence subsequent judgement. ERC evaluation is no exception. Experienced panel members often begin forming an initial opinion during the Extended Synopsis. This early impression influences how they interpret the remaining sections. If the first pages communicate ambition, clarity and originality, evaluators are more likely to approach the technical sections with curiosity. If

the proposal appears incremental or overly descriptive, the opposite may occur. This is one reason why the Extended Synopsis has become increasingly important in recent years. It does not merely summarise the project. It frames how the entire proposal will be interpreted.

Figure 1 – The Formation of an Evaluator's Opinion



2.5 How Evaluators Actually Read

Applicants often imagine that evaluators read proposals sequentially from beginning to end. In practice, experienced reviewers rarely do. A typical reading strategy may resemble the following:

1. Title
2. Abstract
3. Extended Synopsis
4. Scientific objectives
5. Figures
6. CV
7. Track Record
8. Budget
9. Risks
10. Remaining methodology

This behaviour reflects a natural cognitive process. Evaluators first attempt to understand the "big picture." Only after developing a conceptual framework do, they examine technical details. This observation has important implications. Never hide your most innovative idea on page two. Present it immediately.

2.6 Curiosity as an Evaluation Criterion

Although curiosity does not formally appear in the evaluation forms, it plays a surprisingly important role. Panel members are scientists. Scientists enjoy being intellectually surprised. Consequently, proposals that stimulate curiosity tend to receive more enthusiastic discussions during panel meetings. This does not imply writing sensational claims. Instead, applicants should create scientific curiosity. For example, instead of writing:

"We will analyse X."

One might write:

"Contrary to current assumptions, preliminary observations suggest that X may behave according to entirely different principles."

The second formulation immediately generates questions. Questions generate attention. Attention generates discussion. Discussion often determines success.

2.7 The Balance Between Confidence and Humility

Many applicants struggle to find the appropriate tone. Some become overly cautious. Others exaggerate. Neither approach is persuasive. Strong ERC proposals communicate confidence without arrogance. For example:

Weak: "This project will revolutionise medicine."

Better: "If successful, this project could fundamentally change our understanding of..."

The latter is ambitious but scientifically credible. Remember that evaluators are trained to recognise exaggerated claims. Credibility is one of the most valuable currencies in ERC evaluation.

2.8 High Risk Does Not Mean Poor Planning

Perhaps no concept is more frequently misunderstood than "high-risk/high-gain." Applicants often interpret high risk as methodological uncertainty. This is only partly correct. From an ERC perspective, risk primarily concerns the uncertainty associated with scientific discovery. For example: Low scientific risk:

Applying established technology to a new dataset.

High scientific risk: Testing whether an accepted biological mechanism is fundamentally incorrect.

The methodology may be excellent in both cases. The scientific uncertainty is radically different.

Table 2 – Types of Risk

Type of Risk	Important?	Comments
Technical risk	Moderate	Usually manageable
Methodological risk	Moderate	Should be mitigated
Scientific risk	Very High	Central to ERC philosophy
Financial risk	Low	Rarely decisive
Management risk	Low	Secondary consideration

2.9 Consensus Rather Than Voting

One of the least understood aspects of ERC evaluation concerns the role of panel discussions. Many applicants imagine a voting process. In reality, panel meetings are primarily based on scientific discussion leading to consensus. Each proposal is introduced by designated panel members, followed by discussion among the broader panel. This means that success depends not only on convincing individual reviewers but also on providing arguments that can be clearly communicated during panel deliberations. Imagine that your rapporteur has only three minutes to explain your proposal to colleagues. Would they be able to describe:

- → the central idea?
- → why it is original?
- → why it matters?
- → why you are the right person?

If not, your proposal may struggle during panel discussions.

2.10 The Psychology of Comparison

Applicants sometimes ask: "Is my proposal good enough?" This is the wrong question.

The correct question is: "Will my proposal be remembered after the panel has discussed twenty other proposals?" Memory plays an important role. Projects that introduce memorable concepts, striking hypotheses or elegant scientific questions often enjoy an advantage during final discussions. Originality is therefore not only scientifically valuable. It also enhances memorability.

NCP'S TIP

Before submission, summarise your project in exactly one sentence. If that sentence could also describe twenty other projects, your proposal probably lacks a distinctive identity.

2.11 What the New ERC Evaluation Criteria Mean in Practice

With the revised proposal structure introduced for the 2026 calls and expected to continue in 2027, evaluators place even greater emphasis on:

- → the clarity of the research question;
- → the importance of the knowledge gap;
- → the ambition of the objectives;
- → the credibility of the overall research strategy;
- → the intellectual leadership of the PI.

This means that applicants should spend less space describing routine methods and more space explaining why the scientific question matters and why answering it would significantly advance the field.

➔ Common Mistakes

- → Explaining how before explaining why.
- → Starting with societal impact instead of the scientific problem.
- → Presenting objectives without a central hypothesis.
- → Confusing complexity with ambition.
- → Using technical jargon to impress reviewers.
- → Hiding the novelty until later sections.
- → Writing a proposal that resembles a journal article rather than a vision for transformative research.

➔ Chapter Checklist

Before moving to the next chapter, ask yourself:

- → Does my proposal immediately communicate its scientific ambition?
- → Is the central research question obvious within the first page?

- → Have I explained why existing knowledge is insufficient?
- → Is the potential scientific gain proportional to the risks?
- → Would another scientist remember my proposal after reading twenty others?
- → Can my project be summarised in one compelling sentence?

➔ Key Message

The ERC evaluation process is not designed to reward proposals that are merely well written or technically flawless. It is designed to identify researchers with the vision, creativity and leadership to push the boundaries of knowledge. Writing a successful ERC proposal therefore requires more than scientific expertise. It requires understanding how evaluators think, how they compare proposals and how they identify the few projects capable of redefining a field.

3. UNDERSTANDING EXCELLENCE: HOW THE ERC EVALUATES THE RESEARCH PROJECT AND THE PRINCIPAL INVESTIGATOR

3.1 Excellence Is Not One Criterion

Every ERC call states that **scientific excellence is the sole evaluation criterion**. Applicants often interpret this sentence too literally. They imagine a single abstract concept called *excellence*. In reality, excellence has **two complementary dimensions**.

The ERC evaluates:

- → **the research project, and**
- → **the Principal Investigator.**

Neither dimension is sufficient on its own. A brilliant project cannot compensate for a proposal that does not convince the panel that the investigator can lead it. Likewise, an exceptional CV cannot compensate for a project that lacks originality or transformative ambition. The ERC evaluates **the combination**. The central question is therefore not: *Is the project excellent?* Or *Is the Principal Investigator excellent?*

It is*: Are this investigator and this project the right combination to produce a major scientific advance?*

3.2 Excellence of the Research Project

Under the revised evaluation framework, the project is assessed primarily through its scientific ambition and its potential to generate significant advances in knowledge. Panels look for research that is genuinely ground-breaking, intellectually ambitious and capable of opening new directions rather than extending existing ones. This means that evaluators are implicitly asking questions such as:

- → **Does this project address an important scientific problem?**
- → **Does it challenge current paradigms?**
- → **Is the central idea genuinely original?**
- → **Will success change the way researchers think?**
- → **Is the level of am**bition appropriate for frontier research?

Notice what is not on this list. The panel is not primarily asking whether the methods are sophisticated. Nor whether the project is technically impressive. Nor whether every experiment is guaranteed to succeed. Those aspects matter. But they matter because they support the scientific idea. The idea remains at the centre of the evaluation. Evaluator's Perspective: *"After reading the proposal, do I feel that science would genuinely move forward if this project succeeded?"*

3.3 Excellence of the Principal Investigator

The second dimension concerns the Principal Investigator. Here again, applicants often misunderstand the evaluation. The ERC is not searching for the longest publication list. Nor for the largest laboratory. Nor for the highest h-index. The panel is asking a more sophisticated question: *Has this researcher demonstrated the intellectual capacity to conceive and lead this frontier research?* The answer depends on career stage. For a Starting Grant applicant, excellence may be demonstrated through outstanding scientific potential, intellectual independence and the emergence of an original research vision. For a Consolidator Grant applicant, the emphasis shifts towards the consolidation of an independent research programme and evidence of growing scientific leadership. For an Advanced Grant applicant, excellence is reflected in sustained scientific influence and an outstanding record of transformative contributions. Different career stages. The same principle. Scientific excellence must always be interpreted in context.

3.4 The Interaction Between the Two Dimensions

Perhaps the most important consequence of the new evaluation framework is that the two dimensions should never be considered separately. An ambitious project appears more credible when proposed by a researcher whose career demonstrates the capacity to deliver it. Conversely, an exceptional investigator appears even stronger when presenting a project that genuinely requires their unique expertise and vision. The project and the Principal Investigator reinforce each other. This interaction is where many outstanding proposals distinguish themselves. The evaluator gradually reaches a simple conclusion: *I cannot imagine a better person to carry out this project.* That is the real objective of the proposal.

3.5 Excellence Is Relative to Career Stage

One of the most common misconceptions among applicants is the belief that they must already resemble an Advanced Grant holder to be competitive. Nothing could be further from the ERC philosophy. Panels evaluate excellence relative to opportunity and career stage, not against a universal academic standard. A researcher three years after the PhD is not expected to demonstrate the same achievements as someone twenty years after the PhD. Instead, evaluators ask:

Given where this researcher is today, have they already demonstrated exceptional scientific promise and the ability to reach the next level?

This principle is particularly important under the new eligibility windows, where applicants may choose between different grant schemes. The evaluation remains contextual, not absolute.

3.6 The Excellence table

One useful way to think about the evaluation is to imagine a simple table.

Research Project	Principal Investigator	Likely Impression
Outstanding	Outstanding	Highly competitive proposal
Outstanding	Moderate	Brilliant idea, uncertain leadership
Moderate	Outstanding	Excellent scientist, insufficient scientific ambition
Moderate	Moderate	Unlikely to be competitive

The purpose of the proposal is therefore not to maximise one dimension. It is to maximise both simultaneously.

➔ Case Study

Applicant:*"Should I spend more time improving my CV or improving my proposal?"*

➔ Discussion

This is the wrong dilemma. If strengthening one dimension leaves the other unchanged, the proposal remains unbalanced. The strongest ERC applications are those in which the scientific vision and the investigator's profile become mutually reinforcing. The evaluator should finish reading with one impression:

This is an exceptional idea, and this is exactly the researcher who should pursue it.

➔ Common Mistakes

- → **Treating the CV as an appendix rather than part of the scientific argument.**
- → **Assuming that publication metrics alone demonstrate excellence.**
- → **Equating technical complexity with scientific ambition.**
- → **Writing the proposal and the CV as independent documents.**
- → **Forgetting that excellence is always interpreted in relation to** career stage.

3.7 What about feasibility?

One of the most significant practical consequences of the revised ERC evaluation process concerns feasibility. Under the previous evaluation system, applicants often felt compelled to demonstrate, already in the Extended Synopsis (Part B1), that every aspect of the project was technically feasible. As a result, many summaries devoted valuable space to methodological details, preliminary results and risk mitigation strategies. The revised evaluation framework changes this balance. At Step 1, the panel's primary task is to judge the scientific excellence of the research idea together with the excellence of the Principal Investigator. The first question is no longer *"Can this project be carried out?"* but rather:

➔ "Is this a frontier research idea that deserves to remain in the competition?"

This does not mean that feasibility has disappeared from the evaluation. Far from it. Rather, feasibility is now assessed at the stage where evaluators have access to the information needed to judge it properly: the full scientific proposal (Part B2). Consequently, the purpose of Part B1 is not to prove that every methodological challenge has already been solved. Its purpose is to convince evaluators that the scientific question is so important and so original that it deserves a full evaluation. Feasibility has not become less important—it has become differently important. Applicants should not remove feasibility from their B1; they should move the centre of gravity of the feasibility discussion from B1 to B2, while ensuring that B1 still conveys scientific credibility without becoming methodologically overloaded. That distinction is likely to help many applicants avoid one of the most common mistakes under the new rules.

➔ What Should Be Included in B1?

This raises an obvious question. Should applicants completely avoid discussing feasibility in Part B1? The answer is no. An excellent B1 should still convince evaluators that the project is credible. If the proposed research appears physically impossible, conceptually inconsistent or completely detached from existing scientific knowledge, the proposal will obviously not be competitive. However, credibility is different from detailed feasibility. The Extended Synopsis should reassure the panel that the investigator has a convincing scientific strategy, but it should avoid becoming a miniature methodology section. Methodological details, work packages, alternative strategies, technical risks and extensive discussions of preliminary data belong naturally in Part B2, where they can be presented with the necessary depth and context.

➔ A Useful Rule of Thumb

A simple way to think about the distinction is this:

In Part B1, explain *why* the project should be funded.

In Part B2, explain *how* it will be accomplished.

The two documents are complementary. Part B1 creates scientific excitement. Part B2 transforms that excitement into confidence. Applicants who try to answer every feasibility question in the first five pages often sacrifice the very element that the Step 1 evaluation is designed to identify: a bold and transformative scientific vision.

➔ Evaluator's Perspective

"When I read Part B1, I am looking for a compelling scientific opportunity—not for a complete technical manual. I need to believe that the idea is worth pursuing. I will judge in detail whether it is feasible when I read Part B2."

Case Study

➔ Applicant

"Should I include preliminary data and methodological details in my Extended Synopsis to show that the project is feasible?"

➔ Discussion

Include them only if they strengthen the scientific argument. A single sentence showing that a critical proof of principle already exists can be extremely valuable. Three paragraphs describing protocols, experimental procedures or statistical models usually are not. Remember that every paragraph devoted to technical details is a paragraph not devoted to explaining why your idea could change the field. The best B1 leaves the panel wanting to read the B2.

➔ Key Takeaways

1. Excellence remains the sole evaluation criterion, but it is assessed through two inseparable dimensions: the research project and the Principal Investigator.
2. The project is evaluated for its ground-breaking nature, ambition and potential to generate major advances in knowledge, not simply for methodological sophistication.
3. The Principal Investigator is evaluated for the ability to conceive, lead and deliver the proposed frontier research, with expectations calibrated to the relevant career stage.
4. The strongest proposals are those in which the project and the investigator strengthen one another, creating a compelling and coherent scientific narrative.
5. The goal is not simply to demonstrate excellence in two separate dimensions, but to convince the panel that this investigator and this project together represent an exceptional opportunity for the ERC to invest in frontier research.

4. CHOOSING THE RIGHT ERC PANEL

4.1 Why Panel Choice Matters

The best proposal submitted to the wrong panel is often a rejected proposal. One of the earliest decisions in preparing an ERC proposal is also one of the least reversible: selecting the evaluation panel. Applicants sometimes assume that panel choice is an administrative step with little influence on the outcome. In reality, the selected panel determines:

- → the scientific culture of the reviewers;
- → the conceptual framework used to evaluate originality;
- → expectations regarding methodology;
- → perceptions of risk;
- → standards for evidence and feasibility.

The same proposal may be considered visionary by one panel and insufficiently focused by another. Choosing the panel is therefore not about finding the "easiest" panel. It is about finding the community that is most likely to recognise the importance of your scientific question.

4.2 Understanding the ERC Panel Structure

The ERC organises proposals into three scientific domains:

- → LS – Life Sciences
- → PE – Physical Sciences and Engineering
- → SH – Social Sciences and Humanities

Each domain is divided into specialised panels. Although panel descriptions appear relatively broad, each panel develops its own scientific culture over time. Understanding this culture is often more important than reading the official descriptors.

4.3 A Panel Is Not a Journal

Applicants frequently choose a panel by asking: "Where would I publish this research?" This is usually the wrong approach. Scientific journals evaluate completed research. ERC panels evaluate future scientific potential. Instead, ask:

Who will immediately appreciate why this scientific question matters?
These are not always the same people.

➔ Example

Imagine a project combining:

- → epidemiology
- → machine learning
- → nutritional sciences
- → causal inference

Possible panel choices might include:

- → LS2
- → LS7
- → PE6

The correct choice depends not on the techniques employed but on the scientific question. The same methodology supports different scientific narratives. Panels evaluate the narrative.

4.4 Scientific Question First, Methods Second

A common mistake is choosing a panel according to the dominant methodology.

Suppose your proposal uses:

- → artificial intelligence
- → Bayesian statistics
- → mathematical modelling

This does not automatically imply a PE panel. The decisive question remains:

➔ What scientific problem is being solved?

Methods are instruments. Panels evaluate scientific questions.

NCP'S TIP

Never describe your proposal as:

"This is an AI project."

Instead ask: "What biological, physical or social question does AI allow me to answer?"

That answer determines the panel.

4.5 Interdisciplinary Proposals

The ERC strongly encourages interdisciplinary research. However, interdisciplinary proposals are not evaluated by interdisciplinary committees. Every proposal belongs primarily to one panel. This has several consequences. First, reviewers naturally interpret interdisciplinarity through the perspective of their own discipline. Second, interdisciplinary proposals must satisfy two apparently contradictory requirements:

- → be understandable to specialists from one field;
- → remain scientifically credible to experts from neighbouring disciplines.

Finding this balance is one of the greatest challenges in ERC writing.

4.6 The “Home Panel” Principle

In our experience, applicants should ask: Where is my scientific home? Not: Where is my favourite technique? Not: Where is my publication record? Not: Which panel has higher success rates? Scientific identity matters. Panel members should immediately recognise the scientific problem as belonging to their intellectual community.

4.7 Reading Between the Lines of Panel Descriptors

Official descriptors are intentionally broad. For example: Prevention may include

- → epidemiology
- → behavioural sciences
- → public health
- → biomarkers
- → digital health
- → nutrition
- → environmental exposure

Two proposals can therefore fit the same descriptor while requiring completely different evaluation perspectives. Applicants should use descriptors only as an initial guide.

4.8 Looking at Previously Funded Projects

One useful exercise is to examine funded ERC projects. Do not ask: "Does my project resemble these?" Instead ask: What kinds of scientific questions are celebrated in this panel? Patterns often emerge. Some panels favour conceptual breakthroughs. Others appreciate methodological innovation. Others reward technological disruption. Understanding these patterns helps shape the proposal narrative.

➔ **Common Mistake**

Applicants sometimes imitate previously funded projects. This is counterproductive. Panels do not seek repetition. They seek the next breakthrough.

4.9 Should You Choose a Less Competitive Panel?

This question is asked every year. The answer is always **No.** Competition exists within every panel. Submitting to a panel less familiar with your field rarely creates an advantage. Instead, it increases the probability that reviewers underestimate the importance of your question. Scientific fit is far more important than perceived competitiveness.

4.10 Panel Members Are Generalists Within Their Discipline

Another misconception concerns reviewer expertise. Applicants often imagine that one reviewer will be an international authority precisely on their niche topic. In reality, this is uncommon. Panel members are experts. They are not necessarily experts on your exact research question. Consequently, proposals should explain concepts that would be obvious only to a very small scientific community.

NCP'S TIP

If understanding your proposal requires reading three review articles beforehand, your proposal is too specialised.

4.11 The Role of External Reviewers

Depending on the evaluation stage and panel, proposals may also be assessed by external experts. These reviewers provide specialised scientific opinions. However, funding decisions remain the responsibility of the panel. Therefore:

- → External reviewers evaluate details.
- → Panel members evaluate the overall scientific vision.

Applicants often overestimate the influence of external reviews. Ultimately, the panel must be convinced.

4.12 The Importance of the Extended Synopsis

The Extended Synopsis plays an even greater role for interdisciplinary proposals. Panel members may have limited familiarity with parts of the project. The synopsis should therefore:

- → establish the scientific problem;
- → explain why existing approaches fail;
- → introduce the new conceptual framework;
- → convince non-specialists before technical details appear.

Think of the synopsis as a translation between disciplines.

4.13 Choosing Secondary Review Panels

Some ERC calls allow applicants to indicate additional descriptors or secondary expertise. These should never contradict the primary scientific identity of the proposal. Secondary expertise should enrich evaluation. It should not redefine the project. If your proposal could honestly belong equally to three different panels, the project narrative may still require refinement.

➔ Common Mistakes

- → Choosing the panel according to methodology.
- → Choosing the panel according to publication history.
- → Choosing a panel because of perceived success rates.
- → Trying to appeal equally to three different scientific communities.
- → Assuming panel members know your specialised terminology.
- → Believing interdisciplinarity alone strengthens the proposal.

NCP'S TIPS

- → Choose the panel that immediately understands why your scientific question matters.
- → Scientific identity matters more than methodology.
- → Panels evaluate ideas, not technologies.
- → Explain your discipline before explaining your methods.
- → Write for highly intelligent scientists outside your niche.

➔ Final Checklist

Before submitting, ask yourself:

- → Would researchers from this panel immediately recognise my scientific question as important?
- → Does the proposal speak the language of this panel?
- → Have I avoided unnecessary jargon?
- → Does interdisciplinarity strengthen rather than confuse the proposal?
- → Could a panel member explain my project to colleagues in three minutes?

If every answer is "yes", you have probably chosen the appropriate panel.

➔ Key Message

The ERC does not fund projects because they use fashionable methods or address popular topics. It funds projects that ask outstanding scientific questions. Your panel should therefore be the community that instinctively recognises the importance of those questions.

5. DEVELOPING AN ERC IDEA

5.1 Where ERC Projects Really Begin

Most research projects begin with an opportunity. A new technology becomes available. A dataset is collected. A collaboration is established. A funding opportunity appears. ERC projects rarely begin this way. Instead, they usually begin with an intellectual discomfort:

"Something about our current understanding is incomplete."

The best ERC ideas emerge when researchers identify a fundamental limitation in current knowledge and ask a question that the field has either overlooked or considered impossible to answer. This distinction is subtle but profound. Many excellent proposals explain *what* they intend to study. Outstanding proposals explain *why the field has been asking the wrong question*.

5.2 Good Science Is Not Enough

One of the most common misconceptions is that an ERC proposal simply requires excellent science. Of course, scientific excellence is indispensable. However, many proposals rejected by the ERC are scientifically rigorous, methodologically sophisticated, and technically feasible. What often distinguishes funded proposals is not the quality of the science alone, but the quality of the scientific vision. Consider the difference between these two statements.

➔ Version A

We will investigate how environmental exposure affects immune function during early life.

➔ Version B

Current models assume that environmental exposures act independently during development. We hypothesise that this assumption is fundamentally flawed and that immune programming emerges from cumulative exposure networks that have never been systematically characterised.

Both statements introduce an interesting research topic. The second introduces a new way of thinking. That is the difference between a good proposal and a potentially transformative one.

5.3 What Makes an Idea "ERC"

Applicants often ask: "Is my project suitable for the ERC?" Unfortunately, there is no checklist that can answer this question. However, most successful ERC ideas share several characteristics. They seek to answer questions that are:

- → scientifically fundamental rather than merely practical;
- → conceptually ambitious rather than technically complex;
- → transformative rather than incremental;
- → uncertain in outcome but potentially revolutionary in consequence.

The defining characteristic is not risk itself. It is the possibility that success would substantially alter current scientific understanding.

Table 3 – Research Ideas Along the ERC Spectrum

Characteristic	Conventional Project	Competitive ERC Project
Main objective	Generate new knowledge	Redefine existing knowledge
Scientific contribution	Incremental	Transformative
Primary innovation	New application	New concept or paradigm
Research risk	Moderate	High but justified
Expected outcome	Better understanding	New understanding

5.4 The Knowledge Gap Is Not Enough

Many proposals begin by identifying a gap in literature. For example: "Few studies have investigated..." This is rarely persuasive. A lack of studies does not necessarily indicate an important scientific problem. Sometimes, a topic receives little attention because it is not particularly significant. Instead, applicants should identify a knowledge limitation. Compare the following examples.

➔ **Weak**

Few studies have examined X in adolescents.

➔ **Better**

Existing models cannot explain why phenomenon X occurs during adolescence despite consistent evidence across multiple populations.

The second statement identifies a genuine scientific limitation rather than a simple absence of publications.

➔ **Evaluator's Perspective**

"I am not interested in knowing that something has not been studied. I want to understand why studying it could change our understanding of the field."

5.5 From Gap to Scientific Tension

Every compelling ERC proposal contains scientific tension. Scientific tension arises when observations challenge existing theories, when competing explanations coexist, or when accepted assumptions fail to account for empirical evidence. This tension creates the intellectual momentum that drives the proposal. Without tension, there is little reason to invest substantial resources in addressing the question. For example:

- → conflicting experimental findings;
- → unexplained biological mechanisms;
- → contradictory theoretical models;
- → observations that current paradigms cannot reconcile.

Scientific tension is often more persuasive than scientific novelty alone.

5.6 Asking Better Questions

The quality of a proposal depends heavily on the quality of its central research question. Compare the following.

➔ **Question 1**

How do dietary factors influence cardiovascular health?

This question is broad but familiar.

➔ Question 2

Why do individuals with apparently similar dietary exposures develop radically different cardiovascular trajectories despite comparable genetic and environmental backgrounds?

The second question immediately generates curiosity. It suggests that current explanations may be insufficient. It invites discovery.

➔ Good Practice

A strong ERC question often has three characteristics:

1. It challenges an implicit assumption.
2. It cannot be answered through routine experimentation.
3. Its answer would have broad scientific implications.

5.7 From Question to Hypothesis

Applicants sometimes avoid proposing bold hypotheses because they fear being wrong. Paradoxically, the absence of a clear hypothesis often weakens a proposal more than an incorrect hypothesis. A hypothesis provides intellectual direction. It tells evaluators what conceptual advance the project seeks to test. For example:

➔ Weak objective

To investigate the relationship between X and Y.

➔ Stronger hypothesis

We hypothesise that X is not an independent determinant of Y but part of a previously unrecognised regulatory network that fundamentally reshapes current models of disease progression.

The hypothesis need not be correct. It must be scientifically meaningful and testable.

5.8 The Difference Between Complexity and Ambition

Applicants frequently equate ambitious research with technically demanding research. The two are not synonymous. A

project involving dozens of sophisticated techniques may still address a relatively modest scientific question. Conversely, a conceptually ambitious project may employ straightforward methodologies. ERC evaluators reward intellectual ambition rather than methodological complexity. The most memorable proposals often ask deceptively simple questions whose answers have profound implications.

➔ **Common Mistake**

Adding more techniques does not automatically increase ambition. A proposal should become more ambitious by asking deeper scientific questions, not by accumulating methods.

5.9 Defining Transformative Objectives

Objectives should describe the scientific transformation the project seeks to achieve. Compare these formulations.

➔ **Weak**

Objective 1: Develop a new computational pipeline.

➔ **Better**

Objective 1: Establish a new conceptual framework for understanding dynamic exposure interactions by developing computational models capable of revealing mechanisms that remain invisible using current analytical approaches.

The method supports the objective. It is not the objective itself.

5.10 The “Why Now?” Question

Every ERC proposal should implicitly answer another important question. Why is this research possible today? Several factors may contribute:

- → recent technological advances;
- → availability of unique datasets;
- → new theoretical developments;
- → emerging interdisciplinary opportunities;
- → unprecedented computational capabilities.

Explaining why the project could not realistically have been undertaken ten years ago increases its credibility.

Evaluator's Perspective: *"I want to understand why this project belongs to the present moment rather than any other point in scientific history."*

5.11 The Transformative Potential Test

A useful exercise is to imagine that the project succeeds completely. Then ask: **What changes?** If the answer is: We will know more. The ambition is probably insufficient. If the answer is: We will need to rethink how the field explains this phenomenon. The project may possess genuine transformative potential.

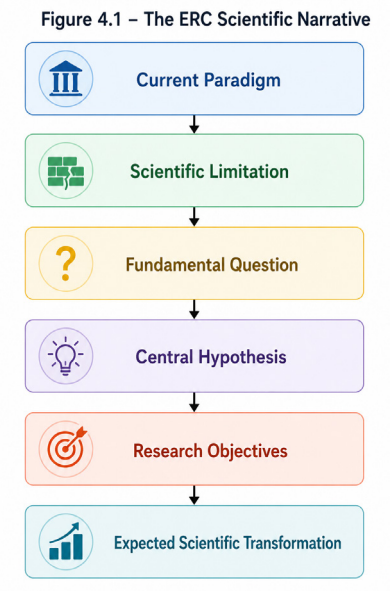
5.12 Building a Coherent Scientific Narrative

An ERC proposal should tell a coherent scientific story. Every section should reinforce the same narrative. The story might follow this structure:

1. Current understanding is incomplete.
2. Existing assumptions fail to explain important observations.
3. A new conceptual approach is needed.
4. The proposed research tests this new framework.
5. Success would redefine the field.

Every objective, figure, and methodological choice should contribute to this narrative.

Figure 2 – The ERC Scientific Narrative



5.13 Originality Beyond Novelty

Novelty alone is rarely sufficient. Applicants often describe their work as "novel" because they apply an existing method to a new context. Originality, however, concerns the generation of new scientific understanding. Consider three levels of originality:

Level	Description
Level 1	New data or materials
Level 2	New methods or technologies
Level 3	New concepts or theoretical frameworks

While all three can contribute to a successful proposal, projects that combine conceptual originality with methodological innovation often leave the strongest impression.

5.14 A Self-Assessment Exercise

Before writing the proposal, answer the following questions in a single sentence each:

- → What assumption does my project challenge?
- → What scientific mystery motivates the research?
- → What would surprise the scientific community if my hypothesis proved correct?
- → Why has nobody solved this problem before?
- → Why am I uniquely positioned to address it?
- → What will researchers think differently if this project succeeds?

If any answer is unclear, the underlying idea may still require refinement.

➔ Common Mistakes

- → Starting from methods instead of scientific questions.
- → Confusing a literature gap with a scientific problem.
- → Defining objectives as technical tasks rather than conceptual advances.
- → Presenting novelty without explaining its significance.

- → Overloading the proposal with techniques while neglecting the central idea.
- → Failing to articulate a clear hypothesis.
- → Underestimating the importance of scientific tension.

NCP'S TIPS

- → Start with the scientific problem, not the technology.
- → Challenge assumptions rather than simply extending existing knowledge.
- → Make the scientific question memorable.
- → Explain why existing theories are insufficient.
- → Ensure that every objective contributes to a single transformative vision.

➔ Questions to Ask Yourself

Before proceeding to the next chapter, ask:

- → Can I summarise my project's central idea in one compelling sentence?
- → Have I identified a genuine scientific limitation rather than a simple gap in literature?
- → Does my proposal challenge current understanding?
- → Would success fundamentally change how researchers think about this problem?
- → Is the methodology clearly serving the scientific vision rather than driving it?

➔ Key Message

The strongest ERC proposals do not begin with an experiment or a technology. They begin with an idea powerful enough to convince evaluators that answering a single scientific question could alter the trajectory of an entire field.

6. WRITING PART B1 – THE EXTENDED SYNOPSIS

If there is one section of the proposal that every ERC applicant should rewrite ten times, it is the Extended Synopsis. The Extended Synopsis is arguably the most influential section of the entire proposal. It is often the first substantial scientific text evaluators read, and it frames how they interpret everything that follows. In the next chapter, we will examine the Extended Synopsis paragraph by paragraph, discussing how to introduce the research problem, present the scientific vision, communicate ambition, structure objectives, and maintain a compelling narrative from the first sentence to the final paragraph. We will also analyse examples of effective and ineffective writing, showing how subtle differences in wording can influence an evaluator's perception of originality, credibility, and leadership.

6.1 Why the Extended Synopsis Matters

The Extended Synopsis is the intellectual gateway to the entire proposal. Although it represents only a limited number of pages, it performs an extraordinary number of functions simultaneously. It introduces the scientific problem, presents the conceptual vision, establishes the novelty of the research, demonstrates the applicant's intellectual leadership, and persuades evaluators that the proposed work deserves the resources requested. Many applicants think of the Extended Synopsis as a summary of the project. This is a mistake. It is not a summary. It is the proposal in miniature. Every important idea that appears later in Part B2 should already be visible here, albeit at a higher conceptual level. An evaluator should finish reading the synopsis with a clear understanding of three things:

- → the scientific problem;
- → the transformative idea;
- → why the Principal Investigator is uniquely positioned to pursue it.

If these three elements are not clear, no amount of technical detail in Part B2 will fully compensate.

6.2 The Purpose of the Synopsis

Many applicants write the synopsis as though they were preparing the introduction to a scientific article. An ERC synopsis has a completely different purpose. A scientific paper explains discoveries that have already been made. An ERC proposal argues for discoveries that have not yet happened. Consequently, the writing style must be forward-looking. The synopsis should constantly answer one implicit question: **Why should the ERC invest several million euros in this idea?**

Not: What have I done?

Not: What techniques will I use?

But: **What scientific future becomes possible because of this project?**

6.3 The Four Questions Every Synopsis Must Answer

Although the official template does not explicitly organise the synopsis around specific questions, almost every successful proposal answers four fundamental questions.

➔ Question 1

What is the scientific problem?

Not the application.

Not the technology.

The scientific problem.

➔ Question 2

Why is current knowledge insufficient?

What assumptions fail?

What remains unexplained?

Where does existing theory break down?

➔ Question 3

What fundamentally new idea does this project introduce?

Notice that this question concerns ideas—not methods.

➔ Question 4

Why is this project transformative?

How would success change the field?

These four questions should become the invisible architecture of the synopsis.

6.4 The First Paragraph

The first paragraph may be the most important paragraph in the entire proposal. Its purpose is not to review the literature. Its purpose is to create scientific curiosity. Compare these two openings.

➔ Weak Opening

Cardiovascular disease remains one of the leading causes of mortality worldwide. Numerous studies have investigated...

This introduction could belong to thousands of proposals. It communicates importance but not originality.

➔ Stronger Opening

Despite decades of research, we still cannot explain why individuals with apparently similar cardiovascular risk profiles experience dramatically different disease trajectories. This unresolved paradox suggests that current models overlook fundamental mechanisms governing disease development.

Notice the difference. The second paragraph immediately creates an intellectual puzzle. Evaluators naturally want to know the answer. Curiosity is established.

➔ Evaluator's Perspective

"I do not need to be convinced that cardiovascular disease is important. I already know that. I need to understand why your scientific question is fascinating."

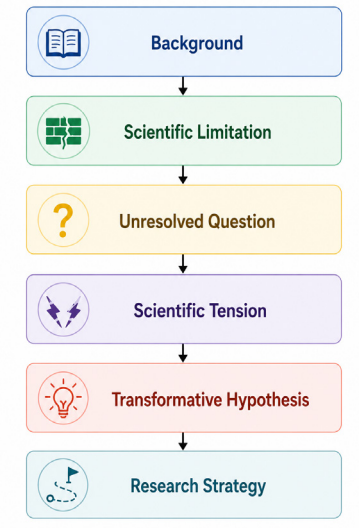
6.5 Building Scientific Tension

Every outstanding synopsis contains tension. Without tension there is no narrative. Without narrative there is no momentum. Scientific tension emerges when:

- → accepted theories fail;
- → contradictory evidence accumulates;
- → unexplained observations persist;
- → current methodologies reach their limits.

Rather than simply presenting background information, the synopsis should gradually increase this tension. Only then should the proposal introduce its central hypothesis.

Figure 3 – Narrative Tension



6.6 Presenting the Central Hypothesis

Many applicants hide their hypothesis until later sections. This is rarely effective. The evaluator wants to understand the conceptual leap as early as possible. A strong hypothesis should be:

- → concise;
- → intellectually bold;
- → scientifically plausible;
- → easy to remember.

For example: "*We hypothesise that immune development is not determined by individual environmental exposures but by dynamic exposure networks that current epidemiological models cannot capture*".

Notice that the hypothesis immediately challenges an existing paradigm.

6.7 Introducing the Scientific Vision

One of the most common weaknesses of ERC proposals is that they explain objectives before presenting the overall vision. Instead, evaluators should first understand:

What future understanding does this project seek to create?

Only afterwards should objectives explain how that vision will be achieved. Vision precedes implementation.

6.8 Objectives Are Not Work Packages

Objectives describe scientific transformation. Work Packages describe project management. Applicants frequently confuse the two. Compare the following.

➔ Weak Objective

Develop software for analysing exposure mixtures.

➔ Better Objective

Develop a conceptual framework capable of identifying exposure interactions that remain invisible under current epidemiological paradigms.

The software becomes one possible instrument. The objective remains scientific.

➔ Common Mistake

Objectives beginning with:

- → collect;
- → measure;
- → develop;
- → optimise;
- → validate;

often describe activities rather than discoveries. Whenever possible, connect each objective to the scientific understanding it enables.

6.9 Communicating Ambition Without Exaggeration

Applicants often fear that ambitious writing will appear unrealistic. Others compensate by making exaggerated claims. Neither strategy is ideal. Compare the following.

➔ Too Weak

This project may improve our understanding.

➔ Too Strong

This project will completely revolutionise medicine.

➔ **Better**

If successful, this project could fundamentally reshape current models of disease initiation by introducing an entirely new conceptual framework for understanding exposure dynamics.

The wording is ambitious but conditional. It inspires confidence rather than scepticism.

6.10 Writing for Non-Specialists

Remember that panel members are experts. They are not necessarily experts in your exact subfield. Consequently, the synopsis should minimise unnecessary jargon. A useful rule is this: Every specialised concept should either:

- → already be widely known;
- → or be explained in one sentence.

If understanding a paragraph requires extensive background knowledge, rewrite it.

➔ **Evaluator's Perspective**

"I am perfectly willing to learn something new while reading your proposal. I am not willing to decode unnecessary technical language."

6.11 The Role of Figures

Figures in the synopsis should simplify thinking. Not decorate pages. Every figure should answer one of three questions.

Option 1

What is currently believed?

Option 2

What knowledge gap exists?

Option 3

How are the scientific objectives logically connected?

Avoid figures that merely reproduce experimental workflows. Conceptual figures are generally far more valuable than methodological diagrams in the synopsis.

6.12 Paragraph Flow

Each paragraph should answer one question while naturally creating the next. For example:

Paragraph 1

What is the scientific puzzle?

↓

Paragraph 2

Why has nobody solved it?

↓

Paragraph 3

What new hypothesis explains it?

↓

Paragraph 4

How will we test it?

↓

Paragraph 5

What changes if we are correct?

This creates narrative continuity.

6.13 The Last Paragraph

Many applicants finish with administration. Timeline. Resources. Management. Instead, the synopsis should end with science. The final paragraph should remind evaluators:

- → what is at stake;
- → why success matters;
- → why this project deserves ERC support.

The final sentence should leave evaluators thinking about the scientific future rather than project logistics.

Good Example vs Weak Example

➔ Weak

The project combines epidemiology, machine learning and biomarkers to investigate environmental determinants of disease.

➔ Stronger

By integrating epidemiology, machine learning and molecular biomarkers within a unified conceptual framework, the project seeks to determine whether disease risk emerges from dynamic exposure networks rather than independent environmental factors. If confirmed, this would require a fundamental revision of current prevention models.

Notice that the methods become secondary. The conceptual advance dominates.

➔ The Hidden Questions

Throughout the synopsis, evaluators continuously ask themselves questions that never appear in the template.

Have you answered them?

- → Why does this problem matter scientifically?
- → Why now?
- → Why has nobody solved it before?
- → Why ERC?
- → Why this PI?
- → Why should I believe this can work?
- → Why is the requested budget justified?
- → What would surprise the scientific community?

The strongest synopses answer these questions naturally, without ever stating them explicitly.

➔ Common Mistakes

- → Beginning with societal impact instead of the scientific question.
- → Spending too much space reviewing the literature.
- → Introducing novelty only halfway through the synopsis.
- → Listing objectives without a unifying hypothesis.

- → Describing methods before explaining scientific vision.
- → Using figures that illustrate workflows rather than ideas.
- → Ending with project management instead of scientific transformation.

NCP'S TIPS

- → Make the first paragraph memorable.
- → Introduce the scientific puzzle immediately.
- → Present the central hypothesis early.
- → Keep methods subordinate to ideas.
- → End with the scientific transformation you seek to achieve.
- → Ensure that every paragraph increases curiosity.

➔ Questions to Ask Yourself

Before submitting your synopsis, ask:

- → Can someone explain my project after reading only these pages?
- → Is my central hypothesis immediately visible?
- → Does every objective contribute to one overarching vision?
- → Would the synopsis still be compelling if all methodological details were removed?
- → What is the single idea evaluators are most likely to remember?

➔ Key Message

The Extended Synopsis is not a condensed version of Part B2. It is the intellectual foundation upon which the entire proposal rests. A successful synopsis does not attempt to describe everything. Instead, it persuades evaluators that one scientific idea is sufficiently original, ambitious and credible to justify an ERC grant.

7. BUILDING SCIENTIFIC LEADERSHIP

Writing the CV and Track Record

"The ERC does not fund CVs. It funds researchers capable of changing the direction of their field."

7.1 The Most Misunderstood Part of the Proposal

Many applicants devote almost all of their energy to writing the research proposal. The CV is often prepared during the last few days before submission. This is understandable. Researchers naturally feel more comfortable describing their science than describing themselves. Yet experienced evaluators know that this is a mistake. An ERC proposal is evaluated through two inseparable lenses. The first asks:

➔ **Is this an extraordinary scientific idea?**

The second immediately follows:

➔ **Is this the person who can actually make it happen?**

Both questions carry equal importance. A brilliant proposal cannot fully compensate for an unconvincing Principal Investigator profile. Likewise, an outstanding CV cannot rescue an unambitious project. ERC funding exists at the intersection of scientific vision and scientific leadership.

7.2 Stop Thinking About Your CV

One of the biggest conceptual shifts applicants need to make is this: **You are not writing a curriculum vitae.** You are constructing an argument. Every publication, every invited lecture, every grant, every supervision experience should contribute to answering one question: **Why should the ERC trust me with this project?**

That question should shape every decision. Do not ask: What have I done? Instead ask: What evidence demonstrates that I am ready to lead this research? This distinction changes everything.

7.3 The Evaluator Is Looking for a Story

Applicants often present achievements as disconnected facts.

- → Twenty publications.
- → Three grants.
- → Seven invited talks.
- → One patent.

Excellent. But what does it mean? Evaluators rarely remember lists. They remember trajectories. The strongest CVs tell a coherent scientific story. Perhaps they show the gradual development of a new research programme. Perhaps they demonstrate increasing independence. Perhaps they reveal a researcher repeatedly opening new directions rather than following established ones. Numbers matter. Narratives matter more.

➔ Evaluator's Perspective

"When I finish reading this CV, can I describe this researcher in one sentence?"

If the answer is no, the profile lacks a clear scientific identity.

7.4 Scientific Independence

Few concepts generate more anxiety than scientific independence. Applicants often ask:

"How many first-author papers do I need?"

"How many papers without my PhD supervisor?"

These questions are understandable. Unfortunately, they are also incomplete.

Independence is not measured by a single metric. It is inferred from a pattern of evidence. The panel asks whether you have developed your own scientific identity. Evidence may include:

- → establishing new research questions;
- → developing original methodologies;
- → supervising students or postdoctoral researchers;
- → obtaining competitive funding;
- → leading collaborations;
- → delivering invited keynote lectures;
- → coordinating research activities;
- → publishing work that clearly reflects your own intellectual contribution.

The precise balance depends on the discipline and career stage.

7.5 Independence Is contextual

A common mistake is to apply the same expectations to every field. Publication practices differ enormously. In some disciplines, alphabetical authorship is standard. In others, first authorship carries particular significance. Some fields are highly collaborative. Others are not. ERC evaluators know this. They evaluate independence within the norms of the discipline. Your task is not to apologise for your field. Your task is to explain your contribution clearly enough that the panel understands your scientific role.

Case Study

➔ Applicant

"I still publish with my PhD supervisor. Will this damage my chances?"

➔ Discussion

Not necessarily. Publishing with former supervisors is common in many disciplines. The more important question is whether the proposal demonstrates that you have developed your own research direction. If every major achievement appears entirely dependent on your previous mentor, evaluators may question your independence. If collaborations continue while your intellectual agenda clearly evolves, this is rarely problematic. The distinction is subtle but important.

7.6 Leadership Versus Seniority

Many researchers equate leadership with age. The ERC does not. Leadership is not measured by the number of years since obtaining a PhD. It is measured by intellectual influence. A researcher who introduces new ideas, attracts collaborators, mentors young scientists and establishes an independent research direction may demonstrate remarkable leadership early in their career. Conversely, a senior researcher who has remained within the scientific agenda established decades earlier may appear less intellectually independent. Leadership concerns initiative. Not chronology.

7.7 The Hidden Question: Why You?

Every section of the proposal implicitly asks: Why this project? The CV asks something different.

➔ Why you?

Imagine that another excellent researcher submitted exactly the same proposal. Why should the panel prefer yours? Your CV should answer that question naturally. Perhaps because:

- → you possess unique expertise;
- → you have generated preliminary observations unavailable elsewhere;
- → you have access to exceptional infrastructures or cohorts;
- → your previous work has prepared you uniquely for this challenge.

Your proposal and your CV should reinforce one another. Neither should exist in isolation.

7.8 Excellence Is Not Quantity

Applicants frequently assume that more publications automatically strengthen their profile. This is rarely true. Panels are remarkably good at recognising scientific influence. A researcher with fewer publications but several field-changing contributions often appears stronger than one with an extensive publication list lacking a distinctive scientific identity. Quality outweighs quantity. Influence outweighs productivity. Originality outweighs volume.

Table 4 – Productivity versus Scientific Influence

Productivity	Scientific Influence
Number of papers	Contribution to knowledge
Number of citations	Why those citations matter
Number of grants	Scientific vision demonstrated
Number of collaborations	Intellectual leadership within collaborations

7.9 Explaining Your Track Record

The Track Record is not simply evidence of past excellence. It is evidence that the proposed future is credible. Every selected achievement should strengthen one of three messages:

- → I generate original ideas.
- → I influence my discipline.
- → I can successfully lead ambitious research.
- → Achievements that do not contribute to these messages deserve less emphasis.

7.10 Writing About Yourself

Many researchers find this uncomfortable. Scientific culture often discourages self-promotion. However, there is an important distinction between arrogance and clarity. Avoid exaggerated language. Do not write:

"I am one of the world's leading experts..."

Instead, allow evidence to support your position. For example:

"My work introduced the first longitudinal framework for..."

Or "Our methodology has subsequently been adopted by..."

Concrete achievements are more persuasive than adjectives.

Good Example vs Weak Example

➔ Weak

"*I have extensive experience in environmental epidemiology.*"

This tells the evaluator very little.

➔ Better

"*Over the past eight years, my research has focused on understanding how early-life environmental exposures shape long-term health trajectories. This work has led to the development of novel statistical approaches now applied in several international birth cohort studies.*"

The second version demonstrates experience rather than claiming it.

7.11 Matching the CV to the Proposal

Perhaps the most overlooked principle is coherence. Imagine a proposal centred on artificial intelligence for biomedical discovery. Now imagine that the CV primarily highlights work in molecular biology. Evaluators may wonder:

Where does computational expertise come from?

The opposite problem also occurs.

A computational scientist proposes an ambitious biological programme without demonstrating sufficient biological understanding. The strongest proposals create a perfect dialogue between the CV and the scientific vision. Each reinforces the credibility of the other.

7.12 The Final Test

Before submitting, ask yourself one final question. Imagine removing your name from the proposal. Would another researcher appear equally convincing? If the answer is yes, your CV is probably not communicating what makes you uniquely suited to lead this project. The goal is not to prove that you are excellent. The goal is to demonstrate that you are the natural person to pursue this specific scientific vision.

➔ Common Mistakes

- → Treating the CV as a list rather than a narrative.
- → Emphasising productivity instead of originality.
- → Failing to explain individual contributions within collaborations.
- → Repeating information already obvious from the publication list.
- → Using self-promotional language is unsupported by evidence.
- → Ignoring the connection between the proposal and the applicant's scientific trajectory.

NCP'S TIPS

- → Tell a scientific story rather than presenting isolated achievements.
- → Explain why your previous work naturally leads to the proposed project.
- → Demonstrate independence through evidence, not assertions.
- → Let achievements speak for themselves.
- → Make it easy for evaluators to describe your scientific identity in one sentence.

Case Study

➔ Applicant

"I don't have a Nature or Science paper. Is that a serious weakness?"

➔ Answer

Not necessarily. High-profile publications can strengthen a profile, but they are neither necessary nor sufficient for ERC success. Panel members look for evidence of originality, independence and scientific leadership, not journal titles alone. A researcher whose work has significantly influenced a specialised field may present a stronger ERC profile than someone with a single prestigious publication but a less coherent scientific trajectory. The crucial question is not: "Where did you publish?" It is:

"What changed because of your work?"

➔ Questions to Ask Yourself

- → What scientific identity emerges from my CV?
- → Does my track record naturally lead to the proposed project?
- → Have I demonstrated independence through evidence?
- → Can evaluators easily explain why I am the right person for this research?
- → Have I selected achievements for their relevance rather than their number?

➔ Key Takeaways

1. Your CV is an argument, not a biography.
2. Independence is demonstrated through a coherent scientific trajectory, not a single indicator.
3. Scientific influence matters more than publication volume.
4. The proposal and the CV must reinforce one another.
5. The panel should finish reading convinced not only that the project is outstanding, but that you are uniquely positioned to deliver it.

8. WRITING PART B2

In the next chapter we will move from **vision** to **credibility**. Part B2 is where many excellent proposals become unnecessarily long, overly technical, or lose sight of the central scientific narrative established in Part B1. Rather than treating B2 as a repository of methodological details, we will explore how to write it as a logical extension of the scientific vision introduced in the Extended Synopsis. We will discuss research strategy, methodology, feasibility, risk management, preliminary data, figures, work plan, and how to convince evaluators that an ambitious project is also achievable.

"Part B1 convinces the panel that your idea is worth funding. Part B2 convinces them that it can be achieved."

8.1 The Most Common Misunderstanding

Ask ten ERC applicants what Part B2 is for, and most will answer: "It is where I explain the methodology." This answer is understandable. It is also incomplete.nPart B2 is not a methodological appendix. It is **the scientific argument that makes your vision credible.** Think of the proposal as a conversation. Part B1 says: "Here is a bold scientific idea." Part B2 answers the inevitable question: **"Why should we believe you can actually accomplish it?"** Notice the difference. The purpose of B2 is not to impress evaluators with technical sophistication. Its purpose is to eliminate reasonable scientific doubts while preserving the excitement created in Part B1.

8.2 The Transition from B1 to B2

One of the easiest ways to weaken a proposal is to create a sharp discontinuity between B1 and B2. Many applicants spend five pages describing a revolutionary scientific vision, then immediately switch to twenty pages of technical detail. The reader experiences a sudden change of tone. The scientific story disappears. The proposal starts to resemble a grant application rather than an intellectual journey. A successful B2 should feel like the natural continuation of B1. The scientific question remains the protagonist. Methods simply become the tools required to answer it.

➔ Evaluator's Perspective

"I already believe this is an exciting idea. Now convince me that the excitement is justified."

8.3 The Three Goals of Part B2

Every section of B2 should contribute to one of three objectives.

Goal 1 Demonstrate scientific credibility.

Goal 2 Demonstrate methodological robustness.

Goal 3 Demonstrate feasibility despite ambitious objectives.

Everything else is secondary.

8.4 Writing a Scientific Narrative

Many applicants organise B2 according to techniques. For example:

- → sequencing;
- → imaging;
- → statistical analysis;
- → machine learning;
- → validation.

This often produces fragmented proposals. Instead, organise the document around scientific questions. Imagine that each objective tells one chapter of a larger scientific story. For every objective, the evaluator should understand:

- → what question is being addressed;
- → why it matters;
- → what approach will be used;
- → what evidence will answer the question;
- → what happens if the hypothesis is wrong.

Notice that methods occupy only one part of the narrative.

8.5 Objectives Should Drive the Structure

The strongest B2 documents are organised around objectives rather than techniques. A useful template is:

Scientific Question

↓

Hypothesis

↓

Research Strategy

↓

Methodology

↓

Expected Knowledge Gain

↓

Risks

↓

Alternative Strategies

This structure mirrors scientific reasoning.

8.6 Methodology Is Not the Star

Applicants often believe evaluators are primarily interested in technical details. They are not. Panel members want sufficient methodological information to judge credibility. Beyond that point, additional detail produces diminishing returns. Consider two versions.

➔ Version A

Four pages describing every laboratory protocol.

➔ Version B

One page explaining why the chosen methods uniquely answer the scientific question. Version B is usually more persuasive.

Remember: Methods are evidence of competence. Ideas remain the centre of the proposal.

8.7 Demonstrating Feasibility

One of the greatest challenges of ERC writing is balancing ambition with credibility. If the project appears too safe, evaluators question its ambition. If it appears impossible, evaluators question its feasibility. The solution is not to reduce ambition. It is to increase confidence. Confidence emerges from evidence. For example:

- → preliminary observations;
- → pilot studies;
- → proof-of-concept experiments;
- → previous methodological expertise;
- → access to unique infrastructures;
- → successful collaborations.

Each piece of evidence reduces uncertainty without reducing ambition.

➔ Common Mistake

Applicants often attempt to demonstrate feasibility by reducing the scientific challenge. This weakens the proposal. Instead, maintain the challenge, strengthen the evidence.

8.8 Preliminary Data

Should ERC proposals contain preliminary data? The answer is yes—but with an important qualification. Preliminary data should not prove that the project will succeed. If everything is already demonstrated, evaluators may wonder why the research remains necessary. Instead, preliminary data should show that the proposed hypothesis is scientifically plausible. Think of preliminary evidence as opening the door. The project itself explores the room.

8.9 Work Packages

Many applicants inherit work package structures from collaborative Horizon Europe projects. ERC proposals are different. Work Packages exist for organisational purposes. They should never dominate the scientific narrative. The evaluator should remember scientific questions, not WP1, WP2, WP3. Whenever possible, introduce work packages only after explaining the underlying scientific logic.

8.10 Writing About Risks

This section often determines whether evaluators perceive a proposal as genuinely ambitious. Unfortunately, it is also one of the weakest sections in many applications. Applicants frequently list technical problems. For example:

- → recruitment difficulties;
- → equipment failure;
- → delayed deliveries.

These are management issues. ERC panels are far more interested in scientific uncertainty. Examples include:

- → the hypothesis may prove incorrect;
- → current theory may fail completely;
- → expected biological mechanisms may not exist.

These are exciting risks. Scientific risk generates scientific opportunity.

Table 5 – Weak vs Strong Risk Sections

Weak Risk	Strong Risk
Equipment may fail	Central hypothesis may prove incorrect
Recruitment delays	Current conceptual model may not explain observations
Technical optimisation	Alternative biological mechanisms may exist
Budget uncertainties	Competing theoretical explanations

8.11 Alternative Strategies

One misconception deserves clarification. Alternative strategies should not eliminate scientific risk. Imagine writing: If hypothesis A fails, we will simply investigate hypothesis B. This may unintentionally suggest that every outcome produces the same scientific conclusions. Instead, alternative strategies should preserve scientific discovery. For example: If the proposed mechanism is not supported, the project will investigate an alternative conceptual explanation with equally important implications. Failure should still generate knowledge.

➔ Evaluator's Perspective

"I am not worried if your hypothesis is wrong. I am worried if your project becomes scientifically meaningless when it is wrong."

8.12 Figures in Part B2

Figures perform a different role here than in B1. In B1, figures communicate ideas. In B2, figures explain strategy. Good B2 figures often illustrate:

- → objective relationships;
- → methodological logic;
- → experimental design;
- → decision trees;
- → contingency pathways.

Avoid overly detailed workflows copied from laboratory protocols. Figures should reduce cognitive effort, not increase it.

8.13 Writing Clearly About Complex Methods

Many applicants assume that technical sophistication requires technical writing. The opposite is usually true. The more sophisticated the methodology, the simpler the explanation should become. Imagine explaining your strategy to an excellent scientist from the neighbouring panel. Could they follow the reasoning? If not, simplify. Scientific depth and linguistic complexity are different concepts.

Case Study

➔ Applicant

"My methodology is extremely innovative. Should I explain every technical detail?"

➔ Discussion

Only if those details are necessary for evaluating scientific credibility. Remember that evaluators are trying to answer: **Can this project realistically achieve its objectives?** They are rarely asking: Can I reproduce every protocol? Technical appendices belong in supplementary documentation. Scientific reasoning belongs in B2.

8.14 Maintaining Momentum

Many proposals lose energy halfway through B2. The first objectives appear exciting. The remaining pages become increasingly descriptive. Avoid this. Every objective should conclude by reminding evaluators: What new scientific understanding will emerge? Curiosity should continue growing until the final page.

8.15 Ending Part B2

The conclusion of B2 should reconnect with the beginning of B1. Return to the central scientific question. Remind evaluators:

- → why the problem matters;
- → why the proposed strategy is convincing;
- → what scientific transformation success would produce.

A satisfying ending creates intellectual closure. The proposal feels complete.

Good Example vs Weak Example

➔ Weak

Objective 3 will apply machine learning models to integrate imaging and omics data.

➔ Better

Objective 3 will determine whether integrating imaging and omics data reveals organisational principles that remain invisible under current analytical frameworks. Machine learning serves as the methodological bridge enabling this conceptual advance.

Again, the science dominates. The methodology supports it.

➔ Common Mistakes

- → Treating B2 as a methods section.
- → Organising chapters around technologies instead of scientific questions.
- → Providing excessive protocol detail.
- → Presenting technical rather than scientific risks.
- → Disconnecting B2 from the vision established in B1.
- → Writing objectives as tasks rather than discoveries.
- → Forgetting to explain why each methodological choice matters scientifically.

NCP'S TIPS

- → Let scientific questions organise the proposal.
- → Explain why every method is necessary.
- → Demonstrate feasibility through evidence.
- → Preserve scientific ambition when discussing risks.
- → End every objective with knowledge, not activities.

➔ Questions to Ask Yourself

- → Does every methodological choice answer a scientific question?
- → Would the proposal remain understandable if technical detail were reduced?
- → Have I distinguished scientific risks from management risks?
- → Is every objective linked to the central hypothesis?
- → Does B2 strengthen rather than dilute the narrative established in B1?

➔ Key Takeaways

- → B2 is about credibility, not technical detail.
- → Scientific questions should determine the structure.
- → Methods support ideas—they do not replace them.
- → High scientific risk is desirable when accompanied by a robust research strategy.
- → The last page of B2 should leave evaluators convinced that the proposed vision is both ambitious and achievable.

9. THE ART OF SCIENTIFIC RISK

Why the ERC Funds Uncertainty

"The purpose of an ERC grant is not to reduce uncertainty. It is to explore uncertainty that has the potential to transform knowledge."

9.1 The Great Paradox

Almost every research funding programme asks applicants to minimise risk. The ERC does something fundamentally different. It asks applicants to embrace it. This is perhaps the single most distinctive feature of the ERC philosophy and, paradoxically, one of the most misunderstood. Applicants spend months making their projects appear safe. They reassure evaluators. They remove uncertainty. They anticipate every possible criticism. By the time they submit the proposal, the project often appears technically impeccable. Unfortunately, it also appears scientifically predictable. The ERC was not created to fund predictable science. It was created because predictable science rarely changes the direction of a discipline.

9.2 What Does "High-Risk / High-Gain" Really Mean?

The phrase appears throughout discussions of ERC proposals. Yet it is often interpreted incorrectly. Many researchers associate risk with failure. Others associate it with poor planning. Neither interpretation captures the ERC perspective. For the ERC, risk is not an absence of preparation. Risk is the possibility that nature may not behave as we currently believe. High-gain research is therefore research capable of producing discoveries that fundamentally alter current understanding. Risk and gain are inseparable. Without uncertainty, there is rarely genuine discovery.

➔ Evaluator's Perspective

"If every outcome of the project is already predictable, why should the ERC fund it?"

9.3 Three Types of Risk

One of the most useful distinctions applicants can make is between different forms of uncertainty.

➔ Technical Risk

- → Can the experiment be performed?
- → Will the equipment work?
- → Can the samples be collected?

These are important questions. However, they are rarely what excite ERC evaluators.

➔ Methodological Risk

Can the proposed analytical strategy answer the question? Will the computational framework perform as expected? Again, important, but still secondary.

➔ Conceptual Risk

This is where the ERC becomes different. Conceptual risk asks: **What if our current understanding is fundamentally wrong?** This is the risk that transforms disciplines.

Table 6 – Understanding Scientific Risk

Type of Risk	Example	ERC Relevance
Technical	Equipment failure	Low
Operational	Recruitment delays	Low
Methodological	Algorithm underperforms	Moderate
Scientific	Hypothesis not supported	High
Conceptual	Existing paradigm proves incorrect	Very High

9.4 Safe Science Is Often Invisible

Imagine reading twenty proposals. Nineteen promise to improve existing knowledge. One proposes to test whether an assumption accepted for thirty years is fundamentally incorrect. Which proposal will the panel remember? The answer is obvious. Panels remember intellectual courage. This does not mean they automatically fund it. But they remember it. Memory matters. Panel discussions begin with proposals that evaluators can still vividly recall.

9.5 The Difference Between Bold and Reckless

Some applicants misunderstand ambition. They propose ideas that have almost no scientific foundation. Others make claims that cannot realistically be evaluated within the duration of the project. This is not high-risk research. It is poor project design. A successful ERC proposal balances two apparently contradictory qualities. It is intellectually daring. It is scientifically disciplined. The proposal should persuade evaluators that success is uncertain, but that every step has been carefully designed to maximise learning regardless of the outcome.

➔ **Common Mistake**

Applicants sometimes equate uncertainty with a lack of planning. A well-designed high-risk project should appear **more** carefully planned than a conventional project, not less.

9.6 Failure Can Be Scientifically Valuable

This idea deserves particular attention. Most research grants implicitly assume that success means confirming the hypothesis. The ERC takes a broader view. Suppose your central hypothesis proves wrong. Does the project still generate important knowledge? If the answer is yes, you are thinking like an ERC investigator. Scientific failure often eliminates widely accepted explanations and opens entirely new research directions. The proposal should communicate this clearly.

➔ **Good Example**

If the proposed mechanism is not supported, the project will establish definitive evidence against the current conceptual framework and provide the basis for developing alternative models.

Notice the wording. Failure still transforms understanding.

➔ **Weak Example**

If the experiment fails, an alternative laboratory technique will be used.

This addresses technical continuity. It says nothing about scientific discovery.

9.7 Why Preliminary Data Matter

Applicants frequently ask: "If ERC values uncertainty, why should I provide preliminary data?" The answer is simple. Preliminary data reduce uncertainty about **feasibility**. They should not reduce uncertainty about **discovery**. These are different concepts. Imagine a bridge. Preliminary data demonstrate that the bridge can be built. They should not reveal everything waiting on the other side.

9.8 The Courage to Challenge Assumptions

Most scientific progress is incremental. Occasionally, however, a project questions one assumption so fundamental that entire research programmes evolve. ERC projects should aspire to challenge assumptions within their own field.

➔ Evaluator's Perspective

"I am less interested in whether the applicant is right than in whether the question is worth asking."

9.9 Writing About Risk

Applicants often treat the "risk" section as an obligation. In reality, it is an opportunity. A well-written discussion of risk demonstrates intellectual maturity. Rather than attempting to reassure evaluators that everything will work perfectly, acknowledge uncertainty openly.

For example:

The proposed hypothesis challenges a central assumption in developmental immunology. It is therefore entirely possible that current theoretical models prove insufficient to explain the observed phenomena.

This sentence does not weaken the proposal. It strengthens credibility.

9.10 Risk Is Different Across Disciplines

The expression "high-risk" has different meanings in different scientific communities. In mathematics, risk may involve pursuing a conjecture that has resisted proof for decades. In archaeology, it may involve developing a radically new interpretation of historical evidence. In epidemiology, it may consist of challenging the conceptual foundations of causal inference. In engineering, it may arise from attempting an entirely new technological architecture. The form changes. The principle remains identical. Risk concerns uncertainty about knowledge.

Case Study

➔ Applicant

"My reviewers always tell me to reduce the ambition because the project seems risky."

➔ Discussion

This advice is common—and often sensible in many funding schemes. However, the ERC is different. The objective is not to reduce ambition. The objective is to demonstrate that ambitious questions can be investigated through rigorous scientific reasoning. Instead of lowering the scientific ambition, ask yourself:

- → Have I explained why the question matters?
- → Have I shown that the methodology is appropriate?

- → Have I demonstrated the expertise needed to pursue it?
- → If the answer is yes, the project may be risky in exactly the right way.

9.11 The Relationship Between Risk and Leadership

An overlooked aspect of ERC evaluation is that scientific risk reflects scientific leadership. Researchers who repeatedly challenge accepted assumptions gradually develop reputations as intellectual leaders. The proposal should therefore communicate not only an ambitious idea but also a willingness to explore difficult scientific questions responsibly. Leadership is demonstrated not by claiming certainty, but by showing confidence in tackling uncertainty.

9.12 The "Would It Matter?" Test

Imagine that your hypothesis is confirmed. Now ask: Would anyone outside my immediate research niche care? If the answer is uncertain, the gain may not be sufficiently high. Next, imagine that the hypothesis is disproved. Would the field still learn something important? If the answer is yes, your project has achieved a remarkable balance. Either outcome advances knowledge. This is precisely the kind of intellectual design that ERC panels appreciate.

Good Example vs Weak Example

➔ Weak

We will investigate whether biomarker X is associated with disease Y.

The scientific gain is limited.

➔ Better

We will determine whether the widely accepted assumption that biomarker X directly reflects disease progression is fundamentally incorrect. If confirmed, this finding would require a reassessment of current diagnostic strategies and disease models.

The scientific stakes are immediately clearer.

➔ Common Mistakes

- → Confusing technical problems with scientific risks.
- → Attempting to eliminate all uncertainty.
- → Presenting alternative strategies that remove intellectual ambition.

- → Treating preliminary data as proof that the project will succeed.
- → Writing risk sections that focus exclusively on project management.
- → Underestimating the scientific value of negative results.

NCP'S TIPS

- → Be honest about uncertainty.
- → Distinguish feasibility from discovery.
- → Let conceptual risk drive the proposal.
- → Show that failure still produces important knowledge.
- → Demonstrate careful planning without reducing scientific ambition.

➔ Questions to Ask Yourself

- → What accepted assumption does my project challenge?
- → Where does the real scientific uncertainty lie?
- → Would failure still advance knowledge?
- → Have I reduced technical uncertainty while preserving conceptual uncertainty?
- → Does my discussion of risk increase or decrease the excitement of the proposal?

➔ Key Takeaways

1. The ERC does not reward reckless projects; it rewards intellectually courageous ones.
2. Scientific risk is more important than technical risk.
3. Preliminary data should demonstrate feasibility, not certainty.
4. Negative results can be scientifically transformative.
5. A convincing discussion of uncertainty strengthens rather than weakens credibility.

10. RESOURCES, BUDGET AND SCIENTIFIC CREDIBILITY

Why Every Euro Should Tell a Scientific Story

"The budget is not a financial document. It is part of your scientific argument."

10.1 The Budget Begins with the Science

Many applicants approach the budget as the final administrative task before submission. They finish writing the proposal. Then they ask themselves: "How much money do I need?" This is precisely the wrong order. The budget should emerge naturally from the scientific vision. Every requested resource should answer a simple question:

Why is this necessary to achieve the scientific objectives?

Notice the wording. Not: Why would it be useful? Not: Why would it be nice to have? But: **Why is it scientifically necessary?** This distinction changes the entire philosophy of budgeting.

10.2 How Evaluators Read the Budget

Most applicants imagine evaluators carefully checking arithmetic. In reality, panels rarely spend much time analysing numbers themselves. Instead, they look for coherence. They ask questions such as:

- → Does the requested team make sense?
- → Are the infrastructures appropriate?
- → Is the requested equipment justified?
- → Does the scale of the project match the requested resources?
- → Would the project still be possible with substantially fewer resources?

Notice that none of these questions concerns accounting. They concern scientific credibility.

➔ Evaluator's Perspective

"I am not asking whether this microscope costs €900,000 or €950,000. I am asking whether the scientific question genuinely requires such an instrument."

10.3 Resources Reflect Scientific Vision

One useful way to think about an ERC budget is this: Resources are physical manifestations of ideas. A revolutionary hypothesis often requires:

- → new expertise,
- → unique infrastructures,
- → unusual collaborations,
- → ambitious data collection,
- → specialised equipment,
- → extensive computational resources.

The budget therefore becomes another way of demonstrating ambition. A transformative scientific question supported by an unrealistically small budget may appear inconsistent. Conversely, a modest scientific question requesting exceptional resources creates a different inconsistency. Strong proposals maintain equilibrium.

10.4 Building the Research Team

Applicants often ask: "How many postdoctoral researchers should I request?" There is no universal answer. The better question is: **What expertise is required to answer the scientific question?** Begin with the science. Suppose your project combines:

- → developmental biology,
- → artificial intelligence,
- → epidemiology,
- → advanced imaging.

The team should naturally reflect these competencies. Do not build a team because similar ERC projects have one. Build the team that your scientific vision demands.

Case Study

➔ Applicant

"I can probably complete the project with two postdocs, but requesting three would make things easier. Should I ask for the third?"

➔ **Discussion**

Imagine explaining that decision to the panel. Could you convincingly describe the scientific role of the third researcher? If yes, request them. If not, you probably do not need them. Remember: Every position should correspond to a clearly identifiable scientific contribution. Panels are remarkably good at recognising "comfort personnel."

10.5 Equipment Should Solve Scientific Problems

One of the weakest justifications found in ERC proposals is: "The equipment is necessary because we do not currently own it." Ownership is irrelevant. Scientific necessity is everything. Instead, explain:

- → what scientific limitation currently exists;
- → why existing infrastructures cannot overcome it;
- → how the new equipment enables discoveries that would otherwise remain impossible.

Equipment purchases should always be linked to knowledge generation. Not institutional capacity.

Good Example

Current imaging platforms cannot resolve cellular interactions at the temporal resolution required to test the central hypothesis. The proposed microscope enables observation of processes that are currently inaccessible.

Notice that the equipment disappears behind science. Exactly as it should.

10.6 Large Equipment Requests

Large equipment requests often create anxiety. Applicants worry that evaluators will consider them excessive. In reality, ERC panels regularly support substantial equipment purchases when they are integral to the scientific vision. The critical issue is not cost. It is indispensability. Ask yourself: Could another reasonable approach answer the same scientific question? If yes, explain why the proposed approach remains superior. If no, make that argument explicit.

10.7 Consumables

Consumables rarely receive much attention. Nevertheless, they contribute to the overall credibility of the proposal. Extremely ambitious laboratory programmes supported by unrealistically small consumable budgets immediately raise concerns. Likewise, very large consumable requests without corresponding experimental activity may appear inflated. Consistency matters.

10.8 Personnel Costs

People—not equipment—are usually the most valuable resource in an ERC project. Applicants sometimes describe personnel generically:

- → Postdoc 1
- → Postdoc 2
- → Technician

This wastes an opportunity. Instead, describe scientific functions. For example:

A postdoctoral researcher with expertise in causal inference will develop the statistical framework required to integrate longitudinal exposure trajectories.

Now the position has become intellectually necessary.

10.9 Collaborations

Another misconception deserves attention. Collaborations are not automatically strengths. Panels often ask: Who is intellectually leading this research? If key discoveries depend entirely upon external collaborators, questions about the PI's independence may emerge. Collaborations should strengthen the project. They should not substitute for scientific leadership.

➔ Evaluator's Perspective

"I welcome collaboration. I do not want the project to depend upon scientific leadership outside the ERC grant."

10.10 Subcontracting

Applicants often become uncertain when discussing subcontracting. The guiding principle is surprisingly simple. Ask: Is this scientific thinking or scientific service? Routine sequencing, specialised imaging, animal facility services, or technical analyses frequently constitute services. Scientific interpretation, hypothesis development, experimental design, or conceptual contributions usually do not. The intellectual ownership of the project should remain firmly with the PI.

10.11 Additional Funding

One distinctive feature of the ERC is the possibility of requesting additional funding beyond the standard grant ceiling. Applicants sometimes hesitate. They worry that requesting additional funding appears greedy. Panels do not think in these

terms. Instead, they ask: Does this genuinely expand the scientific possibilities? Additional funding should therefore support exceptional circumstances, such as:

- → major equipment,
- → access to unique research infrastructures,
- → relocation,
- → establishment of a laboratory,
- → exceptional experimental capabilities.

Again, the justification must remain scientific.

10.12 Value for Money

ERC panels rarely discuss "cost efficiency" in the way innovation programmes sometimes do. Instead, they think about proportionality. Does the requested investment correspond to the expected scientific gain? Imagine two projects requesting €2.5 million. One proposes incremental improvements. The other seeks to redefine an entire discipline. The same budget may appear excessive in one case, entirely reasonable in the other. Scientific ambition provides the context.

10.13 The Budget as Evidence

Perhaps the most overlooked principle is this: The budget itself communicates information. Consider these examples. A proposal requesting:

- → no computational expertise,

while relying heavily on AI, creates doubt.

A proposal proposing an ambitious international birth cohort with minimal travel resources creates doubt. A proposal centred on sophisticated molecular analyses without laboratory personnel creates doubt. The budget silently tells evaluators whether the applicant has realistically imagined the project.

Good Example vs Weak Example

➔ Weak

Equipment: €850,000

Justification:

Purchase of high-resolution microscope.

➔ Better

Equipment: €850,000

Justification:

The project's central hypothesis requires direct observation of dynamic intracellular processes occurring over milliseconds. Existing institutional facilities cannot achieve the necessary temporal resolution. Without this capability, the central scientific question cannot be addressed.

Notice the difference. The second justification never argues about price. It argues about discovery.

10.14 Writing the Resource Justification

Imagine removing every reference to money. Would the explanation still make scientific sense? If not, rewrite it. The strongest justifications explain scientific necessity first. Costs become almost incidental.

Case Study

➔ Applicant

"I already have access to a sequencing facility. Should I still request sequencing costs?"

➔ Discussion

Potentially, yes. The important question is not whether sequencing is available. It is whether the facility can provide the scale, quality, flexibility and scientific independence required by the project. If outsourcing a service allows the project to answer its central scientific question more effectively, then the request may be entirely appropriate. Always justify the choice in terms of scientific strategy, not administrative convenience.

➔ Common Mistakes

- → Preparing the budget after the proposal is finished.
- → Justifying equipment by ownership rather than scientific necessity.
- → Describing personnel without explaining their intellectual role.
- → Confusing collaborations with scientific leadership.

- → Overlooking inconsistencies between ambition and requested resources.
- → Treating the budget as an administrative appendix.

NCP'S TIPS

- → Every major cost should support one scientific objective.
- → Explain resources through the discoveries they enable.
- → Build the team around expertise, not job titles.
- → Demonstrate proportionality between ambition and investment.
- → Make the budget feel inevitable.

➔ Questions to Ask Yourself

- → Could every requested resource be defended scientifically?
- → Does the budget reflect the ambition of the proposal?
- → Have I explained why existing infrastructures are insufficient?
- → Would evaluators understand the role of every team member?
- → Does the budget strengthen my credibility as a scientific leader?

➔ Key Takeaways

1. The budget is part of the scientific narrative.
2. Resources should arise naturally from the research questions.
3. Equipment is justified by the discoveries it enables, not by its specifications.
4. Every team member should have a clearly defined intellectual contribution.
5. A coherent budget reinforces the credibility of the entire proposal.

11. INSIDE THE ERC PANEL ROOM

How Evaluators Really Make Decisions

"The proposal you submit is not the proposal that gets funded. The funded proposal is the one that survives a room full of scientific discussion."

11.1 The Moment After Submission

For the applicant, submission feels like the end of a long journey. For the ERC, it is only the beginning. Once the deadline has passed, your proposal starts a life of its own. It will be read by people you have never met. It will be interpreted through different scientific backgrounds. It will be discussed, questioned, defended, compared with dozens of other outstanding proposals and, ultimately, judged not in isolation but alongside the very best ideas submitted across Europe and beyond. This is an important mental shift. You are no longer writing for yourself. You are writing for a conversation that will take place in your absence.

11.2 A Proposal Is Read More Than Once

Applicants often imagine a single evaluator reading the proposal from beginning to end. Reality is more complex. Different evaluators read the proposal with different objectives. One may focus on conceptual originality. Another may examine methodological credibility. A third may concentrate on the Principal Investigator's profile. External reviewers often approach the proposal as specialists. Panel members frequently read it as broad disciplinary experts. The same document therefore needs to work at multiple levels simultaneously. This explains why clarity is so important.

➔ **Evaluator's Perspective**

"I may not remember every methodological detail, but I need to remember the central scientific idea when we discuss this proposal tomorrow."

11.3 The Invisible Competition

One of the most difficult aspects of ERC evaluation is that proposals are never judged against an abstract standard. They are judged against one another. Imagine twenty excellent proposals. Every one of them deserves funding. Unfortunately, only a fraction can actually receive it. This changes the evaluator's mindset. The question gradually shifts from:

"Is this excellent?"

to

"Is this more compelling than the proposal I discussed ten minutes ago?"

Applicants sometimes forget this. ERC evaluation is comparative. Not absolute.

11.4 Memory Is a Competitive Advantage

Psychologists have long known that human decision-making depends heavily on memory. ERC panels are no exception. Suppose a proposal contains:

- → one unforgettable scientific question;
- → one elegant conceptual figure;
- → one powerful central hypothesis.

Now compare it with a technically perfect proposal containing twenty equally important ideas. Which proposal will evaluators naturally remember? Almost always, the simpler narrative. This does not mean oversimplifying science. It means giving evaluators something cognitively stable around which they can organise discussion.

11.5 Consensus Is Not Mathematics

Applicants often imagine that evaluation consists of averaging scores. Real panel discussions are much richer. Panel members explain. Challenge. Question. Persuade. Sometimes one enthusiastic evaluator changes the entire discussion. Sometimes a seemingly minor concern gradually becomes decisive. Consensus develops through scientific argument. Not arithmetic.

➔ Evaluator's Perspective

"Can I confidently defend this proposal to my colleagues?"

Notice the wording. Evaluators do not merely assess proposals. They advocate for them—or fail to.

11.6 The Importance of Scientific Champions

One of the least discussed aspects of panel meetings is the role of scientific champions. Every funded proposal usually has someone in the room who genuinely believes in it. That person explains its originality. Clarifies misunderstandings. Responds to concerns. Places weaknesses into context. Applicants cannot choose their champion. But they can make advocacy easier. Clear proposals are easier to defend. Confusing proposals require explanation. And every explanation introduces uncertainty.

Case Study

➔ Applicant

"Should I anticipate every possible criticism?"

➔ Discussion

Not every criticism. That would produce an impossibly long proposal. Instead, anticipate the criticisms that threaten the central scientific argument. If evaluators can answer their own major doubts while reading the proposal, discussions become smoother. Your objective is not to eliminate disagreement. It is to minimise misunderstanding.

11.7 Why Small Weaknesses Sometimes Matter

Applicants often become frustrated. "My proposal was criticised for something relatively minor." Sometimes that observation is correct. Yet context matters. Imagine two equally outstanding proposals. One contains no obvious weaknesses. The other contains one unresolved concern. That concern may become surprisingly influential—not because it is intrinsically severe, but because panels must distinguish between proposals operating at an exceptionally high level. In elite competitions, margins become small. This is why careful editing matters.

11.8 Why Great Proposals Are Rejected

Perhaps the hardest lesson for applicants to accept is this: Many rejected ERC proposals are genuinely excellent. Panels know this. Evaluators know this. Rejection is not always evidence of weakness. Sometimes it simply reflects extraordinary competition. This is one reason why experienced applicants often succeed after resubmission. Not because the original proposal lacked quality, but because excellent ideas can often be made even clearer.

➔ Evaluator's Perspective

"I wish we had enough funding for this proposal as well." This sentence is heard more often than applicants realise.

11.9 Reading Between the Lines

Panel discussions rarely focus exclusively on what is written. They also explore what is implied. For example:

- → Does this PI seem intellectually independent?

- → Does this hypothesis genuinely challenge current thinking?
- → Does the requested budget reflect the proposed ambition?
- → Will this researcher become a future leader?

These questions are rarely written explicitly in the proposal. Nevertheless, they strongly influence evaluation. Good proposals answer implicit questions before they are asked.

11.10 Confidence Inspires Confidence

Language affects perception. Compare these statements. We hope to investigate... versus We will determine whether... The second communicates scientific confidence. Not arrogance. Confidence encourages confidence. Uncertain writing creates uncertain readers. Applicants sometimes believe cautious language appears more scientific. Excessive caution often has the opposite effect.

11.11 The Proposal Evolves During Discussion

One fascinating aspect of panel meetings is that proposals change conceptually. Different evaluators notice different strengths. Someone may identify an originality that others initially overlooked. Someone else may reveal a methodological weakness. By the end of the discussion, the proposal exists as a collective interpretation. Your writing therefore needs to guide interpretation as much as possible.

11.12 The Final Ranking

Eventually, every proposal must find its place relative to the others. At this stage, comparisons become increasingly direct. Panels ask themselves questions such as:

- → Which proposal would most transform its field?
- → Which PI appears most capable of delivering?
- → Which project would we regret not funding?

Notice that these are fundamentally qualitative questions. Scientific excellence cannot be reduced to mathematics.

Good Example vs Weak Example

➔ Weak Proposal Experience

Evaluators agree that the proposal is technically excellent. No one strongly argues in its favour. Discussion ends quickly. The proposal quietly settles in the middle of the ranking.

➔ Strong Proposal Experience

The proposal generates discussion. One evaluator passionately explains why the idea could redefine the field. Others ask questions. Concerns are answered. Consensus gradually emerges. The proposal rises. Excellent proposals are remembered. Outstanding proposals generate advocates.

11.13 What Makes Evaluators Fight for a Proposal?

This is perhaps the most important question in the entire handbook. Not: What makes evaluators like a proposal? But: **What makes evaluators defend a proposal?** In our experience, several characteristics repeatedly appear. The proposal presents:

- → one exceptionally clear scientific vision;
- → a genuinely important question;
- → a bold but credible hypothesis;
- → an outstanding PI;
- → a coherent research strategy.

Above all, it gives evaluators a compelling scientific story they can easily explain to one another.

➔ Common Mistakes

- → Assuming evaluation is mechanical.
- → Writing for reviewers rather than for panel discussion.
- → Presenting too many equally important ideas.
- → Ignoring how memory influences comparison.
- → Confusing technical perfection with scientific excitement.
- → Assuming excellent science automatically explains itself.

NCP'S TIPS

- → Make your proposal easy to remember.
- → Build one central scientific narrative.
- → Anticipate major concerns.
- → Write so that others can advocate for your idea.
- → Remember that evaluation is ultimately a scientific conversation.

➔ Questions to Ask Yourself

- → What sentence will evaluators remember tomorrow?
- → Could someone summarise my proposal in one minute?
- → What argument would my strongest advocate use during panel discussion?
- → What concern would my strongest critic raise?
- → Have I made advocacy easier than criticism?

Case Study

➔ Applicant

"If my proposal is objectively excellent, shouldn't that be enough?"

➔ Answer

No. And this may be one of the most important lessons of ERC proposal writing. Scientific excellence is essential—but excellence also has to be communicated, understood, remembered and defended. A proposal that requires lengthy explanation during panel discussion starts at a disadvantage compared with one whose scientific vision is immediately clear. The goal is not simply to write an excellent proposal. The goal is to write a proposal that remains excellent after being interpreted by multiple readers with different expertise and then debated around a table.

➔ Key Takeaways

1. ERC evaluation is a scientific discussion, not a mathematical exercise.
2. Your proposal is competing with many other outstanding proposals.
3. Clarity helps evaluators become advocates.
4. Memorable scientific ideas have a strategic advantage.
5. The proposal that is funded is often the one that is easiest to explain, defend and remember.

12. WRITING FOR THE EVALUATOR

Why Brilliant Science Is Sometimes Forgotten

"Evaluators do not fund the proposal they read. They fund the proposal they remember."

12.1 The Greatest Illusion in Proposal Writing

Most scientists believe that if an idea is sufficiently important, it will naturally be understood. This seems reasonable. Unfortunately, it is not how human cognition works. Understanding and remembering are two different processes. A reviewer may fully understand your proposal while reading it. Twenty-four hours later, during the panel discussion, they may remember almost none of it. This is not because your science lacked quality. It is because the human brain was never designed to remember twenty complex research proposals simultaneously. Excellent proposal writing therefore involves more than communicating ideas. It involves designing ideas that survive memory.

12.2 Cognitive Load

Imagine reading fifteen ERC proposals in one week. Each proposal contains:

- → dozens of hypotheses,
- → hundreds of technical terms,
- → multiple figures,
- → several objectives,
- → numerous methodologies.

The reader's working memory becomes overloaded remarkably quickly. Psychologists refer to this as cognitive load. Once cognitive load becomes excessive, understanding begins to deteriorate. Even experienced scientists become mentally fatigued. Applicants often respond by adding even more explanation. Ironically, this usually increases the problem.

➔ Evaluator's Perspective

"I understand every paragraph individually. I no longer understand how they fit together."

12.3 Working Memory Is Small

Modern cognitive psychology consistently shows that working memory is extremely limited. Readers cannot actively manipulate dozens of independent concepts simultaneously. Consequently, the proposal should minimise unnecessary mental effort. Every paragraph should help the reader answer one question before introducing the next. The evaluator should never wonder: Why am I reading this now? Every section should feel inevitable.

12.4 The Curse of Knowledge

Perhaps the greatest challenge facing ERC applicants is something psychologists call the curse of knowledge. Once we become experts, it becomes extraordinarily difficult to imagine what it feels like not to know something. As a result, experts routinely skip essential conceptual steps. They assume connections that exist only inside their own minds. The proposal appears perfectly logical—to its author. The evaluator experiences unexpected jumps. This phenomenon affects every scientific discipline.

Case Study

➔ Applicant

"My panel members are experts. Why should I simplify?"

➔ Discussion

Because expertise is not the same as specialisation. Panel members are experts in broad scientific areas. Few will work on exactly the same research question. The objective is not simplification. It is transparency. Good explanations never reduce scientific sophistication. They reduce unnecessary effort.

12.5 The Primacy Effect

Human beings remember beginnings disproportionately well. Psychologists call this the primacy effect. The first ideas encountered create a mental framework through which later information is interpreted. This has profound implications for ERC proposals. The opening paragraph should establish:

- → the scientific problem;
- → the conceptual tension;
- → the central question.

Do not begin with historical background. Do not begin with definitions. Do not begin with literature reviews. Begin with

curiosity.

12.6 The Recency Effect

The opposite phenomenon also exists. Readers tend to remember endings. This is why conclusions matter. Many applicants finish with administration. Timelines. Management. Deliverables. Instead, the proposal should finish where it began. With science. Leave evaluators thinking about discovery, not logistics.

12.7 Repetition Is Not Redundancy

Scientists often fear repetition. They associate it with poor writing. Yet cognitive psychology tells a different story. Important ideas should appear several times throughout a document. Not copied. Reinforced. For example, the central hypothesis might appear:

- → in the introduction,
- → when introducing objectives,
- → in the discussion of methodology,
- → in the concluding section.

Each appearance deepens understanding. This is not redundancy. It is cognitive reinforcement.

Good Example

Opening:

We hypothesise that...

Later:

Objective 2 tests this hypothesis by...

Later still:

If confirmed, the hypothesis would imply...

The idea gradually becomes impossible to forget.

12.8 Chunking

One of the brain's most powerful strategies is chunking. Rather than remembering isolated facts, people remember meaningful groups. Excellent proposals exploit this naturally. Instead of presenting twelve disconnected objectives, group them into three coherent scientific themes. Instead of describing twenty techniques, organise them around four scientific questions. The evaluator remembers the structure, not every detail.

12.9 Figures as Cognitive Anchors

Perhaps the most underestimated component of ERC proposals is the conceptual figure. Many applicants treat figures as illustrations. In reality, the brain often uses images to organise abstract ideas. A good conceptual figure becomes a cognitive anchor. During panel discussion, evaluators frequently reconstruct the proposal around that mental image. Ask yourself:

Could someone redraw my central figure from memory?

If yes, it is probably working.

➔ Evaluator's Perspective

"I cannot remember every paragraph, but I clearly remember that diagram showing how the three hypotheses connected."

12.10 Narrative Beats Information

Human beings evolved to understand stories long before they understood scientific papers. Stories answer three questions naturally:

- → What is the problem?
- → What changes?
- → Why does it matter?

Scientific proposals follow the same psychological logic. Without narrative, information becomes difficult to organise. Narrative is not decoration. It is structure.

12.11 Signposting

Readers should never feel lost. Simple expressions help enormously. For example: *This leads to three questions. The next objective addresses... This result would fundamentally change...* These phrases require almost no cognitive effort. Yet they continuously

orient the evaluator. Good writing guides attention.

12.12 One Central Idea

Applicants often believe that more originality requires more ideas. The opposite is usually true. Most outstanding ERC proposals revolve around one unforgettable concept. Everything else supports it. Imagine asking an evaluator one week later: "What was this proposal about?" The answer should fit into one sentence. Not because the science is simple, but because the central vision is clear.

Case Study

➔ Applicant

"My project has five revolutionary ideas."

➔ Discussion

Perhaps. But evaluators are unlikely to remember five equally important concepts. Instead, identify the one idea that changes everything. Allow the remaining innovations to reinforce it. Unity creates memorability.

12.13 Writing Under Time Pressure

Applicants often imagine evaluators reading slowly with unlimited concentration. Reality is different. Panel members manage:

- → proposals,
- → teaching,
- → laboratories,
- → students,
- → travel,
- → committee work.

Even highly motivated readers experience interruptions and fatigue. Good proposals respect the reader's attention. They make understanding easy. Not because evaluators are incapable. Because their attention is valuable.

12.14 The Recognition Test

Here is a simple exercise. Give your proposal to a colleague. Twenty-four hours later, ask three questions. What scientific problem was I addressing? What was my central hypothesis? What made the proposal original? If they hesitate, your proposal probably asks too much of memory.

Good Example vs Weak Example

➔ Weak

The proposal introduces:

- → six hypotheses,
- → eight objectives,
- → four conceptual frameworks,
- → three competing models.

Everything appears important. Nothing becomes memorable.

➔ Better

The proposal revolves around one central hypothesis. Three objectives explore different consequences. Methods naturally support each objective. By the end, the evaluator can easily reconstruct the scientific logic.

➔ Common Mistakes

- → Confusing detail with clarity.
- → Introducing multiple equally important ideas.
- → Writing paragraphs without transitions.
- → Using figures that explain techniques rather than concepts.
- → Avoiding repetition of key ideas.
- → Assuming understanding automatically produces memory.

NCP'S TIPS

- → Reduce unnecessary cognitive effort.
- → Repeat important concepts strategically.
- → Use conceptual figures as memory anchors.
- → Organise ideas hierarchically.
- → Help evaluators explain your proposal to others.

➔ Questions to Ask Yourself

- → What is the one sentence evaluators should remember?
- → Does every section reinforce the central hypothesis?
- → Could someone reconstruct the proposal from memory?
- → Have I organised ideas into coherent groups?
- → Does my proposal respect the reader's limited attention?

➔ Key Takeaways

1. Scientific excellence is necessary but not sufficient; it must also be memorable.
2. Working memory is limited, even for expert evaluators.
3. Narrative reduces cognitive load.
4. One unforgettable idea is stronger than many competing ideas.
5. The easiest proposal to remember is often the easiest proposal to defend.

13. THE ERC ACROSS CAREER STAGES

How Expectations Change from Starting to Advanced Grants

"The ERC does not ask every applicant to demonstrate the same qualities. It asks each applicant to demonstrate excellence appropriate to their stage of scientific maturity."

13.1 One Philosophy, Four Career Stages

One of the most common misconceptions about the ERC is that the different grant schemes simply correspond to different budgets or career ages. They do not. Each grant represents a different scientific question. A Starting Grant asks: **Can this researcher become an international scientific leader?**

A Consolidator Grant asks: **Is this researcher already establishing an independent research programme?**

An Advanced Grant asks: **Can this investigator redefine the direction of an entire discipline?**

The evaluation philosophy evolves. So should the proposal.

13.2 Starting Grant

At this stage, panels are not looking for a "mini-Advanced Grant." They know applicants are still building their scientific identity. The proposal should therefore emphasise:

- → originality,
- → intellectual independence,
- → potential,
- → the beginning of a distinctive research programme.

Many applicants make the mistake of trying to appear older than they are. Instead, embrace your career stage. Panels are investing in future leadership. Not finished careers.

The Hidden Question

"If we fund this researcher today, where might they be in ten years?"

13.3 Consolidator Grant

This is perhaps the most delicate stage. Applicants are no longer judged primarily on potential. They are judged on momentum. Panels want to see evidence that an independent scientific programme is emerging. The proposal should therefore demonstrate continuity without becoming repetitive. The best Consolidator proposals often feel like the natural expansion of an already visible scientific trajectory.

➔ The Hidden Question

"Has this researcher moved beyond independence into scientific leadership?"

13.4 Advanced Grant

At this level, expectations change fundamentally. Panels already assume excellence. The discussion shifts towards influence. Applicants are expected not merely to solve problems, but to redefine them. The proposal should therefore avoid appearing as a collection of sophisticated experiments. Instead, it should resemble an intellectual manifesto.

➔ The Hidden Question

"Could this project change how an entire field thinks?"

➔ Key Takeaways

- → Each ERC grant evaluates a different form of excellence.
- → Adapt your narrative to your career stage.
- → Do not imitate more senior applicants.
- → The proposal should answer the hidden question appropriate to its grant type.

13A. STARTING OR CONSOLIDATOR?

Choosing the Right Grant in the Era of Overlapping Eligibility Windows

"Eligibility tells you what you may apply for. Scientific maturity should determine what you should apply for."

13A.1 A New Strategic Decision

For almost twenty years, choosing between a Starting Grant and a Consolidator Grant was simple. There was no choice. Your PhD date determined the scheme. Beginning with the ERC 2027 calls, that simplicity disappears. Researchers between five and ten years after the PhD are now eligible for both Starting and Consolidator Grants. The ERC intentionally gives applicants this flexibility and does not prescribe which grant they should choose. At first sight, this appears to be an advantage. In reality, it creates one of the most important strategic decisions in the entire application process. Many applicants immediately ask: *Which grant is easier to obtain?* This is the wrong question. The correct question is: **Which evaluation narrative best matches my scientific profile?**

13A.2 The Biggest Mistake

Applicants often think that the difference between Starting and Consolidator is simply seniority. It is not. The two schemes reward different stages of scientific development. A Starting Grant rewards the emergence of independence. A Consolidator Grant rewards the consolidation of a research programme. This distinction is subtle, but fundamental. The panel is not asking: "How old is this researcher?" It is asking: **"Where is this researcher in their scientific evolution?"**

13A.3 The Five-to-Ten-Year Dilemma

Suppose you are seven years after your PhD. You satisfy the eligibility criteria for both grants. How should you decide? There is no universal answer. Instead, consider five questions.

➔ Question 1

Have you already established a clearly identifiable research programme? Or are you still defining it?

➔ Question 2

Does your CV tell the story of scientific independence? Or does it already tell the story of scientific leadership?

➔ Question 3

When evaluators read your publication record, do they see a promising investigator? Or someone already shaping the direction of the field?

➔ Question 4

Is your proposal the first chapter of a larger vision? Or the next major step in an already coherent programme?

➔ Question 5

Which grant allows the panel to interpret your achievements more naturally?

This last question is often decisive.

13A.4 There Is No "Safer" Choice

Some applicants believe that Starting Grants are "easier." Others assume that Consolidator Grants carry greater prestige. Neither assumption is correct. Both schemes fund outstanding science. The issue is not prestige. It is fit. Submitting a proposal to the wrong scheme may unintentionally distort how evaluators interpret your achievements. Imagine a researcher with an exceptionally mature scientific profile applying for a Starting Grant. The panel may wonder why the proposal does not reflect the next stage of scientific development. Conversely, a researcher still establishing independence may appear less competitive if evaluated against the expectations naturally associated with a Consolidator profile. The strongest application is the one in which the proposal and the applicant's career stage reinforce each other.

➔ Evaluator's Perspective

"I am not comparing this applicant with an abstract definition of a Starting or Consolidator Grant. I am asking whether the proposal reflects where this researcher is in their scientific journey."

13A.5 Should You "Aim Higher"?

Many researchers are tempted to apply for a Consolidator Grant simply because they are eligible. This can be a mistake. Choosing Consolidator should never be viewed as a promotion. Nor should choosing Starting be viewed as a sign of weakness. The ERC does not reward applicants for selecting the more senior scheme. It rewards applicants whose scientific trajectory is coherent. The best choice is the one that allows your achievements to appear strongest.

13A.6 What About Researchers at 0 Years After the PhD?

Perhaps the most surprising consequence of the new rules is that researchers become eligible for a Starting Grant immediately after successfully defending their PhD. Many people immediately conclude: "Nobody can win an ERC Starting Grant immediately after finishing a PhD." I do not believe that this is the right way to think about it. The more interesting question is: **Under what circumstances could someone succeed?** The answer is simple. Not because they defended their PhD yesterday. But because, despite their very early career stage, they have already demonstrated an exceptional level of scientific independence. The PhD date is merely an eligibility criterion. Scientific maturity remains the evaluation criterion.

13A.7 Is Winning at 0 Years Realistic?

Yes. But it will almost certainly remain exceptional. Imagine a researcher who has:

- → produced genuinely field-changing work during the PhD;
- → clearly developed an independent scientific vision;
- → already established an original research direction that goes well beyond the doctoral project;
- → presents an ambitious proposal that naturally follows from this vision.

Such a profile could be competitive. Not because the panel lowers its standards. Quite the opposite. Because the applicant has reached an extraordinary level of scientific maturity unusually early. Panels are instructed to judge achievements in the context of career stage, taking into account what can reasonably be expected at that point in a researcher's career.

13A.8 Should Early-Career Researchers Apply Immediately?

This is perhaps the most difficult question. My advice would generally be: Do not apply because you are eligible. Apply because your idea is ready. The new eligibility window should be understood as an opportunity—not as an invitation to submit as early as possible. An outstanding proposal submitted one year later is usually stronger than a premature proposal submitted today. This consideration becomes even more important under the newer ERC application and resubmission rules, where an unsuccessful application may limit near-term opportunities to reapply.

Case Study

➔ Applicant

"I'm six years after my PhD. I could apply to either Starting or Consolidator. Which one would you recommend?"

➔ Discussion

Forget the calendar for a moment. Imagine removing the words *Starting* and *Consolidator* from the application. Now ask: **What story does my CV tell?** If your achievements describe the emergence of an independent scientist with enormous potential, the Starting Grant narrative may be more natural. If your achievements describe an established independent programme that is already influencing your field, the Consolidator Grant may provide the better narrative. The correct choice is the one that makes the evaluator think: **"This is exactly where this researcher should be."**

➔ Common Mistakes

- → Choosing Consolidator simply because it sounds more prestigious.
- → Assuming Starting is easier to obtain.
- → Letting eligibility determine strategy.
- → Applying immediately after the PhD simply because the rules now permit it.
- → Confusing scientific age with scientific maturity.

➔ Key Takeaways

1. The new eligibility windows give applicants flexibility—but also require strategic judgement.
2. The overlap between five and ten years after the PhD should be viewed as a **choice of narrative**, not a choice of prestige.
3. Scientific maturity matters more than chronological age.
4. Researchers immediately after the PhD **can** be competitive, but only if they already display exceptional independence and originality.
5. The best ERC application is not necessarily the earliest possible one; it is the one submitted when the scientific idea, the researcher's trajectory and the chosen grant scheme are perfectly aligned.

14. LEARNING FROM SUCCESS—AND FAILURE

Why Outstanding Proposals Are Sometimes Rejected

"Every rejection contains a lesson—but not always the lesson applicants imagine."

14.1 The Myth of the Perfect Proposal

Applicants often search for the perfect ERC proposal. It does not exist. Every funded proposal contains weaknesses. Every rejected proposal contains strengths. The difference often lies in which strengths and weaknesses dominate the evaluators' memory.

14.2 Weaknesses Are Not Equal

Panels easily forgive:

- → minor methodological imperfections,
- → typographical errors,
- → small inconsistencies.

They rarely forgive:

- → unclear scientific vision,
- → lack of originality,
- → confused hypotheses,
- → uncertain leadership.

Some weaknesses are cosmetic. Others alter the scientific narrative. Learn to distinguish between them.

14.3 Reading Evaluation Reports

Many applicants read evaluation reports defensively. This is understandable. But reports should be read diagnostically. Ask: What concern repeatedly appears? Often, different reviewers describe the same underlying problem using different words.

➔ **Example**

➔ **Reviewer A:**

"Objectives appear disconnected."

➔ **Reviewer B:**

"Overall vision lacks coherence."

➔ **Reviewer C:**

"The proposal seems fragmented."

Three comments. One underlying issue. Narrative.

14.4 Should You Resubmit?

Often, yes. History shows that many successful ERC grantees were unsuccessful in previous attempts. The important question is not: Was I rejected? It is: Have I understood why?

14.5 Success Leaves Clues

Funded proposals often share characteristics. Not identical science. Identical clarity. The central question appears obvious. The hypothesis feels memorable. The PI seems uniquely positioned. The scientific ambition feels exciting but believable.

Case Study

➔ **Applicant**

"The reviewers misunderstood my proposal."

➔ Discussion

Perhaps. But if several reviewers misunderstand the same idea, the proposal—not the reviewers—usually needs clarification. Communication is part of scientific excellence.

➔ Key Takeaways

- → Rejection does not necessarily imply lack of excellence.
- → Learn from recurring comments.
- → Distinguish narrative weaknesses from technical ones.
- → Clarity can be improved.
- → Resubmission is often part of the ERC journey.

15. THINKING LIKE AN ERC SCIENTIST

The Mindset Behind Frontier Research

"Long before you write an ERC proposal, you begin thinking like someone who could receive one."

15.1 The Proposal Begins Years Earlier

Perhaps the most important idea in this handbook appears at the very end. ERC proposals are rarely created in six months. They emerge from years of scientific curiosity. Every paper you publish, every collaboration, every conference, every question you decide not to pursue, gradually shapes your scientific identity. The proposal simply reveals it.

15.2 Curiosity Before Funding

Many researchers begin with a funding opportunity. ERC researchers often begin with a scientific obsession. Funding follows curiosity. Not the reverse. The strongest proposals rarely ask: What can I write for this call? Instead, they ask: What question have I been unable to stop thinking about?

15.3 Intellectual Courage

Frontier research requires a particular kind of courage. Not confidence. Not optimism. Curiosity. The willingness to explore questions whose answers remain genuinely uncertain. Incremental science expands knowledge. Frontier science sometimes changes its direction.

15.4 Scientific Identity

Throughout this handbook we have repeatedly returned to one concept: identity. Applicants often ask: What topic should I propose? A more useful question is: What scientific problem do I want my name to become associated with? That answer gradually defines a career.

15.5 The Legacy Question

Imagine the project succeeds beyond expectations. Now ask: What sentence might appear in a textbook twenty years from

now? Not because of prestige. Because of knowledge. If no meaningful scientific change can be imagined, perhaps the question is not yet ambitious enough.

Case Study

➔ Applicant

"What is the single most important piece of advice you would give an ERC applicant?"

➔ Answer

Do not try to write an ERC proposal. Become the scientist who naturally has one to write. The proposal is not the objective. It is the consequence.

➔ Final Reflections

The ERC is often described as a funding programme. It is also an idea. that science advances when researchers are given the freedom to ask difficult questions, challenge accepted assumptions, and pursue ambitious visions whose outcomes cannot be guaranteed. Writing an ERC proposal is therefore not merely an administrative exercise. It is an act of scientific imagination. The best proposals do not simply promise discoveries. They invite evaluators to imagine a future in which our understanding of the world has fundamentally changed. And perhaps that is the real question every applicant should ask before writing the first sentence:

"If this project succeeds, how will scientists think differently?"

If you can answer that question clearly, convincingly and honestly, you have already taken the first step towards writing an outstanding ERC proposal.

16. THE ARCHITECTURE OF AN OUTSTANDING ERC PROPOSAL

How Every Part of the Proposal Reinforces Every Other Part

"An ERC proposal is not a collection of sections. It is a single scientific argument told from different perspectives."

16.1 The Illusion of Separate Sections

Applicants often think in terms of the template. There is the Extended Synopsis. Then the CV. Then the Track Record. Then the methodology. Then the budget. Then the resources. Each section is written almost independently. This approach is understandable. It is also one of the most common reasons why otherwise excellent proposals feel fragmented. Evaluators never experience your proposal this way. They experience one continuous scientific narrative. Every section changes how they interpret every other section. The proposal behaves less like a collection of documents than like a living organism. Its strength depends not only on the quality of each part but on the coherence of the whole.

16.2 The Proposal as a System

Imagine removing one chapter from a novel. The remaining chapters still exist. But the story changes. The same happens with an ERC proposal. Suppose the methodology is excellent. If the hypothesis is weak, the methods appear excessive. Suppose the CV is exceptional. If the research question is incremental, the applicant appears underambitious. Suppose the scientific vision is revolutionary. If the requested resources appear unrealistic, credibility weakens. Every component influences the interpretation of the others. Nothing stands alone.

16.3 The Principle of Alignment

Outstanding proposals display remarkable alignment. Consider the following chain.

The scientific question determines:

↓

the hypothesis.

The hypothesis determines:

↓

the objectives.

The objectives determine:

↓

the methodology.

The methodology determines:

↓

the expertise required.

The expertise determines:

↓

the team.

The team determines:

↓

the budget.

Everything follows naturally. The evaluator never asks, "Why is this here?" Because every decision appears inevitable.

16.4 The Five Layers of Conviction

After years of observing ERC proposals, I think every successful application convinces evaluators on five different levels.

Layer 1 — The Problem

Is the scientific question genuinely important?

Layer 2 — The Idea

Does the proposal introduce a fundamentally new way of thinking?

Layer 3 — The Investigator

Is this the right person?

Layer 4 — The Strategy

Can the proposed work answer the question?

Layer 5 — The Transformation

If successful,

how will science look different?

Only when all five layers align does a proposal become exceptionally persuasive.

16.5 Every Page Should Answer the Same Question

Throughout the proposal, evaluators repeatedly ask themselves: **Why should the ERC fund this project?** Notice something interesting. Every section answers that question differently. The Extended Synopsis answers: Because the scientific vision is transformative. The CV answers: Because this investigator can lead it. The methodology answers: Because the approach is convincing. The budget answers: Because the requested resources are necessary. The proposal is therefore one argument, expressed in several languages.

➔ Evaluator's Perspective

"I should never feel that I am reading different documents written by different people."

16.6 The Echo Principle

One characteristic repeatedly appears in outstanding proposals.

Ideas echo.

Not through repetition,

but through reinforcement.

The central hypothesis introduced in the synopsis quietly reappears:

- → in the objectives,
- → in the methodology,
- → in the figures,
- → in the discussion of risks,
- → in the concluding paragraph.

Each appearance deepens confidence.

The evaluator gradually develops the reassuring feeling that everything belongs together.

16.7 Avoiding Internal Contradictions

Many proposals contain hidden contradictions. For example: The proposal claims to be highly interdisciplinary. The team includes only one discipline. The proposal describes revolutionary ambition. The methodology consists entirely of standard approaches. The PI claims complete independence. The scientific strategy depends almost entirely on collaborators. The proposal describes transformational science. The expected outcomes are incremental. These inconsistencies rarely arise intentionally. Most appear because different sections are written at different times. One of the final editing tasks should therefore be to search specifically for contradictions.

Case Study

➔ Applicant

"I have revised my synopsis several times. Should I also revise the other sections?"

➔ Discussion

Almost certainly. Every major change to the scientific vision should trigger a review of:

- → objectives,
- → methodology,
- → figures,
- → resources,
- → team,
- → budget,
- → CV emphasis.

A proposal evolves as a whole. Not chapter by chapter.

16.8 Reading the Proposal Backwards

Here is an exercise I often recommend. Start with the budget. Could you reconstruct the scientific objectives? Now read the methodology. Could you infer the hypothesis? Read the CV. Could you predict the project? If every section points naturally towards the same scientific vision, the proposal is probably well integrated.

16.9 The Orchestra

Perhaps the best metaphor is not architecture. It is music. An orchestra contains:

- → strings,
- → brass,
- → percussion,
- → woodwinds.

Each section performs a different function. None attempts to play every melody. Together, they produce something impossible individually. An ERC proposal works the same way. The synopsis should not explain every methodological detail. The CV should not defend every budget item. The methodology should not repeat the literature review. Each section contributes its own voice. The evaluator hears one composition.

Good Example vs Weak Example

➔ Weak

Each section appears individually well written. However, the proposal gradually feels repetitive, fragmented, and disconnected. Evaluators finish reading with uncertainty about the central message.

➔ Better

Every section introduces new information while reinforcing the same scientific vision. The proposal becomes progressively stronger. By the final page, the evaluator has encountered the central idea several times, from different perspectives, without ever feeling repetition.

➔ Common Mistakes

- → Writing sections independently.
- → Changing the hypothesis without updating later chapters.
- → Allowing the budget to contradict the scientific ambition.
- → Introducing objectives that never reappear.
- → Treating the proposal as a template rather than a coherent argument.

NCP'S TIPS

- → Think about coherence before detail.
- → Let every section reinforce the central vision.
- → Remove internal contradictions.
- → Ensure every requested resource supports an objective.
- → Read the proposal as a single story.

➔ Questions to Ask Yourself

- → Does every section answer the same scientific question?
- → Would an evaluator describe my proposal consistently after reading any section?
- → Is there anything in the proposal that feels surprising for the wrong reason?
- → Does every objective contribute to the same vision?
- → Have I written one proposal—or several documents?

One Minute Before You Turn the Page

Close your proposal. Imagine someone has accidentally separated all its sections and mixed them with those from another application. Would an evaluator still be able to recognise that they belong together? Would your CV naturally lead to your hypothesis? Would your figures naturally match your objectives? Would your budget naturally follow your methodology? If not, your proposal may still be scientifically excellent. But it is not yet architecturally coherent. And coherence is one of the invisible qualities that distinguish good ERC proposals from unforgettable ones.

➔ Final Thought

If I had to summarise the entire handbook in a single sentence, it would be this:

An outstanding ERC proposal does not convince evaluators one section at a time. It convinces them by making every section point to the same transformative scientific idea.

17. WHEN SHOULD YOU APPLY FOR AN ERC PLUS GRANT?

Choosing Between ERC Plus and the Classical ERC Schemes

"ERC Plus is not a larger ERC grant. It is a different scientific proposition."

17.1 A New Funding Philosophy

The introduction of the ERC Plus Grant marks one of the most significant developments in the history of the ERC. Rather than extending the existing Starting, Consolidator or Advanced Grant schemes, ERC Plus introduces a new funding philosophy: **substantial funding for up to seven years** aimed at researchers ready to pursue exceptionally ambitious long-term frontier research. The initiative forms part of the broader **Choose Europe for Science** strategy. This immediately raises a strategic question. Should applicants choose ERC Plus simply because it offers more funding and a longer duration? In our view, absolutely not.

17.2 Bigger Is Not Better

One of the first instincts many applicants will have is: "If I am eligible for ERC Plus, why shouldn't I apply?" Because the question is not about eligibility. It is about scientific fit. The classical ERC grants ask whether a single frontier project can transform a field over approximately five years. ERC Plus asks something more demanding. Can this researcher build a research programme whose scientific ambitions require a longer time horizon, broader resources and a different scale of intellectual investment? The distinction is qualitative, not quantitative.

17.3 A Project or a Programme?

Perhaps the simplest way to think about the difference is this. A classical ERC grant funds an extraordinary project. ERC Plus is expected to support an extraordinary research programme. Projects answer questions. Programmes create new scientific landscapes. This does not mean that ERC Plus is necessarily "more ambitious." It means that the ambition unfolds over a different temporal scale.

17.4 Should Every Advanced Grant Applicant Consider ERC Plus?

No. Many transformative scientific ideas are perfectly suited to the classical ERC model. Longer funding is not automatically advantageous. The additional time should correspond to additional scientific necessity. If the central scientific questions can realistically be answered within a traditional ERC project, then a classical grant may remain the stronger choice.

17.5 What Kind of Science Fits ERC Plus?

Although the programme is still new, one can already identify the type of research that appears particularly well suited. Projects that:

- → require exceptionally long observation periods;
- → build entirely new research infrastructures;
- → establish major scientific platforms;
- → integrate multiple generations of discovery;
- → seek to redefine an entire research landscape rather than answer one major question.

These are examples of programmes whose intellectual trajectory naturally extends beyond five years.

Case Study

Applicant

"My proposal could be expanded to seven years if I added more objectives."

Discussion

That is almost certainly the wrong approach. Duration should arise from scientific necessity, not from additional work packages. The panel should feel that the research programme naturally requires a longer horizon.

17.6 The Strategic Question

Ultimately, applicants should ask themselves:

Am I proposing a frontier research project—or am I proposing the foundation of a long-term scientific programme?

The answer to that question is likely to be more informative than any comparison of budgets or project duration.

Key Takeaways

- → ERC Plus is not simply a "larger" ERC grant.
- → More funding and a longer duration are not reasons, by themselves, to choose ERC Plus.
- → Think in terms of research programmes, not just projects.
- → Choose the scheme that best matches the scientific architecture of your vision.
- → The best application is always the one whose scope, ambition and funding instrument are perfectly aligned.

CONCLUSION

Preparing an ERC proposal is a demanding intellectual exercise. It requires much more than translating a research idea into a funding application. It challenges researchers to reflect on the significance of their scientific questions, the originality of their vision, the coherence of their research strategy and, ultimately, on the contribution they aspire to make to their field. Throughout this guide, we have emphasised that an ERC proposal should not be regarded as a collection of administrative documents or a series of independent sections to be completed. It is a single scientific narrative that brings together an ambitious research idea, a convincing strategy and a Principal Investigator capable of leading frontier research. Every part of the proposal should contribute to answering one fundamental question:

Why should the European Research Council invest in this researcher and in this idea?

There is no universal formula for writing a successful ERC proposal. Scientific excellence cannot be reduced to a template, nor can originality be generated by following a checklist. The diversity of funded projects across all scientific disciplines demonstrates that there is no single profile of a successful applicant and no single way of presenting an outstanding idea. Nevertheless, the proposals that succeed often share common characteristics. They address questions that truly matter. They challenge current knowledge rather than extending it incrementally. They communicate a clear scientific vision, acknowledge uncertainty without being constrained by it, and demonstrate that the Principal Investigator has both the creativity and the leadership to transform an ambitious idea into new knowledge. Applicants should also remember that writing an ERC proposal is never a wasted effort. Even when a proposal is not funded, the process of developing a coherent research vision, refining scientific questions, identifying weaknesses and articulating long-term objectives often strengthens future research programmes, collaborations and funding applications. Many ERC grantees succeeded only after revising and substantially improving earlier unsuccessful proposals. Perhaps the most important message of this handbook is that the ERC does not simply fund excellent science; it invests in researchers who have the vision and determination to push the boundaries of knowledge. The proposal is therefore not only an application for funding. It is an opportunity to explain why a particular scientific question deserves to be pursued, why now is the right time to pursue it, and why you are the person best placed to lead that journey. We hope that this handbook has helped to clarify the principles behind ERC evaluation, to demystify some of the most common misconceptions, and to provide practical guidance for developing a competitive proposal. Whether you are preparing your first Starting Grant application, deciding between a Starting and a Consolidator Grant, considering a ERC Plus proposal, or supporting applicants as a research manager or mentor, our hope is that the ideas presented here will encourage you to think critically, write clearly and aim boldly. Frontier research begins with curiosity. It grows through creativity, perseverance and intellectual courage. The ERC was created to support precisely this kind of science. We wish you every success in developing your ideas and, above all, in pursuing the discoveries that inspire them.

APPENDIX A

References and Useful Resources

This handbook should be read together with the official documentation published by the European Research Council (ERC), the European Research Council Executive Agency (ERCEA) and the European Commission. Applicants are strongly encouraged to consult the most recent versions of these documents before preparing and submitting an application.

A.1 Essential Official Documents

European Research Council (ERC)

- → ERC – Apply for a Grant
- → ERC Work Programme
- → ERC Document Library
- → Funding & Tenders Portal

A.2 Proposal Preparation

Applicants should carefully read the following documents for the specific call to which they intend to apply:

- → Information for Applicants
- → ERC Work Programme
- → Proposal Template (Part A, Part B1 and Part B2)
- → Rules for Submission and Evaluation
- → Panel Descriptors
- → Evaluation Forms
- → Guide for Peer Reviewers
- → Ethics Guidance
- → Gender Equality Plan Guidance (where applicable)

All of these documents are available through the ERC Document Library.

A.3 Understanding the Evaluation Process

The ERC provides several excellent resources explaining how proposals are evaluated.

Particularly recommended are:

- → ERC Evaluation Process Videos
- → ERC Interview Preparation Videos
- → ERC Ethics Guidance Videos

These videos complement the written documentation and provide useful insights into the evaluation process directly from ERC and ERCEA staff.

A.4 ERC News and Updates

Because ERC rules evolve over time, applicants should regularly consult the official news pages. Recommended resources include:

- → ERC News
- → ERC Events and Information Sessions
- → ERC Support and Contact Page

These pages contain updates on new Work Programmes, changes to eligibility rules, webinars, frequently asked questions and applicant support.

A.5 National Contact Points (NCPs)

Applicants are strongly encouraged to contact their National Contact Point (NCP) for individual advice on:

- → eligibility;
- → panel selection;
- → proposal preparation;
- → interpretation of ERC rules;
- → interview preparation;
- → administrative aspects of the application.

NCP services are free of charge and available in every EU Member State and Horizon Europe Associated Country. Contact details are available through the European Commission and national funding agencies.

Italian ERC NCPs: erc@apre.it APRE-ERC

Full list of ERC NCPs

Since September 2025, a network project of ERC NCPs was launched. The project was funded by the ERC. mERCury project is a network of National Contact Points aiming to provide guidance, practical information and personalized support on all aspects of participation in the European Research Council (ERC) within Horizon Europe. Our mission is to strengthen the support system for researchers and institutions between national contact points across Europe. mERCury website

A.6 Useful Background Reading

Applicants wishing to better understand the philosophy of frontier research and research excellence may find the following resources useful:

- → Frontier research policy documents published by the ERC Scientific Council.
- → ERC Annual Reports.
- → ERC-funded project portfolio and success stories.
- → Scientific publications and lectures by ERC grantees in their respective disciplines.

These resources provide valuable insight into the diversity, ambition and originality of research supported by the ERC.

A.7 Before You Submit

Before pressing the "Submit" button, verify that you have consulted the latest versions of:

- → ERC Work Programme
- → Information for Applicants
- → Proposal Template
- → Rules for Submission and Evaluation
- → Panel Descriptors
- → Ethics Guidance
- → Funding & Tenders Portal instructions

Finally, remember that this handbook is intended to complement—not replace—the official ERC documentation. Whenever there is any difference between the recommendations contained in this guide and the official ERC documents, the official ERC documentation always prevails.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