**Writing and ERC Grant**

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***Disclaimer***

*This document contains personal views gained partly from being a member and chair of the ERC Starting Grants PE6 panel. When I write “we”, I mean “I”. Nothing in this document represents in any way the views of the ERC.*

# Part I. What are we looking for?

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All in all, we want to give you an ERC starting grant if we find that:

1. You are a highly promising early-career researcher.

Your publication record shows a focus on quality. It is coherent and driven by ambitious ideas.

We expect that you have published in top conferences and/or journals (whatever “top” means in your specialization). But whether you have published 10, 15, or 30 papers, is less relevant. Your self-selected list of top-5 or top-6 articles says a lot about you.

We pay attention to evidence of (multi-faceted) impact.

We expect to see a reasonable number of citations. If you are almost not cited at all, we wonder why your advancements or ground-breaking results have not been recognized (maybe they’re too recent)? But beyond a certain threshold (dependent on the specialty and academic age) the number of citations does not matter (much).

Notable citations to your work are worth highlighting, e.g. tell us what others have written about your work.

We value applicants who have contributed to tools, datasets, or other reusable artefacts that are (widely) used in academia and/or industry.

We usually pay attention to awards and recognitions, such as a doctoral thesis awards or a best paper award in a reputable venue. Talks at reputable venues or other indicators of esteem from a community of researchers or practitioners are a bonus.

We expect you to be an independent researcher or that you have taken initial steps towards gaining independence and this ERC grant will help you to attain full independence. Having successful (international) collaborations is a big plus.

Naturally, we expect that you have expertise relevant to the proposal and that your show that your previous research has prepared you for your ERC project.

We value professional experience in R&D positions, especially if this experience has given you a unique insights or expertise relevant to the project.

1. You have found a fundamental gap or limitation in the state of the art in your field (or close to your field).

The gap you have identified is important in a community of researchers and/or practitioners.

There is clear potential gain if this gap is addressed. The “potential gain” can be purely academic, or it can be potential application in industry, insights to policy-making, or potential impact on society. If the project is successful, it will trigger a new line of fundamental and/or applied research.

Addressing the gap is scientifically challenging either because it requires connecting concepts and methods from different disciplines, or because the problem is technically hard and cannot be tackled with existing methods.

1. You have a fundamentally new idea or a new perspective to address the identified gap.

The idea is ambitious (high-risk, high-gain) and will potentially lead to a breakthrough – a new set of insights, a new method that will allow others to tackle a (broad) set of problems.

The idea(s) is/are concrete and specific. Convincing examples of how the idea would work are worth gold.

For example, saying that I am going to apply machine learning (e.g. neural networks) is not concrete. What specific methods? What would be the inputs/outputs? Why this idea would work in this setting?

Tells us: I will transform this input in this way to produce this representation, which I will encode in this way to give to a supervised machine learning technique / I will collect ground-truth in this way, I will try algorithms X and Y because of they have these and these advantages/properties.

The idea (or set of ideas) is clearly distinct from what you or other researchers have been pursuing so far.

You have clear criteria to assess the success of your idea(s). It’s great if your objectives are measurable and represent an order-of-magnitude improvement (not a small “delta improvement”).

1. Your idea(s) is/are feasible.

You understand what are the risks involved in pursuing your idea. You have fall-back options or stop-gap approaches that ensure you will deliver something significant – maybe you won’t achieve your big objectives, but you will get somewhere during the project.

You provide a clear decomposition of your ideas into tasks or work-packages, each with a clear and credible approach or set of approaches. In part A of the proposal this may be just sketched, but it is refined in part B, where there are detailed ideas given for each task or work-package (or “research vector” or whatever you call it) and there is a realistic timeline and detailed resource allocation.

In other words: You need to have an ambitious, high-risk/high-gain objective(s) that if achieved would lead to a breakthrough. The objectives should be supported by a novel and feasible overarching idea (or small set of ideas), which should then be followed up with lots of other good and concrete ideas, all fitting together.

Good luck!

End of Part I

# Part II. Writing

##

## Abstract

* I suggest three paragraphs
* First one gives the context (problem area) and says why this area is important.
* Second one articulates the fundamental limitations of existing approaches (a “gap”) and states what new idea(s) – preferably one but maybe 2-3 – are you putting forward to address this gap.
* Third one explains why this idea is high-gain and high-risk, and why it has a good chance of leading to significant/considerable advances in the state of the art. Towards the end of this paragraph you can also give an idea of the key outcome(s) of the project and the magnitude of the improvements with respect to the state of the art. A statement “we will achieve 50%-80%-90%” improvements is a good punchy line, but it can also be qualitative improvement, or a new result, a new theory, a new perspective. But beware: your targeted “improvement” should be feasible.

And also: Feel free to add a small diagram/pictogram to your front-page, between the title and the abstract. One picture can be worth 1000 words while fitting in what would otherwise be white space…

## Part A. Synopsis

* 5 pages + references + CV + grants + early achievements
* Remember you are writing this synopsis for the panel members. There are only about 15 of them, so they might be generalists w.r.t. your proposal. Check who the panel members have been in previous years. What would they think?
* **A.1 Context and state of the art**
* Here you should explain your problem area to a generalist audience (the panel members).
* You should cite the main theories/results in the field. You should highlight what are the fundamental assumptions or the key ideas that previous work have pursued, so that you can later explain what is new in your proposal.
* **A.2 Objectives and challenges**
* State what are the fundamental limitations, the fundamental gap in the state of the art that you are addressing.
* Then chain this with a statement of what is/are the main new idea(s) that you are bringing to the table. Highlight the novelty of your idea(s), contrast it to previous methods. Be selective with your arguments.
* Then spell out the objectives of the proposal. Try to convince why these objectives are ambitious (high-risk, high-gain).
* If possible make your objectives measurable or otherwise state how to assess the successful achievement of your objectives.
* Beware: do not say “I am going to achieve this, or improve this by 90%” if you do not articulate later in the proposal how these improvements will be achieved. You should walk the fine line between ambition and feasibility.
* If the potential applications of your proposed research are not self-evident, try to put forward a “killer application” (or multiple ones).
* In any case, it should be obvious what can be done using the concepts/approaches you will propose that cannot be done using existing concepts/approaches.

Example taken from Daniel Marx’s PARAMTIGHT project:

The theory of NP-hardness gives evidence that exponential running time cannot be avoided for many problems of interest. From the practical point of way, the classical role of complexity theory is to use NP-hardness to show that it is unlikely that the problem can be solved in polynomial time. [..]

This setup has been the standard framework since the 70s and literally thousands of problems were analyzed this way. However, the limitation of this approach is that for NP-hard problems (which are quite common among the practical problems that need to be solved) we do not get any useful message on how to handle them. […]

Instead of focusing on a certain fixed type of yes/no question (*“Is problem X polynomial-time solvable?”, “Is problem X NPhard?”*), we have to set up one or more dimensions that measure the performance of the algorithm (such as running time or quality of the solution), and aim for an understanding of what the best possible algorithm is along these dimensions. Every algorithm can be considered as a point in the conceptual space defined by these dimensions.

Our goal is to map the region of this space that can be realized by algorithms. Classical complexity questions investigate particular points of this space. While there are good reasons why these particular points are investigated, these questions give access only to a very restricted part of the big picture. […] Instead of investigating particular points, we want to achieve results saying that *“For problem X, techniques A, B, and C* *contain all the relevant algorithmic ideas and there is no way to improve them.”*

* **A.3 Organization and methodology**
* Define 3-4 high-level tasks (some call it “WP”, doesn’t matter much, I prefer tasks). Remember this is a high-level decomposition of the project. Explain the scope of each task/WP, but don’t spend too much space in details.
* Focus on ideas, not on project management or bureaucracy. In part A, you might not have space to put a timeline.
* If applicable, consider including one or two moonshots in your research plan – i.e. a very difficult application scenario that you will tackle/implement using the methods you will develop – e.g. using the fundamentally new machine translation algorithms I will develop, I will build a translation tool from English to Estonian which will make 50% less mistakes on a given benchmark while requiring 50% less resources for training. Beware: the moonshot(s) should be credible in light of the fundamental limitations/assumptions and the bright new ideas you will develop.

The composition of the research team should be briefly described (you, postdocs, etc.).

Highlight your commitment to the project and preferably make it 70-80% at least if you can? The minimum is 60% FTE, but are you minimally committed to an ERC project?

* **A.4 Risk assessment**
* For each task/WP, explain what are the key risks. The project has to be risky. But also explain how will you minimize risk. What are your fallback options? What is the minimum you will achieve.
* A.5 Expected impact
* Who will gain from the output of the project (it could be people in academia or outside, industry, society, etc.)? What will the output of the project enable them to do that they cannot do with the existing state of the art? What fundamental new insights you will bring?
* **A.6 Team**
* This can be short. Just explain what will be your percentage involvement in the project and how it would fit with other duties or major projects if you have any. How many PhD students/postdocs/other people will be involved. Are their names known? And very briefly what parts of the project will each of them focus on?
* **B. CV**

This is rather standard. But use it to highlight key achievements or recognitions.

You have to mention current and previous career gaps.

Explain briefly if there is any overlap between those projects and this one and why this overlap is clear, and not an issue at all.

* Remember to state who was your PhD advisor.
* **C. Early achievements record**
* **C.1 Achievements.**
* Structure this part in a thematic manner. Highlight the main chunks of research you have undertaken. Point out to key achievements. Show coherence in the way you present your achievements.

Also: show independence. Show you have come up with your own ideas and that you are passionate about them.

**C.2 Publications**

Be very carefully in selecting them. Focus on top publications that you have led. Highlight citations or anything else that shows impact.

+ sections for invited talk, participations in program committees, editorial board or other academic roles. And prizes and awards…

## Part B. Full Proposal

15 pages + references

This one will be read by specialists in addition to panel members. Be very specific now…

1. State of the art and objectives

You need to expand the state of the art review and make it bullet-proof so that the specialists can see you understand the field very well.

You need to argue that there is a fundamental gap. That existing approaches (including maybe your previous work) make some fundamental assumptions that limit it in some way.

Or else argue that the problem(s) you are targeting has/have not been addressed in this form before (but it should then be clear why not, or else the reviewers will wonder why has nobody tackled it before).

Examples are wonderful – they make things concrete. Use an example to show that existing methods do X on a given example, but they do not achieve Y. Explain what is the fundamental limiting property of existing methods that make it unable to achieve Y.

This argumentation should lead you to the (detailed) description of your objectives.

Again, make sure the objectives are measurable, or at least, that the “success criteria” are clear.

Decompose your objectives either into sub-objectives, or research questions, or problems/ challenges.

1. Methodology

The methodology should be significantly enhanced.

Start by discussing the overarching idea(s) and why these ideas are novel and would lead to breakthroughs. Show that you understand very well the obstacles and potential pitfalls.

Next, you need to show that you have a clear idea of each task/WP (or research stream or research vector) in your project decomposition. Make it clear that the tasks are challenging, but also achievable.

Make sure there is a clear link between the tasks/WPs and the objectives.

If your research requires access to some datasets, make sure it’s clear to the reviewers you have access to these datasets.

You can then proceed to present a timeline followed by a description of the resources, with clear roles/responsibilities for each researcher (postdoc or PhD student).

A typical proposal would request 1-2 postdocs and 2-3 PhD students. Possibly also a scientific programmer to develop large-scale prototypes or conduct experiments.

Other resources should be justified. Don’t ask for a lot of travel budget (over 80k) unless you justify it. If you plan to invite visiting researchers, who?, For what period, why?

Think about asking for publication costs (open-access).

If applicable, don’t forget to discuss any requires ethics clearances and show you are aware and there are clear processes in your institution for it.