Funder: European Commission Scheme: ERC Consolidator Grant 2018

Compiled Feedback from ERC evaluators-Consolidator Grants

1 Research Project: Ground breaking nature, ambition, feasibility.

Criteria:

Ground-breaking nature and potential impact of the research project

- *To what extent does the proposed research address important challenges?*
- *To what extent are the objectives ambitious and beyond the state of the art-SOTA- (e.g. novel concepts and approaches or development across disciplines)?*
- *To what extent is the proposed research high risk/high gain?*

Scientific Approach

- *To what extent is the outlined scientific approach feasible bearing in mind the extent that the proposed research is high risk/high gain (based on the Extended Synopsis)? (This ranks highly in evaluators comments)?*
- *To what extent are the proposed research methodology and working arrangements appropriate to achieve the goals of the project (based on the full Scientific Proposal)?*
- *To what extent does the proposal involve the development of novel methodology (based on the full Scientific Proposal)?*
- *To what extent are the proposed timescales and resources necessary and properly justified (based on the full Scientific Proposal)?*

Common Strengths

Research Project

- The objectives are ambitious, and presented as clear hypotheses. Project is addressing one of the major challenges of the coming years.
- Project goes beyond the state of the art/ will fill a gap in current SOTA.
- The topic is timely with very strong aims. Utilises pioneering approaches.
- The project addresses a fundamental question largely left unexplored.
- This is ground breaking and original research. Truly novel ideas with high potential gain.
- The project is multidisciplinary and if successful may yield important new insights.
- The outreach activities associated with the research project (e.g. summer school for children) are beyond the state of the art and represent a novel approach.
- The project pays attention to a long-standing problem both in scientific practice and in the way the value of scientific knowledge is perceived/established.
- The goal of the research is realistic, and has the potential of being extendable to other areas of scientific inquiry. It acknowledges the complexity of scientific practice and, hence, the need for a new and more sophisticated assessment of its nature.
- The results of this project will definitely advance our understanding of scientific practice.
- The work is challenging and has high feasibility

- The individual experimental steps are clearly laid out and presented in a convincing manner, including the discussion of potential problems and how they will be addressed
- The use of powerful new techniques will drive the field forward

Scientific Approach

- Proposal is carefully designed and thus feasible. Promising preliminary results.
- The scientific approach is very carefully laid out and is refreshingly open-minded to alternative methods and theoretical approaches.
- Project is feasible because if experience if the PI, access to expertise/equipment essential to the project and the clarity of the experimental plan.
- The project description has been formulated as questions, which is practical to understand the hypothesis.
- Impressive: focused approach, in which specific questions are posed to address important question and where the appropriate models and techniques are chosen to answer the question.
- An excellent to outstanding project, extremely well presented, clear goals and objectives.
- The scientific approach is sound and clearly addresses all challenges raised in the proposal.
- The methodology and organization of the research schedule and team is very good and well explained.
- The qualitative approach seems perfectly appropriate. One wonders if there may be quantitative indicators that could also be used to test the same hypotheses, which may add a compelling dimension.
- The research methodology is appropriate to achieve the goals of the project. The proposed collaborations are also very promising.
- The approach to the research is well informed. It is coherently based on, and it extends, the PI's previous work.
- The PI has an excellent action plan with each stage addressing clearly defined goals that correspond to each of the major questions/issues the research addresses.
- The project contains a healthy balance of "safe" and "high risk / high gain" components.
- The proposed timescale and resources necessary are appropriate and properly justified.
- The PI is one of the ground-breaking scholars of some of the methodologies proposed
- Proposal is very convincing and intriguing. This is accomplished by including both foundational/fundamental research and also building on existing knowledge/tools.

<u>Common Weaknesses</u>

Research Project

- Missing ground-breaking aspects, lack of novelty. It’s very low gain.
- The project is in most parts a continuation of current work.
- Just a collection of existing approaches with no clear goals. Overall design is conservative.
- Description of work is reasonable but does not clarify the underlying hypothesis and how this will be tested.
- The proposal does not discuss or identify risks of not succeeding to provide high gain answers to the posed questions.
- Primary aspect of the research project seem to be flawed or not well explained.
- Extended synopsis doesn’t instil confidence that the project will be successful as it is scant in ideas on how the SOTA will be improved.
- Vague objectives, lack of focus/ambition/consistency.
- Aims are very ambitious but very broad and this and this may not lead to a breakthrough.
- Impossible to determine what the impact of the project maybe.
- Objectives are ambitious but not well integrated with each other.
- Impact of the goal is very indirect and unclear how its success will be measured.
- Lack of mentioning major ongoing work by others OR only referring to own work. The applicant seems to ignore or is not aware of the controversy in the field.
- Proposal reads more like a compendium of ideas rather than a ground breaking idea.

- Exaggerating approaches as “critical” or “novel” when that is not the case.
- The proposal is missing expected outcomes and metrics of success.
- Ignorance towards state of the art and work performed by others/does not fully reflect the SOTA
- Missing high risk/high gain strategy or these are not discussed.
- Innovation of the project not convincing OR project will not result in any breakthroughs.
- The proposal has lots of nice ideas and seems to have potential, but its purpose is not well articulated.
- Difficult to see the big picture of the work planned, projects appears as a list of nice & complex approaches under a very wide topic.
- No previous expertise in the research area the PI is proposing to pursue, so the project appears less credible.
- Approaches to be used have already been established by others.
- High reward project but risks appear to be underestimated.

Scientific approach

- Unconvincing methodology/ not novel/does not clearly describe the technical dimension of the problem. Hypothesis not convincing.
- Approach is very high risk as there is very little preliminary data to support the hypothesis.
- Scientific approach is weak/inappropriate. In best case scenario improvements will be incremental. Feasibility of some of the experiments is worrying.
- Approach is very ambitious for a single lab to complete. Some timescales are unrealistic.
- Timeliness of the project is not well justified.
- Could have included more details on the experiments in B1.
- A timeline of the proposed project is not given, and the enumeration of the specific tasks assigned to the post doc personnel is also missing.
- Missing risk assessment/no contingency plans.
- Proposed research is not feasible.
- No convincing preliminary data available to establish feasibility.
- Not clear where data is coming from or how it will be collected.
- The scientific proposal uses conventional methodology and is not very convincing.
- Not considered a high risk project.
- Some weak aspects in the scientific approach
- Applicant has no previous experience on a vital aspect of the methodology which is not trivial
- The scientific approach lacks appropriate case studies on which the proposed approaches will be tested.
- PI lacks expertise and it involves too many external actors with crucial roles at key parts.
- Some of the suggestions made by the PI are simply not true (based on work by others).
- Proposal does not develop novel methodologies.
- I would have expected to see a detailed description of the milestones and the associated time stamps but this is completely missing in both documents.

2 Principal Investigator: *Intellectual capacity, creativity and commitment*

Criteria

For each of the statements below, reviewers were asked to choose one of the following four responses: Outstanding /Excellent / Very good / Non-competitive.

Intellectual capacity and creativity

- *To what extent has the PI demonstrated the ability to propose and conduct ground-breaking research?*
- *To what extent does the PI provide evidence of creative independent thinking?*
- *To what extent have the achievements of the PI typically gone beyond the state of the art?*

Commitment

- *To what extent does the PI demonstrate the level of commitment to the project necessary for its execution and the willingness to devote a significant amount of time to the project (min 30% of the total working time on it and min 50% in an EU Member State or Associated Country) (based on the full Scientific Proposal)?*

Common Strengths

- The PI demonstrates an extremely high level of commitment and determination.
- The PI has demonstrated his capacity of creative independent thinking
- The applicant is an excellent scientist trained in outstanding laboratories, received several awards and their work is well recognised among their peers.
- The applicant is very close to excellent, with a number of international fellowships.
- The PI is certainly one of the few scientists in the world with the right expertise to carry out the project and bears an excellent track record.
- The publication/track record is excellent and provides evidence of creative and independent thinking (there are only very few scholars of that career stage that have more impressive publication records).
- The professional and editorial activities of the PI are impressive.
- The PI has been invited to give several plenary lectures at prestigious international conferences and has given a substantial number of talks at several international departments.
- The PI is co-editor in chief of one of the two leading journals in the area.
- The amount of invitations that the PI receives and the prizes obtained are proof that he enjoys a high level of recognition in the field. The quality of the output is proof that the recognition is entirely deserved.
- The scholarly output is quite impressive not only in terms of quantity but also the quality and diversity of the research.
- Thus far in their scholarly career the PI has been a model for the role and importance of public outreach in academic work.
- The PI has shown impressive energy and commitment in setting up and directing a large, state-of-the-art and lab at their institution, as well as conducting other valuable activities like conference organization and science outreach/education.
- The PI's work is focussed and has had some good impact with an excellent record of technological innovation.
- The applicant bears an excellent CV and has already shown the ability to carry out independent and innovative research, also outside of Europe.
- Relative to career stage the PI has published an outstanding number of papers in leading international multidisciplinary journals, has attracted considerable amount of funding, and has supervised an impressive number of PhD students.
- In the last years he has had several nice papers published in top tier journals. All of these publications center around the scientific topic outlined in the current grant.
- The PI has written several very good reviews in excellent journals and is a leader in the field.
- The applicant comes across as an emerging leader in the field.

Common Weaknesses

- Weak/modest publication record/ most papers are from conferences.
- Limited experience in the approach
- Limited impact of PI's research
- The CV of the PI does not reveal clear signs of scientific leadership.
- The PI has most of his papers (especially those with larger number of citations) written in collaboration with other researchers.
- His leading role in previous research is not presented in convincing detail.
- PI only published in second tier journals.
- No previous expertise/published papers in the proposed area of research
- Poor funding record
- No prizes/awards that would indicate PIs ability to perform ground-breaking research
- Lack of experience to successfully complete the proposed methodology
- Lack of experience in coordinating large projects
- Ability for conducting ground breaking research and independent thinking do not clearly appear from the proposal OR track record of the PI does not demonstrate this ability.
- Non-competitive track record/ no participation in organisational committees for international conferences and workshops.
- Not so many senior authored publications with widely recognised impact in the field.
- Scientific autonomy of the PI not fully clear.
- Independence of the PI not evident, too much association with more senior staff (is publishing still with Postdoc mentor).
- The PI is deeply aware of current research and able to articulate a clear and unique vision.