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Agenzia per la Promozione della Ricerca Europea

PILLOLE DI ECCELLENZA: DOCUMENTO ANALISI EVALUATION SUMMARY REPORT ERC ADVANCED GRANT

Pillole di eccellenza: documento Analisi Evaluation Summary Report ERC Advanced Grant
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INTRODUZIONE

Breve presentazione dell'ERC e della call Advanced Grant

Lo **European Research Council (ERC)** è l'organismo dell'Unione europea che finanzia i ricercatori di eccellenza di qualsiasi età e nazionalità che intendono svolgere attività di ricerca di frontiera negli Stati membri dell'UE o nei paesi associati.

L'ERC supporta progetti di ricerca ad alto rischio, condotti da Principal Investigator (PI) con curriculum di rilievo a livello internazionale. I progetti sono finanziati sulla base delle idee progettuali presentate dai ricercatori, in qualsiasi campo della scienza, senza argomenti di ricerca predefiniti, e valutati sulla base del solo criterio dell'eccellenza scientifica. L'ERC è composto da un Consiglio Scientifico e un'Agenzia Esecutiva (ERCEA). Il Consiglio Scientifico è l'organo direttivo dell'ERC, definisce le strategie scientifiche, gli strumenti di finanziamento, le metodologie di valutazione; l'ERCEA implementa e applica tali strategie nella gestione operativa delle attività dell'ERC. L'ERC opera in autonomia garantita dalla Commissione europea.

L'**Advanced Grant (AdG)** permette a leader della ricerca eccezionali ed affermati di qualsiasi età e nazionalità di portare avanti progetti innovativi e ad alto rischio in grado di aprire nuove direzioni nei rispettivi campi di ricerca e in altri settori. I ricercatori devono essere scientificamente indipendenti, attivi nella ricerca negli ultimi dieci anni ed avere un profilo che li identifichi come leader del rispettivo settore di ricerca. Il finanziamento può arrivare a 2,5 milioni di euro per singolo progetto per una durata massima di 5 anni.

Obiettivo del documento

Il presente documento è frutto dell'esperienza del Competence Team che in APRE si occupa dell'ERC e mette in evidenza i principali punti di forza e di debolezza rilevati dai valutatori nei report di valutazione del bando ERC Advanced Grant.

Per la realizzazione del documento sono stati presi in considerazione i report di valutazione delle proposte progettuali presentate al bando HORIZON-ERC-AdG-2021, per tutti e 3 i domini scientifici dell'ERC: Physical Sciences and Engineering (PE), Life Sciences (LS) e Social Sciences and Humanities (SH).

Per la preparazione di questo documento di analisi, il Competence Team ERC ha:

- inserito "i punti di forza" e "i punti di debolezza" sotto i diversi criteri *(Criterion 1 - RESEARCH PROJECT - Ground-breaking nature and potential impact of the research project, Scientific Approach; Criterion 2 - PRINCIPAL INVESTIGATOR - Intellectual capacity and creativity);*
- evitato di inserire un commento già presente;
- cancellato gli argomenti scientifici;
- cercato di rilevare i commenti più frequenti dei valutatori.

Alcuni dei punti di forza e di debolezza possono avere dei significati simili ma sono stati annotati intenzionalmente dal Competence Team ERC perché scritti da valutatori diversi che hanno utilizzato parole con significati simili. Con questo documento, il Competence Team ERC desidera mostrare ai ricercatori alcuni aspetti da prendere in considerazione durante la stesura della loro proposta progettuale.

CRITERION 1 - RESEARCH PROJECT

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 (e.g. novel concepts and approaches or development between or across disciplines)?
- To what extent is the proposed research high risk/high gain (i.e. if successful the payoffs will be very significant, but there is a high risk that the research project does not entirely fulfil its aims)?

STRENGTHS

- The proposal is addressing an important environmental challenge and is ambitiously aiming to combine diverse approaches across disciplines
- This proposal aims to explore novel uses of the topic
- The proposed research addresses highly important challenges
- The objectives are highly ambitious and beyond the state of the art
- The proposed research is certainly high risk/high gain.
- The objectives are ambitious in scope
- The proposal also seeks to address more targeted questions
- The objectives and approaches presented in this proposal are not novel
- The PI was a coordinator of a large EU (FP7) project on this strategy
- The proposal addresses an important challenge in the field
- The proposal tackles a challenging and timely topic
- The proposal focuses on two important problems in today's world: desalination of sea water and capture of greenhouse gases

WEAKNESSES

- The idea on its own is charming, but the implementation lacks convincing feasibility
- The proposal has a very wide scope and hence it appears rather unfocussed on what really will be delivered
- The summary is particularly vague.
- The panel was concerned about the large number of poorly connected aims, some with a narrow scope
- The proposed research does have risk but some of the risk is due to incomplete or unclear description of the major strategies to be undertaken to accomplish tasks, not due to inherent risk in the questions being asked
- The proposal is novel although not particularly groundbreaking
- the proposal does formulate a set of logic experiments around the central and important question
- The research topic is important, but I don't see it as beyond the state of the art, neither in terms of technology, nor in its general idea. It is more of a collection of interesting but conventional avenues to follow, rather than a homogeneous beykkioh+ìond state of the art approach

If successful, there would be numerous applications in the daily life (gas enrichment, water purification)

Naturally, the topic is of extreme importance, and may lead to very significant energy savings in mobility, coupled with the decrease of CO2 emission

The previous results of the PI and his teams promise a successful project, which may bring significant improvements in optimising the vehicle control systems

The novelty of the overall approach presented in the proposal needs further justification

The proposed research is high risk because of the intrinsic difficulties in achieving 20% energy savings over current electric car designs. It would be high gain because the mentioned energy saving would be a major contribution to combating global warning

The challenge is important because if the projects objectives are reached, it will give the community a new tool to get a better understanding of plant physiology and plant nutrients acquisition

The novelty of the project is to combine the isotopic measurements with quantum chemistry modelling of the fractionation and physiological processes responsible for the uptake and the translocation of the metal ions. It is therefore clearly an interdisciplinary project(chemistry, geochemistry, plant physiology, biochemistry). This is clearly a strength of the project

The project is high-risk high-gain for the step 1 objective or task which is to obtain the isotopic composition of the free metal ion and the complexed metal ion in the solution. This is a difficult task as there are few methodologies that can be used to make the distinction between the two signatures. The project proposes to develop a new one. This is clearly the part with the most risk since the subsequent steps of the project are depending on the step 1. Indeed Step 2 needs experimental data to validate the numerical determination of the fraction factors

It seems to be organized so that each WP appear to be completely independent from the others. On one side this might be a plus, since pitfalls in the development of one of WPs will not impair the others. However, the choice of proposing such a "wide" exploration limits the depth of the insights that could be gained from each of the WPs

The objectives are ambitious, represent a logical but largely incremental approach to improve the existing technology with some innovative aspects

The proposal is not particularly high risk/high gain

The topic is indeed important but the proposal is not well structured. It is not clear what the PI considers the main challenges to be, and which are more straightforward. This is a good quality proposal but does not obviously go beyond the state of the art.

It is not well written and it is difficult to understand the meaning of certain phrases (e.g."external solicitation")

The studies, however, although innovative do not seem to be particularly high risk since it uses available modeling methodologies to investigate these effects

The project addresses important automotive challenges; the objectives are ambitious but not very different from other similar projects that are run in academia and industry

In addition to a clear risk mitigation, I also miss deliverables and milestones

The proposal uses a lot of buzz words that seem to be misplaced

For an interdisciplinary proposal to be successful in my view it would be necessary to make a contribution to both disciplines (automotives and AI), even if smaller than a mono-disciplinary project. Simply applying the techniques from one discipline to a problem in the other does not suffice)

The work is truly interdisciplinary, as is the background of the PI (holding 2 PhDs in Structural Engineering and in Biology and Applied biotechnology)

The idea is novel and exciting, offering a vast exploration space for designs of new materials

The dissemination plan is interesting and quite creative

The proposal addresses an outstanding challenge in the field and if successful it will be a game-changer in the field, and therefore is high-gain research

If successful, this project would have major implications for a wide range of disciplines

The PI has an outstanding expertise in the field

The proposal is a bit annoying to read because of too much hype and jargon

The project proposes an ambitious research agenda clearly beyond the state of the art both, conceptually and empirically

The project has the potential to make substantial conceptual and methodological contribution to the literature, and as any big contribution there is a high risk associated to it from being able to develop a theoretical model that properly accounts for all the variables the PI has proposed, to the collection of data, to being able to implement the relevant empirical methods to test the predictions of the model

The PI proposed a well thought-out strategy to mitigate these risks by creating a network of top researchers in a variety of disciplines to advise and co-author in the project, but actively collaborating with experienced institutions for the collection of data, and by exposing research to the scrutiny of peers through active engagement with the research community through the attendance and presentation of academic conferences and seminars

The overall project does not show a real coherence linking the various sub-projects together and synergies to deliver substantial scientific new results that would answer or partially answer some key open questions in astrophysics related to the dust content in the early Universe. Hence, from the construction of the proposal itself, it is hard to figure in what the project would be transformational, lead to scientific breakthrough and be in the end high gain project

The resources to be funded by the project are not very clear

The objectives are ambitious but not novel per se

The research is highly interdisciplinary and will likely lead to substantive advances in understanding of how these metal-ligand complexes function, but it does not attain the level of novelty or high-risk/high gain that is characteristic of an ERC Advanced Grant

The panel agreed the proposal addresses an important challenge, but did not find it high-risk/high-gain. It was concerned that the proposal is not about work to be done but focuses on work that is already carried out, so the conclusion seems to be already anticipated. The panel found the new research line interesting, but not ground-breaking

The description of the state-of-the-art is too concise, and the synopsis does not offer sufficient insights about the scientific approach

The proposal does not include a detailed research plan and does not clarify the relationship between the quantitative survey and the in-depth, micro-history section

The panel was concerned that many key scientific aspects of this proposal have been under-specified, making it difficult to assess whether it is ground-breaking

The panel agreed the proposal addresses an important challenge, but did not find it high-risk/high-gain

There is also a hiring of a project manager which is fundamental to the well-functioning of any research project

Overall, there is no doubt that the PI has taken all the actions needed to mitigate the risks associated with such a potentially high gain project and there is absolutely no doubt in my mind that the project will yield what it has promised

All in all, the project is a good combination of solid projects, basically guaranteed to yield a substantial amount of solid papers and some high-impact papers (the disorder work). Apart from that there is a (relatively low) chance the project as a whole will actually succeed in increasing our understanding of awareness

Even though the subject of investigation (awareness, perceptual binding and its fleeting nature) is definitely interesting, the current proposal lacks a clear focus, it is not well organized and does not present a coherent story

Generally, the project goals and the envisioned conceptual advancement remain unclear. While many interesting questions and populations are tackled, it is difficult to see whether and how the planned lines of work will converge at the end of the project

The planned research appears somewhat overambitious and needed to be focused more

Some approaches seem to lack feasibility while others lack novelty, or are not combined convincingly.

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)?

STRENGTHS

- The PI proposes an integrated laboratory
- The applicant is also highly experienced in that particular field
- A range of methodologies is proposed
- The risk of the programme is low but this is also because the goals are broadly defined
- The objectives are highly ambitious and beyond the state of the art. The proposed research is certainly high risk/high gain
- Feasibility for WP1 is high
- The proposed aims in this research proposal are clearly feasible
- Give the list of publications and expertise of the author, the collection of aims, or a significant fraction, are in my opinion feasible in principle
- The main objectives of the proposal are clearly identified, both in terms of results to achieve and in schedule of the main milestones
- The division of the work and the staff dedicated to the proposal seems functional to the achievement of the objectives
- The scientific approach is feasible
- The proposal explains the current state-of-the-art approaches and their drawbacks, followed by a well-argued research plan on how to combine state-of-the-art deep learning approaches
- The methodology is clearly appropriate and has innovative aspects that have a reasonable risk/gain balance

WEAKNESSES

- While the proposal timely addresses some important challenges, the approach seems in many places superficially presented
- It is of course advantageous when scientists with very different and complementary expertise can work together on a common project at the same location, but whether such a diverse group of scientists should be supervised by a single PI to obtain the best results remains an open question
- The panel considered that the proposal's outlined scientific approach lacked focus and details
- Because the proposal lacks focus, the rationale behind several of the work packages (1, 3 and 5) is poorly explained and there is no supporting evidence, it is difficult to believe this proposal will lead to ground-breaking discoveries.
- This is not a high risk high gain proposal
- The panel was therefore not convinced that the approach taken in the proposal is robust enough.
- The ultimate goal of the proposal is not clearly outlined
- The methodology is a continuation of the PI's research, there is no revolutionary new approach proposed
- The topic is certainly very important. However, although there is indeed a great need for this type of methodology, I am not sure if the PI has the necessary preparation to execute the proposed project
- The scientific approach looks feasible, but is not written in sufficient depth for major new ideas to be identified
- Hybrid MC/MD modelling of small molecules within a large flexible MOF is challenging but the PI does not clearly describe the scientific approach that he will use to go beyond the state-of-the-art. As a result, I have real concerns about the feasibility of the proposal research

The expertise of the PI and his lab is outstanding to conduct this research

The approach is an innovative use of existing modeling methods and could provide improved performance of existing materials

Several proof-of-concept experiments have already been conducted to ensure that the proposed methodology is suitable for the project's objectives

The outlined scientific approach is well founded and based on solid competences and previous experience of the PI

The Extended Synopsis is well detailed and structured, the work divided into 4 aims. The PI will use many FEL and synchrotron facilities

The tasks make this research quite comprehensive but feasible. The PI has demonstrated success in several of these areas by obtaining preliminary results. Milestones are clearly defined for each task. The PI and his team are highly qualified to conduct this research. They also have sufficient institutional resources to conduct this research

The budget is justified

All of the proposed projects have a chance for high gain. They would lead to a deeper understanding and a broader view of the results obtained so far

The proposal describes a rather concrete work plan with explicit intermediate targets

Among the eight research methods in the mixed-methods approach, the participatory creative methods involving artistic activities
is stimulating and innovative

The PI has gone through great length in outlining the mitigating strategies in the proposal, such as having a well-structured network of collaborators, having worked on this topic in the past and being a leader in the field, as well as hiring a private company for data collection

Despite documented collaborations and previous/ongoing activity, it would have been good to discuss accessibility to infrastructures that often requires project proposals

The project is divided into five work packages, the approach seems feasible but there is no risk assessment

The word "scientific" does not appear in this proposal. I strongly believe that such a project is more industry oriented

The very large and big picture addressed at the start of the proposal is not addressed by the scientific approach

While the PI has a very good track record, for an ERC advanced proposal there needs to be a very clear science focus

The 5 projects that are listed, seem all nice to do, but are not very connected and it is not very obvious how collectively they are going to give answers to the bigger questions in the field

The methods could have been spelled out better and the expected results and associated uncertainties could have been better discussed

The linguistic part of the analysis is unconvincing as it stands, for lack of detailed case studies, potential theories and theoretical consequences, and anchoring in the linguistic literature

One risk that could have been more explicitly acknowledged is that for one aspect of data collection the applicant is planning to rely on voluntary participation in an app

In addition, it is unclear how novel the proposal is with respect to confidence. There is already some work on confidence in other domains, and the PI recognises that it is not audition-specific confidence. It is therefore unclear what would be the exciting novel aspect here

The panel was not convinced that the key concepts of "identity change," and "identification" had been adequately clarified and could be effectively operationalised for the purposes of a cross-regional comparative study. The panel also raised concerns about case selection. It was observed that the choice of non-European cases were inadequately justified, and that the proposal did not integrate sufficient expertise on these cases

I consider the proposed research methodology and working arrangements appropriate to achieve the goals of the project. The description of working packages and how they meet the overarching goal of the project is well defined, as well as the team of researchers, collaborators, consultants, and admin support

The scale of the research is appropriate to the project's ambitions and the range of films (1970s to present) is both appropriate to the research topic and doable

The approach is clear, well-structured and feasibility seems secured

The project is well designed; the correspondence and connections between objectives and work packages are reassuring in terms of the coherence of the proposal

Given this project's broad scope, the PI appears to have prepared a sound and workable research procedure of recruitment, research, and consultation

The proposed structuring hypothesis is evidently true; I doubt any mainstream scholar in this field would doubt it. Data will be found to support it

The regional organization component of the proposal was also somewhat underdeveloped. In view of the panel, the research design did not sufficiently specify the concrete institutional and policy mechanisms of identity influence by regional organizations

However, overall the goal of this research, its potential theoretical impact, the work plan and the hypotheses tested were not sufficiently well developed and many aspects remained unclear. The inclusion of particular patients groups also needed a better justification

The scientific approach is not feasible. It is not properly planned and depends on many variables that are not controlled by the PI

The project team will consist at any single point of ten team members: three MA students, three PhD students, three postdoctoral scholars and the PI. A weakness of the project, in my view, is that the PhD (and MA) applicants are mostly to come from the HI. Particularly for the PhD students, it is unclear why a wider body of applicants might not be canvassed. The postdocs are to be appointed for between 12-24 months, making it possible in principle to appoint a maximum of 15 postdocs to the project - though the sacrifice in continuity would doubtless be too great, if that were to be done. I think there is a difficult balance to strike between international and disciplinary breadth and core team strength and that the PI has been wise to build in flexibility here. Much will depend on who applies, at what stage, and how their particular capabilities will enable the project to flourish

Although I do believe the project could be very valuable, there are many risks. A too-wide international and interdisciplinary spread of participants in the central team, combined with too high a turnover, could lead to the fragmentation of the research.

It claims to be interdisciplinary, as it involves such different disciplines as "the history of science and technology, industrial history, social history, literature, anthropology, museology, industrial archaeology, conservation research, design studies, environmental history, chemistry, sociology and engineering." However, collecting multiple disciplines does not make interdisciplinary research. This proposal does not seem to show what will be done for connecting and integrating these disciplines, apart from that all of them are in some way related to 'plastics'

The proposal does not explain the rationale for the selection of the case studies, which is, instead, a crucial issue in any comparative project.

CRITERION 2 - PRINCIPAL INVESTIGATOR

Intellectual capacity and creativity

(assessed over the last 10 years, extended with the time of any eligible career breaks – ERC WP 2021, p.19-20)

The questions below can have one of the following five responses: Exceptional/Excellent/Very Good/Good/Non-competitive

- To what extent has the PI demonstrated the ability to conduct ground-breaking research?
- To what extent does the PI have the required scientific expertise and capacity to successfully execute the project?
- To what extent has the PI demonstrated sound leadership in the training and advancement of young scientists?

STRENGTHS

The PI has a good publication record over the past 10 years where he is a corresponding author in ca. half of them.
The PI has been active and successful in recruiting external funding. The PI has mentored many PhD and Post-doc; quality of mentorship is unclear given the lack of detail about the mentees

The PI has a long record of sound research on metalloenzymes and made various discoveries. There is a good funding record

There is solid productivity by the PI over the last 10 years. Papers in good journals, trainees that have had success, some leadership roles. A strong/very good investigator with clear impact in his field

The PI has demonstrated the ability to conduct and manage competitive research. As such, the PI is very well considered

The PI has a good track record with solid publications in the field. Some recent higher impact work is duly acknowledged. For a competitive Advanced Grant application, one certainly wishes for some very high impact contributions in recent years

WEAKNESSES

The PI’s track record is good but not outstanding

There is no information on scholarship

The applicant has an excellent publication record, but often is not the leading author on many of the selected publications

Training and advancement of scientists is good, along the normal career paths, but from what I see none went to an academic or industrial group leader or leadership position

As stated in the CV section, his work as a director, has created a break in their scientific career

The track record description appears to be incomplete, there is no specific list of publications attributable to the applicant, nor is detailed scholarship information on the advancement of young scientists provided

The 10-year publication record of original research as senior author is not particularly strong. There is little evidence of outstanding creativity

The track record of the PI is impressive in term of citations but can hardly be compared to more academic profiles as it consists almost exclusively in original papers and reviews in the field of drug delivery; e.g. no publications in general journals

The PI has mentored of most impressive number of PhD students (about 69) within his last 20 years of academic career

The PI appear to have considerable problem-solving capacity. The proposal is directly in his line of expertise

There is a reasonable funding record, with national and EU grants

The PI is clearly well qualified to pursue the proposal, and has a convincing record in leadership and training of young scientists

The PI has mentored numerous Post doc (about 21 from 1997) plus about 7 PhD students.

The wide experience of the principal investigator on the study of the topic, and the expertise of the research group would allow the achievement of the planned results

The PI is an established researcher and has been involved in organizational activities and supervision of junior scientists

The applicant has an excellent track record in the field;
the results of the research have been published mostly in specialized journals. The PI has been successful in recruiting funds, and is providing service to the academic community

The PI has a very good track record and is an expert in the field with a very good publication and citation record with an upwards trend

He has been invited in many important international conferences. He has trained numerous post docs and PhD positions who are now holding important academic or industrial positions

Since 2005 the PI has supervised about 4 Postdocs, 7 PhD students (2 finalists in best PhD) and about 15 Master students, which is not particularly high for the PI's seniority

The PI is a productive scientist, who recently established himself as an independent researcher. His track record in the specialized area is strong but his work is not yet widely recognized and appears not to have a major global impact

In the "Ten years track-record", 12 papers are listed, and one is older than ten years; formal requirements should be complied with

The papers are well cited, but papers with really high impact (>100 citations) are lacking

The PI contributed to the organization of several national meetings, but he organized only one international conference

PhD supervision is very rare

The PI has authored many publications although a majority of them are not within the research area described in the application

Most of the project related publications are from the last 4-5 years and the articles are not very highly cited

It is unclear if the PI has demonstrated enough ability within this research area (nothing is mentioned about the project group)

Out of his top 10 references, there is no single one as a first author

It is difficult to assess the PI's publication record because in many publications he is one out of many authors (in some cases leading the list of authors simply because they are alphabetically ordered), which is typical for particle physics.

The PI is an expert in the area of the proposal. He has a good record in the training of PhD and Master students
The PI has a good publication record, including a book on the area of the proposal which raised some attention
He has clearly demonstrated his ability to train young scientists. He had many postdocs and a large number of PhD students, and two of them opted for an academic career
The PI has the required expertise in structural biochemistry to carry out this project. He has a very good track-record in terms of publications, and invitations to international conferences. He has been strongly involved in training young researchers that he considers as a key performance indicator. All of them have found qualified positions
The PI has an excellent experience in successfully managing research projects
The PI has international collaborations
The PI is internationally well recognized, as invited talks testify
The PI is very active scientifically and has published more than 30 peer-reviewed journal papers this year
The PI has had a major role in a large number of industry sponsored research projects, which, among others, demonstrates the PI's ability to conduct ground-breaking research
The PI had a number of PhD students who now hold high positions in academia. The latter demonstrates the PI's sound leadership in the training and advancement of young scientists.
The PI has broad experience in leading large teams and training young researchers.
The PI has an excellent experience in managing large budget projects

The panel appreciated the PI's track record in light of challenging circumstances in recent years, but did not consider it sufficiently competitive at the international level, in line with the standards required for this category of grants
The track record of the applicant PI is good but previous research does not seem to have been ground-breaking
The PI appears to have relatively little experience of landscape studies, and the work on memory that has been done within it, and does not cite any scholarship emanating from that highly relevant field
The PI does not demonstrate leadership in training young scientists
The PI's ten-year track record does not seem to demonstrate high academic productivity
The PI states that received prizes and awards amount to "about 1,000,000 EUR," but details are not given
No experience in supervising research projects like these
The PI has no experience in leading large-scale and long-term projects
The PI reports formal or informal supervision of graduates students, however does not demonstrate sound leadership in the training and advancement of young scientists
The track record of research achievements related to the project in the last 10 years is very good, however among publications there is no monograph
The CV is strong with a good publication list that is relevant for the project (although expertise is very much only on Spain and Portugal, not on the other cities included in the project, this is a risk)
While the network of the PI is strong, there are not many contacts mentioned in the cities under study

The PI has an excellent mentoring record with mentees pursuing careers in the private sector but also in highly visible academic institutions all over the world.	The research expertise does not clearly match with the value aspects of the proposed research, and I find the quality of the list of recent publications underwhelming
The PI's key results were all obtained in collaboration with different groups of top experts	The PI's ten-year track record is not quite impressive, and there are a number of repetitions and ambiguities in the description given by the PI
The PI is the author of a large number of publications, which provide excellent evidence of creative independent thinking	The PI has limited experience in convening major international gatherings or running large-scale research grant projects
The PI's research and the publications that have arisen from their projects have informed academic and non-academic debates	Hardly any information is given about supervision experience ("I ́ve been advising and arguing several theses (M.Sc and PhD) and work with postdocs" and "Regarding the supervision of students I have been a supervisor and cosupervisor of master and doctoral theses since 2010.") and there seems as yet to be no experience with leading large-scale research projects.
The PI is a leading scholar in the analysis of time-use and telework	
The PI has experience in leading long-term large-scale projects - he was successful in applying for external grants in his country (total about 1 million EUR)	

Panel comment

- The panel recognises the need for more efficient xxx in the topic, but is not entirely convinced that the approach taken in the proposal will lead to success.
- The proposal builds on the applicant's strong track record on the topic.
- Overall the panel considers this proposal to be of good quality. However, based on the combined set of criteria used in the assessment it was not ranked highly enough to be retained for Step 2. The panel therefore recommends that the proposal should not be retained for Step 2 and should not be considered for funding.
- The scientific approach should have been described in the proposal in a bit more detail. There is considerable redundancy between parts 1 and 2 of the proposal, and also within part 2 itself, occupying space that could have been spent on more methodological details (pilots, example stimuli, anticipated numbers of datapoints) as well as a clearer definition of core notions (e.g., 'language code' isn't really defined). Based on the applicant's publications and the content of the proposal we have no real reason to doubt that the details will be adequately worked out, but this shortcoming should be flagged.

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