

ERC information meeting

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Leuven, December 8, 2011

PANEL MARKS

<p>1. Principal Investigator</p> <p>Quality of research output/track-record: How well qualified is the Principal Investigator (and any co-Investigator if applicable) to conduct the project (reviewers are expected to evaluate the quality of the prior work such as published results in top peer review journals as well as other elements of the Principal Investigator's CV). To what extent are the publications and achievements of the Principal Investigator groundbreaking and demonstrative of independent creative thinking and capacity to go significantly beyond the state of the art? To what extent does the quality and quantity of funding the Principal Investigator has attracted during the last ten years demonstrate his/her reputation as a performer of ground-breaking research?</p> <p>Intellectual capacity and creativity: To what extent does the Principal Investigator's record of research, collaborations, project conception, supervision of students and publications demonstrate that he/she is able to confront major research challenges in the field, and to initiate new productive lines of thinking?</p>	3.5 / 4
<p>2. Research project</p> <p>Ground-breaking nature of the research: Does the proposed research address important challenges at the frontiers of the field(s) addressed? Does it have suitably ambitious objectives, which go substantially beyond the current state of the art (e.g. including inter- and trans-disciplinary developments and novel or unconventional concepts and/or approaches)? How well conceived and organized is the proposed activity?</p> <p>Potential impact: (a) Does the research open new and important, scientific, technological or scholarly horizons? (b) Will the project significantly enhance the research environment and capabilities for frontier research in Europe (including the host institution)?</p> <p>Methodology: Is the proposed research methodology (including when pertinent the use of instrumentation, other type of infrastructures etc.) comprehensive and appropriate to the project? Will it enable the goals of the project convincingly to be achieved within the timescales and resources proposed and the level of risk associated with a challenging research project?</p> <p>High-gain/High-risk balance: a) does the proposed research involve highly novel and/or unconventional methodologies, whose high risk is justified by the possibility of a major breakthrough with an impact beyond a specific research domain/discipline?</p>	3.5 / 4
<p>Total mark</p>	7.0 / 8
<p>Has the proposal passed the thresholds (2/4) for criteria 1 and 2?</p>	Yes

with consequence:

I am pleased to inform you that the panel has recommended your proposal for funding. However, due to the competition amongst the many excellent proposals and the limitations of the ERC budget, the ranking of your proposal does not allow for immediate funding. Consequently, your proposal has been included on a short reserve list of proposals, some of which may be granted when additional budget becomes available.

PANEL MARKS

<p>1. Principal Investigator</p> <p>Quality of research output/track-record: <i>How well qualified is the Principal Investigator (and any co-Investigator if applicable) to conduct the project (reviewers are expected to evaluate the quality of the prior work such as published results in top peer review journals as well as other elements of the Principal Investigator's CV), taking into account the early or more mature phase of his/her transition to independence, as well as prior career breaks and/or unconventional research career paths (especially in the case of women scientists).</i></p> <p><i>To what extent are the publications and achievements of the Principal Investigator groundbreaking and demonstrative of independent creative thinking and capacity to go significantly beyond the state of the art?</i></p> <p><i>To what extent does the quality and quantity of funding the Principal Investigator has attracted during the last ten years demonstrate his/her reputation as a performer of ground-breaking research?</i></p> <p>Intellectual capacity and creativity: <i>To what extent does the Principal Investigator's record of research, collaborations, project conception, supervision of students and publications demonstrate that he/she is able to confront major research challenges in the field, and to initiate new productive lines of thinking?</i></p>	3.63 / 4
<p>2. Research project</p> <p>Ground-breaking nature of the research: <i>Does the proposed research address important challenges at the frontiers of the field(s) addressed? Does it have suitably ambitious objectives, which go substantially beyond the current state of the art (e.g. including inter- and trans-disciplinary developments and novel or unconventional concepts and/or approaches)? How well conceived and organized is the proposed activity?</i></p> <p>Potential impact: <i>(a) Does the research open new and important, scientific, technological or scholarly horizons?</i> <i>(b) Will the project significantly enhance the research environment and capabilities for frontier research in Europe (including the host institution)?</i></p> <p>Methodology: <i>Is the proposed research methodology (including when pertinent the use of instrumentation, other type of infrastructures etc.) comprehensive and appropriate to the project? Will it enable the goals of the project convincingly to be achieved within the proposed timescales and resources (including the costs of the Principal Investigator and the members of the team who will be engaged in the project) and the level of risk associated with a challenging research project?</i></p> <p>High-gain/High-risk balance: <i>a) does the proposed research involve highly novel and/or unconventional methodologies, whose high risk is justified by the possibility of a major breakthrough with an impact beyond a specific research domain/discipline?</i></p>	3.5 / 4
Total mark	7.13 / 8
Has the proposal passed the thresholds (2/4) for criteria 1 and 2?	Yes

with consequence:

I am very pleased to inform you that the Panel has recommended your proposal for funding. I offer you my warmest congratulations for this success.

Conclusion:

- 2 panel evaluations,
- 8 referee evaluations.

- 1) 8 referees
- know me,
 - know my works
 - are from the field:

1. Principal Investigator:

Brilliant researcher in normative public finance, a field that has lost importance in the profession over the last decade or so. His publication record reflects this relative disinterest with only three papers among the top 10 publications in top 5 journals - and the others in high reputation journals in public finance, social choice and theory in general.

Conclusions: they didn't blindly look at publications and citations, they weighted these statistics by taking the field into account.

Panel evaluation: "While not necessarily subscribing to each and every opinion expressed, the panel found that they provide a fair overall assessment, ..."

2) About the trade-off between being credible and being innovative:

- (2008) "Overall, a well-constructed proposal, with a high chance of success, but perhaps lacking in ambition since it follows very much in the line of the PI's existing research."

- (2010) "Poverty has been an intractable problem, and so this proposal—which could contribute to effective anti-poverty policy—has a potentially high payoff. At the same time, the proposed research seems likely to be doable, in view of the PI's previous accomplishments."

My conclusion: the emphasis is more on the innovative side.

3) Panel evaluations:

2008: "The panel thought that the proposed research is strong and worthy of funding, and that the principal investigator has an excellent track record. The proposal was not ranked very high, since the third part of the proposal did not seem to fit very well with the first two.

The panel recommended that the proposal be retained for funding with a grant not exceeding 800,000 Euros, reducing the number of Ph.D. students to one. It also felt that the proposed travel figures were rather high."

2010: "The panel found the PI's plans to develop a preference-based measure of poverty (topic three) to be very interesting and potentially highly significant. However, the panel concluded that the part of his research proposal devoted to the persistence of poverty was not thought through sufficiently carefully. For this reason, the panel decided to fund only the research relating to topics one and two of the proposal. The budget will be cut accordingly."

My conclusion: a trade-off between being focused and propose ideas they like.