

The proposal “Climate, Hydrology, Energy, Water: the Conversion of Uncertainty Domination and Risk Into Sustainable Evolution” (CHEWtheCUDandRISE), submitted to the ERC IDEAS Grant Scheme, passed the thresholds for criteria 1 and 2 but was not retained for funding. The proposal received several comments by four reviewers (denoted hereinafter as R1 to R4) and the Panel (denoted as P), some of which are positive and some negative. My research team and I feel that the comments that downgraded our proposal demonstrate shortcomings in the way the proposal has been evaluated. Specifically, in our opinion, there were serious deficiencies in the way the proposal was read and interpreted, with reference to the ERC Guide for the IDEAS Grant Scheme, by the reviewers and the evaluation panel. This led to unjustified low marks and unfounded negative criticism including untrue statements, as I address in detail below, thereby requesting for redress and eventually funding of the proposal.

For convenience and ease of reference I have copied below the comments (retaining their spelling) classifying them into positive and negative.

\*\*\* 1. EVALUATION OF THE PRINCIPAL INVESTIGATOR (PI) \*\*\*

\*\*\* Positive comments \*\*\*

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- (R1) Senior scientist in hydrology; strong editorial activity
- (R2) The PI is a world leading scientist with very high writing skills
- (R3) PI record is good...
- (R4) PI has a rather unique background in the field of hydrology with bold and visionary ideas; combines a strong quantitative background with broad understanding of problem area.
- (P) The principal investigator has a good track-record

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\*\*\* Negative comments \*\*\*

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- (R1) moderate international recognition; low level of citations
- (R3) ... although some publications seems not to be of high impact

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I will not focus on the obvious self-contradiction, which cannot be regarded to be a scientific judgment (i.e. in R1: “strong editorial activity” vs. “moderate international recognition”): are

editorial posts offered to scientists with moderate recognition?). However, I cannot overlook the significant level of inconsistency in the reviewers' comments: Can a PI be a world leading scientist with a unique background in the field of hydrology with bold and visionary ideas, combining a strong quantitative background with broad understanding of problem area, and, at the same time, of moderate international recognition with low level of citations?

It is exactly the research of breakthrough character (challenging established beliefs) that, more often than not, meets with resistance and hence lower level of recognition at the initial stages. This problem was discussed in the proposal, in anticipation of review comments of this kind. Quoting from p. 3 of Part B of the proposal: "Since a significant part of my work is, arguably, breakthrough research challenging established beliefs, getting my papers to be published initially required persistence, even challenging editors' decisions. I now know that rejection is a very likely outcome of the review process for novel and innovative research (cf. Miller, 2007 [The Government Grant System: Inhibitor of Truth and Innovation?, J. Inf. Ethics, 16(1), 59-69]). A key focus of my work has always been to dispel well-established myths that hinder science, and attempt to go beyond the state of the art. ... Published research challenging existing beliefs tends to be ignored by other researchers, unless it can be proved to be mistaken. However, there has never been any publication reporting errors in my work, despite the fact that I always challenge reviewers, who suggest rejection of my papers, to publish their opinion. Ignoring challenging publications is an easier path, and perhaps this is the reason why my research findings were not quoted as much initially."

I find it disappointing that this part of my proposal was not paid proper attention in the evaluation process and that the reference contained in the above quotation, which analyzes this very problem in relation to a grant awarding system, perhaps was not noticed.

Since, as quoted from the "ERC Grant Schemes -- Guide for Applicants":

- "the ERC's mandate is to encourage the highest quality research in Europe", and
- "grants are awarded and managed according to simple procedures that maintain the focus on excellence, encourage creativity and combine flexibility with accountability",

and since the evaluation criteria include:

- the addressing of the proposed research of "important challenges at the frontiers of the field(s)",
- the "ambitious objectives, which go substantially beyond the current state of the art", and

- the involvement of “highly novel and/or unconventional methodologies”,

one would expect that the reviewers would focus on the “bold and visionary ideas” and avoid the stereotypes such as “moderate international recognition; low level of citations”.

This argument notwithstanding, both negative statements of R1, “moderate international recognition” and “low level of citations” are in fact untrue. The lower table on p. 3 and the additional table on p. 4 clearly indicate that currently I have achieved high recognition and high level of citations within my scientific discipline. Although higher numbers of citations may be likely in some disciplines, clearly, citation data are relevant to the discipline, rather than absolute measures, and should be considered only in the category of a scientist’s peers.

Bibliometric data for the field of hydrology have been analyzed by Koutsoyiannis and Kundzewicz (2007; Editorial - Quantifying the impact of hydrological studies, Hydrological Sciences Journal, 52 (1), 3-17). This article contains, inter alia, analysis of the h index (an index encompassing productivity and recognition by citations) of a list of outstanding hydrologists, specifically the 27 laureates of the International Hydrological Prize (IHP) by the International Association of Hydrological Sciences (1981-2006). As shown in Fig. 3 of this article, the average h index, among the laureates, in the last decade is around 10 (it was much lower in the 1980s). Given that my h-index value (lower table in p. 3) reached 14 (according to data from ISI and Scopus) or 15 (according to data of Google Scholar, accessed Feb 2008), a simple comparison with the above information for outstanding hydrologists proves that the level of citations to my work and international recognition is actually high.

### \*\*\* 2. EVALUATION OF THE RESEARCH PROJECT \*\*\*

#### \*\*\* Positive comments \*\*\*

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- (R1) Novel approach in climate change research based on probabilistic methodology; with application to water resources
- (R2) very interesting methodological approach
- (R4) A very interesting concept and approach proposed for solving/addressing a very important issue, uncertainty and interrelation between climate, hydrology, energy and water; worthy goals ...
- (P) The proposal is built around a very interesting concept and approach proposed relating uncertainty and linkages between climate, hydrology, energy and water processes. The panel

appreciated the holistic perspective and the novel approach to hydrology... This proposal is of a high standard. ... and the proposal is of good scientific quality.

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\*\*\* Negative comments \*\*\*

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- (R1) ...but lack of focussed objectives
- (R3) The proposal seems too broad encompassing several topics instead on focussing on specific scientific questions. The impression is that the work could not be achieved in all parts with the same excellence.
- (R4) ... but not entirely clear how they will be realized, in particular how the different researchers from the group will contribute. Not clear how the team work will lead to achieving the goals.
- (P) ...but it is not entirely clear how the work will be realized, in particular how the different expertise needed to integrate the science will contribute.

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I am thankful for the recognition of the “very interesting concept and approach”, the “holistic perspective” and the “high standard” of the proposal. Yet, I would expect from the ERC panel a more thorough and in-depth evaluation of the proposal and a clearer recognition of the importance of the research topic. What follows suffices to demonstrate this importance: The 2008 Meeting of Nobel Laureates (28 June-4 July 2008) at Lindau on Physics, organized a Panel Discussion on “Climate Changes and Energy Challenges” with Nobel Laureates Profs. Deisenhofer, Giaever, Michel, Osheroff, Rubbia, von Klitzing, Steinberger, chaired by Prof. Dr. Hans Joachim Schellnhuber, from where the importance of climate and energy issues in research and its societal implications, as well as the relevance with the ideas discussed in the proposal can be verified. (This Panel Discussion -- <http://www.lindau-nobel.de/PublicMeetingProgram.AxCMS?Meeting=105> -- was held on 1 July 2008, four months after the submission of the proposal and the discussion is available on line at [http://nobellaureate.feedroom.com/?fr\\_story=60d8eeecb63502e7f7b041331a3c1a6f30c9766f&rf=bm&skin=showcase](http://nobellaureate.feedroom.com/?fr_story=60d8eeecb63502e7f7b041331a3c1a6f30c9766f&rf=bm&skin=showcase))

Coming to the negative comments, I would like to express my disappointment that none of them refers to the proposal per se. Rather all these comments are stereotypical and could be used to any type of scientific proposal, regardless of its theme, in order to undermine it without discussing its content. Therefore, I do not regard that these negative comments consist a scientific judgment of my proposal. In addition, they are unfounded as I discuss next, summarizing them into three: (a) too broad scope and lack of focused objectives; (b)

insufficient clarity of realizing the research programme; (c) insufficient description of integration of team work.

**\*\*\* 2.A. TOO BROAD SCOPE AND LACK OF FOCUSED OBJECTIVES \*\*\***

It should be clear that, given its topic, the scope of the proposal is necessarily broad and this cannot, and indeed should not, be regarded as a weakness: This is in fact a strong point and a major advantage of the proposal, fully compatible with the mandate and scope of the IDEAS programme. The research and technological problems related to climate, hydrology, energy and water, cannot be tackled with piecemeal endeavours. They demand a holistic, integrative and visionary approach in contrast to existing approaches focusing on isolated parts of the entire system.

While the concept of the proposal is, as suggested, necessarily broad, its structure is clear and is working towards well defined specific scientific objectives. The specific objectives, as summarized in p. 15 of the proposal, are:

- A. Deconstruction of myths and fallacies related to hydroclimatic processes.
- B. Development of a consistent theory of hydroclimatic processes.
- C. Knowledge discovery from past data and historical information to extract lessons for the present and future, related to hydroclimatic processes and their interaction with socioeconomic development.
- D. Proposal of a methodological framework, consistent with the new theory of hydroclimatic processes, linking climate and water to renewable energy production and storage.
- E. Proof of concept of the developed theoretical and methodological framework in a real world case study and extraction of transferable lessons.

These objectives are further clarified and analyzed in the proposal and they integrate to a coherent research framework.

**\*\*\* 2.B. INSUFFICIENT CLARITY OF REALIZING THE RESEARCH PROGRAMME \*\*\***

This remark of R4, on which the Panel was based, is unjustified and untrue. Pages 16-24 of the proposal contain:

- Exceptionally detailed analyses of the research programme, that is shaped into six interrelated activities (corresponding to the above objectives plus the development of

informatics tools), further sub-divided into components, each one including one or more research paths, all analyzed at a high level of detail;

- Lists of expected difficulties per activity and the ways to tackle them;
- Discussion per activity of the benefits if the goals are attained;
- Discussion per activity of the implications if the goals are not attained;
- Description of informatics tools to disseminate information;
- Clear definition of envisaged outcomes of the research programme;
- Concise list of measurable indicators of progress of the research and its links with education.

In my opinion, this level of research programme structure and clarity of realization strategy are rare for a visionary scientific endeavour fitting for the IDEAS programme (as opposed to other “conventional” FP7 programmes).

Consider for instance the first activity, entitled “A. Deconstruction of myths”. The thorough description in pp. 16-17, clearly outlines a hierarchical tree of components and research paths. Thus, the activity includes three components:

- Component A1. Deconstruction of the myth that climate and impacts of climate change are deterministically predictable.
- Component A2. Deconstruction of the myth that deterministic approaches can advance hydrological sciences and water resources technologies.
- Component A3. Deconstruction of the myth that classical statistics and stochastics provide a sufficient scientific basis for hydroclimatic processes.

Each of the components is then analyzed into research paths. For example, component A1 includes three research paths as follows:

- Path A1.1. Dispute of the predictive capacity of deterministic general circulation models, which are the basis of current analyses of hydrological and other impacts.
- Path A1.2. Analysis of the natural variability and the related predictive uncertainty and comparison with changes projected when hydrological models are fed by climatic model outputs.

- Path A1.3. Development of simple deterministic models to demonstrate the infeasibility of reliable deterministic climatic predictions.

Since the proposal submission, there has been growing factual evidence that the research programme is clear, realistic and realizable. Following the submission, the research team started to work on the programme and particularly on the path A1.1. As a result, we produced a poster paper for the EGU 2008 Assembly (April 2008) which we processed further and published the following scientific paper:

Koutsoyiannis, D., A. Efstratiadis, N. Mamassis, and A. Christofides (2008), On the credibility of climate predictions, *Hydrological Sciences Journal*, 53 (4), 671-684.

The paper corresponds precisely to the description given in the research proposal for path A1.1 except for the lower number of examined meteorological stations, which was dictated by lack of funding (as mentioned also in the paper). The paper was submitted and approved as a Rapid Communication and was published four days after the rejection of the research proposal.

The review committee may wish to do a Google Search with the exact title of the paper (“On the credibility of climate predictions”) to see the reactions on weblogs and internet forums and resources that the paper triggered. A list of the most interesting ones with the relevant links (more than 100, among which more than 30 devote a particular entry or article for this paper) are given in <http://www.itia.ntua.gr/en/docinfo/864/>. Most well-known scientific weblogs on climate and several popular weblogs have discussed this paper, which verifies the scientific and societal importance of the research, as well as its breakthrough character. Despite some attempts to depreciate the paper, the internet discussions and analyses eventually strengthened the validity of results and findings, because no flaw was spotted, and led to a growing acceptance of the Hurst behaviour in climate (long term persistence) which is central in the proposal. In addition, our perception of these discussions made us absolutely confident about the strong need for our research programme. For instance, the proposed new hydroclimatic theory (activity B) is strongly needed, given that currently even the very definitions of climate and climate change are deficient and perhaps not scientific (as described in the proposal and more recently in the aforesaid internet discussions, particularly in the first four links from the above web site).

### \*\*\* 2.C. INSUFFICIENT DESCRIPTION OF INTEGRATION OF TEAM WORK \*\*\*

Clearly the IDEAS programme focuses on the Principal Investigator. All proposal forms and the Guide for Applicants request information for the Principal Investigator only (perhaps the

reviewers were influenced by other EU research programmes). For this reason (and also due to space limitations), the proposal gives fewer details for the expertise and the contribution of the other members of my research team “Itia”.

However, the proposal:

- Clearly describes the expertise and the contribution of 5 key members of the team (p. 24) who are leaders of activities;
- Mentions all other members of the Itia research team, comprised of 20 scientists, and their expertise (p. 27);
- Specifies four new members to be added to Itia for the research needs of this project and their required expertise (p. 24);
- Names four visitors who will assist our research (p. 24); all are renowned, world leading scientists of European origin with relevant expertise;
- Describes the long (more than 20 years) history of Itia (pp. 2, 4, 27-28), which shows that it is not an occasional work team established for a specific project but a continuously operating scientific group;
- Stresses the “life-cycle” approach of Itia, thanks to its competences in problem understanding and analysis, in developing theoretical and mathematical descriptions and methodologies, in building algorithms, in implementing them in computer programs, in analyzing and explaining the results, and in using them in engineering design and management decisions (p. 28) and outlines a sample of research projects elaborated by the team (p. 2);
- Emphasizes, among the strong points of Itia, the intentness to transparency (pp. 2, 3, 4, 28), i.e. the implementation (since 1993) of the principle that all team activities, data and research results should be available on line, open to any interested scientist as well as to the general public; now this principle for transparency and open access has become an official policy of the EU and the ERC in particular.

The above information proves the long-standing coherence and true integration of the team, using historical facts rather than hypothetical plans, designed ad hoc, specifically for this project. It further demonstrates the ability of the team to produce specific scientific and technological outputs, which is a strong point of the proposal.

Itia's continuous presence and activity for more than 20 years and all its achievements had unfortunately only a minimal support of EU research bodies (p. 2), whereas, arguably, more support from EU research funds would be justified. By approving this application for redress and eventually funding the proposal, ERC will acquire, in the form of Itia, an ideal collaborator of proven scientific excellence, capable for producing frontier research and implementing healthy scientific and social principles.

I hope that the above arguments and evidence will persuade ERC to take a closer look at the proposal and recognize its breakthrough nature and global importance, which make it an IDEAL project to be funded under the IDEAS programme.