

Review

1. Scientific success of the project:

Rating:

Contribution and importance of the work to the further development of the scientific discipline, any importance for related disciplines (transdisciplinary questions and methods), quality of the dissemination of the scientific results (publications, activities at conferences and so on)

Comments

In the judgment of the referee, this has been a highly successful project. I wish to emphasize the importance of the five special topics explored in laboratory experimentation. In the general literature of experimental economics, there has been an overwhelming tendency to test models derived from classical economical theory. It is perhaps a mistake to say *theory* and to speak in terms of classical *theories*. I would like to make some additional comments of a more particular character regarding the "Bounds and Likelihood Procedure Revisited."

There are two features of merit that stand out: one is the strong empirical support of the surprisingly simple rule of choice and the second is the emphasis on the response being guided by the recent direction of the time series. In this reviewer's opinion, sensitivity to the recent past outcomes is critical to any theory and decisions or predictions developed for unspecified environments. There are too many examples in economics and also psychology in what is termed additive models that rule out such sensitivity. In the same vein, I like the Lotka Volterra experiments on analyzing how subjects tried to reduce the complexity of their task. In a similar vein the emphasis on interaction between a concept of fairness and the degree of punishment, found in the third study, is important.

2. Development of human resources in the course of the project

Rating:

Improvement of the project leader's standing in the relevant scientific community;
involvement of young scientists in the project work; development of international contacts

Comments

This project has been unusually successful in completing and publishing a surprising number of studies given the restricted budget. In my long career as a reviewer of many projects, it is hard to think of one that has been more successful. In this respect, on the basis of the publications reported here alone, this research group should have much greater international visibility than it did before the project.

3. effects of the project beyond the scientific field
(Section 3 of the report):

Rating:

Comments

Topics of four and five (voting in EMU and the management game) could easily have consequences beyond the direct scientific field of investigation. They both study aspects of behavior of considerable interest to managers of both public and private enterprizes as well as to citizens. The fourth study, particularly on voting procedures, contributes directly to growing public study of the financial institutions of the European Union.

4. Project performance (in the sense of efficient use of Available resources): Rating:
Were the goals achieved? Were changes to the original research plan sensible? What is the relation between the results obtained¹, the duration of the project and the resources available?

Comments

I have already commented above on the surprisingly effective use of resources in this project, as reflected in the large number of scientific publications. I am especially impressed with the large-scale experimentation. It is very desirable to have experiments of the kind reported here having several hundred subjects participating in the experiments. For this reason the goals of the project were more than satisfied.

5. Future perspectives of the research work: Rating:
Should the topic be pursued or should the scientists involved be advised to switch their attention to a different area? What goals does the referee feel should be set for future work?

Comments

In principle there is a basis for recommending continued pursuit of the five special topics of laboratory experimentation begun this far. As an outside referee I do not see any clear basis for recommending that one or two of these lines of work be abandoned. On the other hand it is important that the researchers themselves, who are the ones with the clearest insight about what will be productive in the future, should have the freedom to make a choice, which could well not involve all five special topics. I want to emphasize what I judge to be important in development of furture work. This is to reach for as much depth as possible in applying the concepts already studied. Experimental economics is still very much a new field. Unlike classical topics, like visual perception in experimental psychology the scientific literature is still relatively small. Greater depth is much to be desired at the present time.

¹ Please bear in mind that failure to achieve goals may sometimes also lead to significant findings.