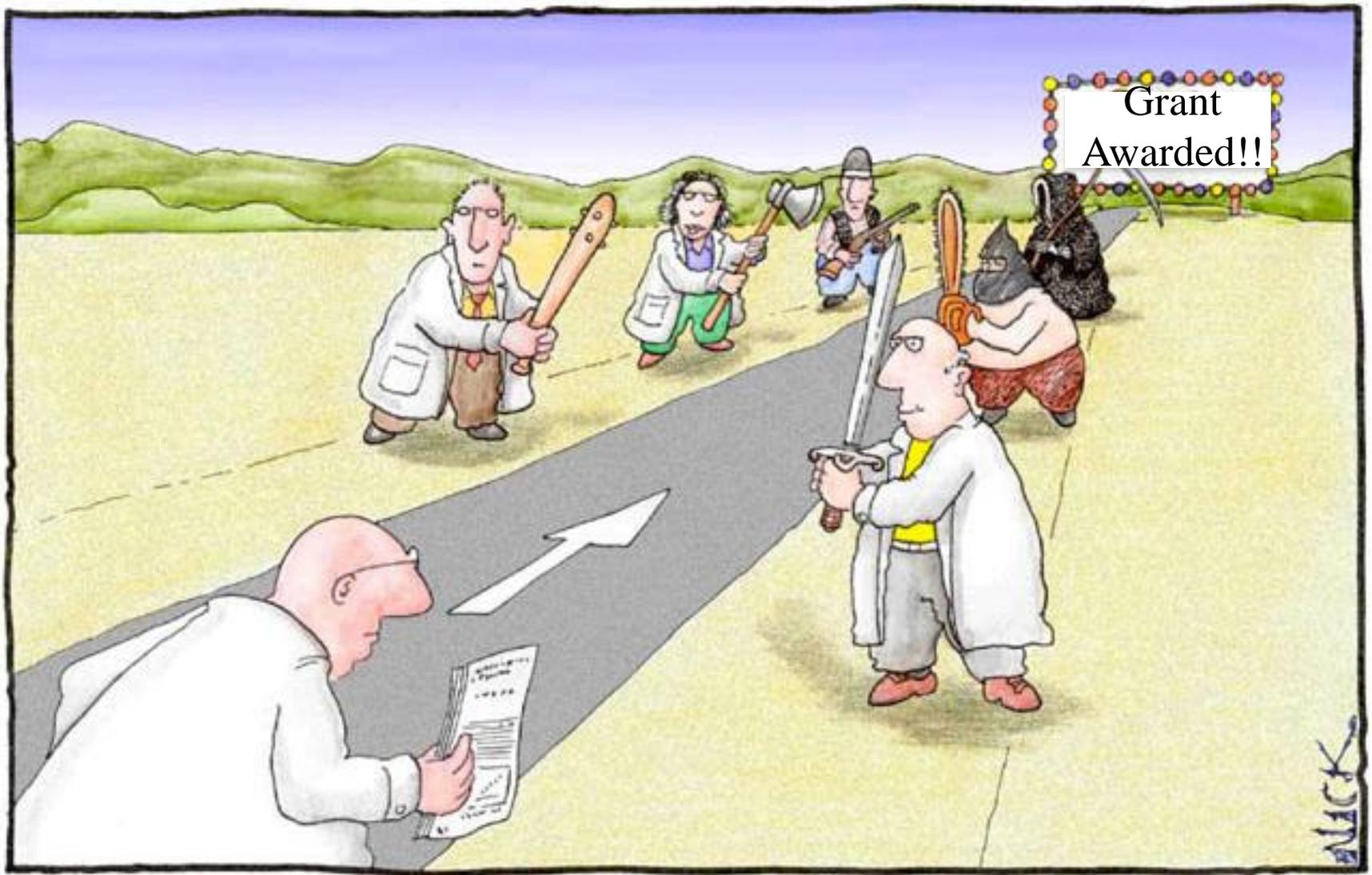


# Hitting the sweet spot with grant writing

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Are those really my *peers*??

# What you are scored on

- Overall impact
- 5 criteria
  - Significance
  - Investigator
  - Innovation
  - Approach
  - Environment

# Be sure these 4 categories are well-presented

- Overall impact
- 5 criteria
  - **Significance**
  - **Investigator**
  - **Innovation**
  - Approach
  - **Environment**

# Approach is where the bulk of the critique will focus upon

- Overall impact
- 5 criteria
  - Significance
  - Investigator
  - Innovation
  - **Approach**
  - Environment

- **“Problem solving” versus “problem finding”**
  - Choose an interesting/important question!
  - Concept of the perfect grant on an unimportant problem being suboptimal

## THEORETICAL NOTES

# Problem Finding: a Theoretical Note

J. W. GETZELS

*The University of Chicago*

Despite the self-evident role of problems in initiating thought and the function of new problems in guiding thought toward new solutions, very little is known about how problems are found and formulated. Although there are dozens of theoretical statements, hundreds of psychometric instruments, and literally thousands of empirical studies of problem solving, there is hardly any systematic work on problem finding (Getzels, 1964; Getzels, 1975; Henle, 1975; Getzels & Csikszentmihalyi, 1976). Indeed, *Cognitive Science* itself, to cite it as an instance of many other journals in the field of cognition, informs potential contributors that it publishes articles "on such topics as the representation of knowledge, language processing, image processing, question answering, inference, learning, problem solving, and planning" (see "Information for Authors"), but fails to make any mention of "question asking" or "problem posing"—as if questions and problems, like the weather, were just there naturally.

The purpose of this note—and it is to be taken only as a note—is to call attention to the relative neglect of the "problem of the problem" by offering some tentative observations regarding the significance of problem finding in thought, the nature and variety of problems, and the human being as problem-finder.

## ON THE SIGNIFICANCE OF PROBLEM FINDING

Need problems be *found*? Is not the world already teeming with problems and dilemmas at home and in business, in economics and in education, in art and in science? The world is of course teeming with dilemmas. But the dilemmas do not present themselves automatically as *problems* capable of resolution or even sensible contemplation. They must be posed and formulated in fruitful and often radical ways if they are to be moved toward solution. The way the problem is posed is the way the dilemma will be resolved (Getzels, 1975).

# Scoring Descriptions

Impact	Score	Descriptor	Additional Guidance on Strengths/Weaknesses
High Impact	1	Exceptional	Exceptionally strong with essentially no weaknesses
	2	Outstanding	Extremely strong with negligible weaknesses
	3	Excellent	Very strong with only some minor weaknesses
Moderate Impact	4	Very Good	Strong but with numerous minor weaknesses
	5	Good	Strong but with at least one moderate weakness
	6	Satisfactory	Some strengths but also some moderate weaknesses
Low Impact	7	Fair	Some strengths but with at least one major weakness
	8	Marginal	A few strengths and a few major weaknesses
	9	Poor	Very few strengths and numerous major weaknesses

**Minor Weakness:** An easily addressable weakness that **does not substantially lessen impact**

**Moderate Weakness:** A weakness that **lessens impact**

**Major Weakness:** A weakness that **severely limits impact**

# New NIH Scoring: Impact

“Is it worthwhile to carry out the proposed study?”

<http://www.niaid.nih.gov/ncn/newsletters/2008/1217.htm#n01>

# Impact =

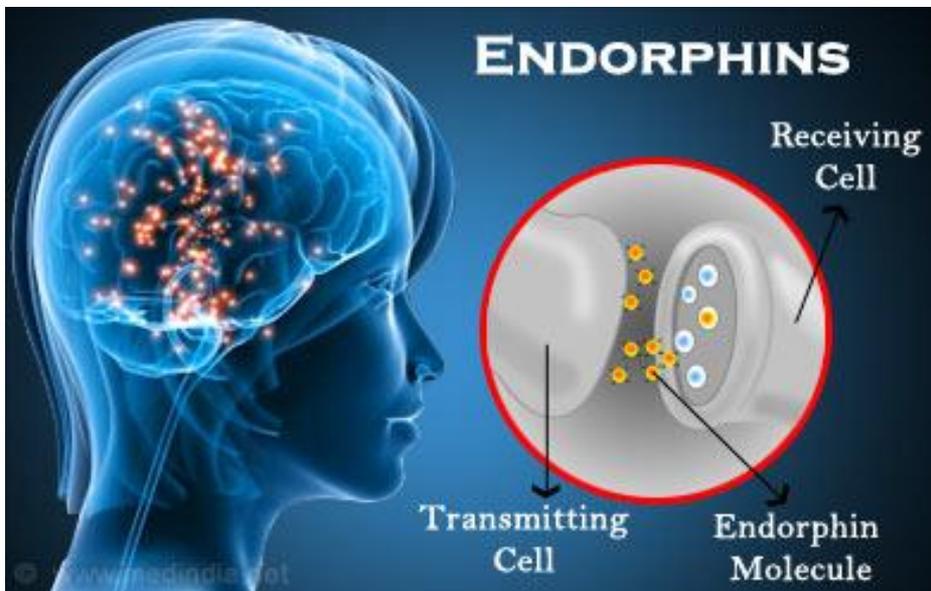
significance of the topic

+

the feasibility (reality) of your approach  
and likelihood it will make a difference in  
field.

[http://funding.niaid.nih.gov/ncn/newsletters/2009/  
1112.htm#n01](http://funding.niaid.nih.gov/ncn/newsletters/2009/1112.htm#n01)

# Create a proposal that causes endorphin release for your reviewers!



"IT'S NOTHING SERIOUS. HIS ENDORPHINS HAVEN'T KICKED IN YET."

# A few positive features in grants

- Pleasure to read
- Logical and organized
- Sometimes less is more—don't over cram
- Don't cheat on font size or spacing
- Figures and legends should be legible
- Cool and novel techniques/innovative
- Strong preliminary data that demonstrate key concepts and feasibility
- Interesting and important ideas

# Specific Aims

- Succinct and unambiguous questions/goals
- Aims should be inter-related, not dependent
  - Success with Aim 1 can't be necessary in order to execute Aim 2
- State what performing each Aim will accomplish
- Conclude: What will be the impact in the field
- Sweet spot for quantity: an aim shouldn't be one big experiment, but on the other hand shouldn't be enough for a complete grant on its own

# Background / Rationale

- Not an exhaustive literature search
- Build a story to form compelling support for the studies
  - Make it seem like a historical imperative that your proposed experiments are the next logical and mission-critical step
- Highlight key concepts (possibly by bold, italic text)
  - But don't annoy reviewers by over-using these highlights!

# How much preliminary data?

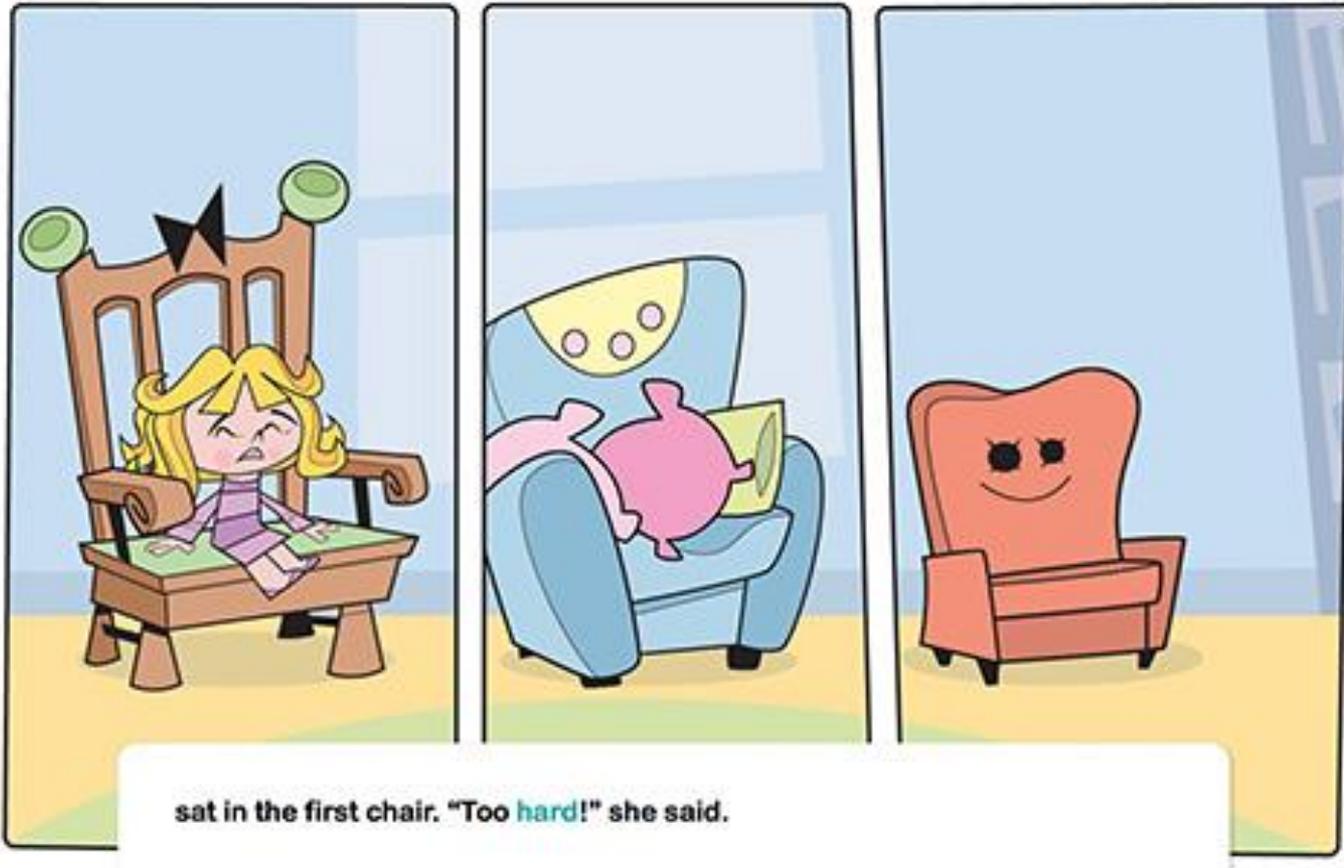


"C'mon, c'mon — it's either one or the other."

# Preliminary results

- You don't need to have “already performed the grant”
- Show key data supporting feasibility and rationale (especially if a new technique or model)
- Preliminary results should be solid and interpretable (including statistics)

# Goldilocks concept: finding the sweet spot for optimal amount of preliminary data



# Experimental Approach

- Emphasize the rationale
- Clarify and justify (defend) the choice of models (e.g. specific animal models)
- Design experiments to determine mechanism (think Koch's postulates)
- Clearly describe interpretation of results
- Don't waste too much space describing detailed methods—refer to published papers and show preliminary data for key techniques
- Clearly describe interpretation of results

# Feasibility!

- Demonstrate that you can do this (yourself and/or with appropriate collaborators/co-investigators)
  - Preliminary data with challenging techniques helps
  - Does not mean including extensive and tedious methodology
- Key relationship between feasibility and impact!

# Pitfalls / Alternative Approaches

- Be your own best critic!
- What can go wrong and what you do about it?
- What if you don't get the expected results?
- Consider alternative approaches and future directions
  - Can be conditional based on types of initial results obtained (if this then that)

# Re-submissions



# Re-submissions

- Take some time to digest the critiques and go over the “reviewers are stupid” first reaction to rejection
- Really listen to what the reviewers have said, but also read between the lines (the critiques are often short and telegraphic)
  - it may be solid with no flaws but just not exciting enough
- If there are ‘fatal’ flaws, reconsider your fundamental approach and/or questions
- Write a thoughtful introduction to the revised application
  - If you emotionally reject/rebel against the reviewers’ critiques it will not go well

# General Conclusions I

- Clearly answer: So What?
- Do I have a clear and important question/hypothesis?
  - descriptive/confirmatory experiments with no mechanism are not enough
- Can I convince the reader that I can do this?
- Do both 'positive' and 'negative' results have meaning?
  - difference between testing a hypothesis versus trying to demonstrate only one viewpoint

# General Conclusions II

- ‘Cosmetics’ matter: Carefully put together and edit
- If necessary, have someone review the English language usage
- Be explicit regarding conclusions (experimental or conceptual): Not ‘results will lead to new directions in the field’ .....What does that mean?
- A summary paragraph at the end of a grant can help: “After completion of these aims, we will have learned whether...”



