Peer Review of Transformative Research Proposals

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Transformative Research

- Kuhn & Scientific Revolutions
- Definition and Characteristics
- Two Major Structures for Supporting TR
- New Federal Programs for Supporting TR
- Proposal Evaluation & Statistical Distributions
- Project Update

Predicting Scientific Progress

October 9 1903

"*The flying machine which will really fly might be evolved by the continuous efforts of mathematicians and mechanicians in from one million to ten million years.*" – New York Times

"*We unpacked rest of goods for new machine.*" –Diary Entry, Orville Wright, NC

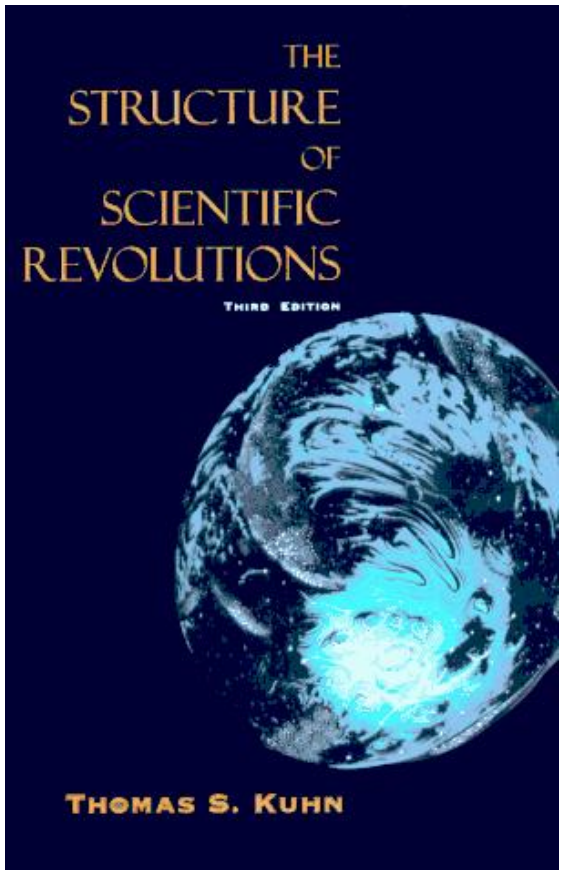
More Pronouncements

- "*There is no likelihood man can ever tap the power of the atom*." –Robert Millikan, Nobel Prize in Physics, 1923
- "*X-rays will prove to be a hoax."* – Lord Kelvin, president, Royal Society, 1883
- "*There is no reason anyone would want a computer in their home."* --Ken Olson, president, chairman and founder of Digital Equipment Corp., 1977
- "*Louis Pasteur's theory of germs is ridiculous fiction*." –Pierre Pachet, Professor of Physiology at Toulouse, 1872

Key Questions

- Is it possible to identify a potentially transformative research project? What are its characteristics?
- What program structures are available for supporting TR?
- Can a more analytical method be established to identify these proposals?

Kuhn: Structure of Scientific Revolutions



Definition

Transformative Research (TR) = research driven by ideas that have the potential to radically change our understanding of an important existing scientific or engineering concept or leading to the creation of a ***new paradigm*** or field of science or engineering. ***Such research also is characterized by its challenge to current understanding or its pathway to new frontiers.*** (NSB 2008).

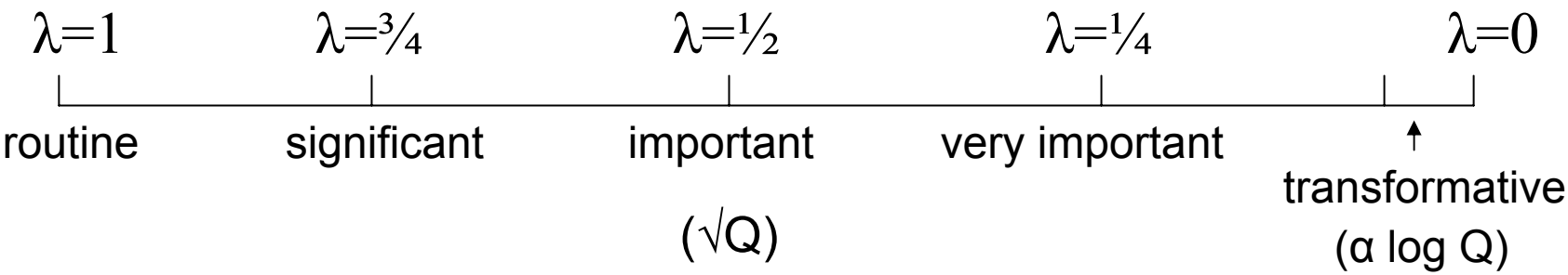
Characteristics

- TR Associated Terms: High Risk / High Impact / Innovative / Frontier Science / Paradigmatic / Revolutionary / Multidisciplinary / Bold / Disruptive
- TR is **not** the very best research that has been the result of incremental progress via well-established science (normal science)
- TR is **not** common and quite rare

NSF Proposals of λ–quality

Q = Total NSF grant applications per year

Q^{λ} = Total quality applications worth supporting, for $0 \leq \lambda \leq ½$:



Revolutions need momentum

- Funding scattered projects is not likely to create momentum
- Challengers are thwarted by prevailing conservative orthodoxy
- Challengers also represent threat to losing money
- Bias against speculative research

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Grants for TR: DARPA

- Relies on a "***strong manager***" role
- Supports ***projects***
- Admired today, as evident in the recent ARPA-E approved for DOE (Gathering Storm Report)

Grants for TR: MacArthur

- The “Genius Grant”
- Relies on ***vetted experts*** for nomination and review
- Supports ***individuals***

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TR Program	Process Structure	Strategy	Focus	Date of Inception	Awards per Year (approx)	Grant Amount per Year / Duration
DARPA	*Strong Manager*, White Papers & Competition	Mission Oriented	Project	1958	Varies, (Classified) Q=? (10%) $Q^{\lambda} = 3$?	Varies (Classified) $200K min / 2-3 years
MacArthur Fellows	Nominations & Panel Review	Independent	Individual	1981	20 / 500? (4%)	$100K / 5 years
NSF EFRI	White Papers & Competition	Targeted Fields	Project	2006	12/ 500? (2%)	$500K / 4 years
NIH Pioneer Award	Open Competition, Panel Review, Interviews	Mission Oriented	Individual	2004	13/500 (2%)	$500K / 5 years
UK Wellcome Trust Showcase Awards	Open Competition, Panel Review	Mission Oriented	Project	1996	20/507 (4%)	$100K / 1 year
European Science Foundation EURYI	Nominations (by country) & Panel Review	Independent	Individual	2004	20/450 (4%)	$200K/ 5 years
NSF SGER	*Strong Manager*	Targeted Fields	Project	1990	272/300 (86%)	$100K / 2 years
NSF	Open Competitions	Targeted Disciplines	Project	1950	N = 40000 Q = 10000 (25%) $Q^{\lambda} = 3$?	Varies, $50K / 3 years
NIH	Open Competitions	Targeted Disciplines	Project	1930	N = 46000 Q = 9100 (20%) $Q^{\lambda} = 3$?	Varies, $75K / 5 years
ARPA-E	*Strong Manager*	Mission Oriented	Project	2007	New	New

Increased Federal Attention for TR

- “Unfettered Exploration” – 2005 DoD Report
- NIH Roadmap makes it explicit the need to stimulate high-risk/high-impact
- “Gathering Storm” – NAS Report warns about “the recent decline in support of high-risk or transformative research.”
- 1999 to 2007 NSB Workshops – urged NSF to make TR a cross-divisional priority and expanded role of SGER

NSF Emerging Frontiers in Research and Innovation (EFRI)

- *"Does the proposed topic represent an opportunity for a significant leap or paradigm shift in a research area, or have the potential to create a new research area?"*
- Solicits white papers and targets RFP
- Reviews and scores proposals
- Supports *projects*

NIH Director's Pioneer Award (NDPA)

- "*The term 'pioneering' is used to describe highly innovative approaches that have the potential for producing an unusually high-impact.*"
- Reviews and scores proposals
- Conducts Finalist interviews
- Supports *individuals*

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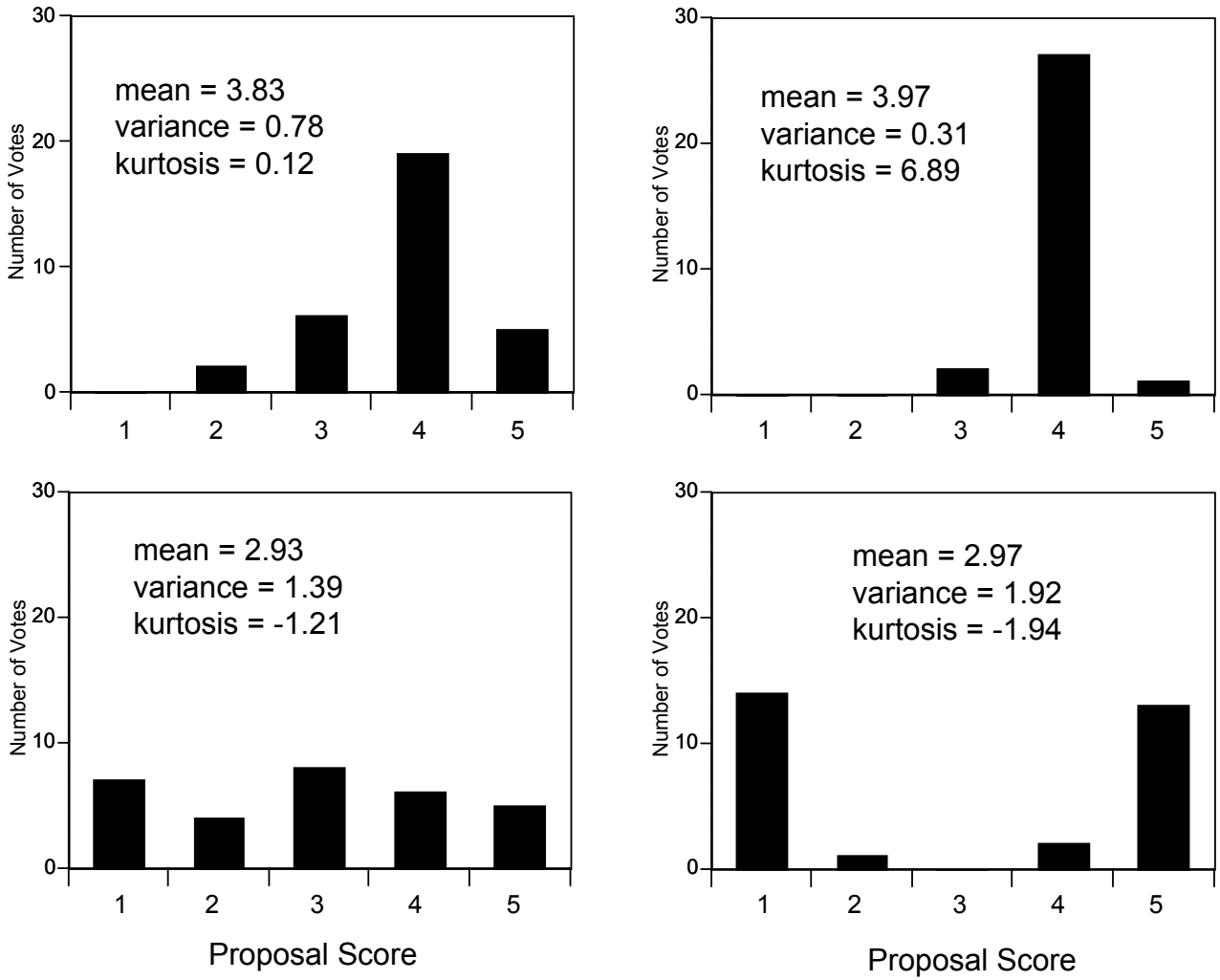
Selection Challenges

- No currently accepted measure for what constitutes TR – ***"I know it when I see it."***
- Scientists often confuse the ***excellent*** with ***exceptional***
- Programs fail to make use of sampling and statistical measures to distinguish TR
- Not all TR projects can be treated equally

Proposal Scoring & Statistical Trends

- TR proposals are often controversial (Kuhnian crisis)
- Controversy is often reflected in scoring spread
- Kurtosis Analysis, using a well calibrated scoring system, could be used to identify presence of TR

Statistical Distributions



TR Statistical Hypothesis

- David Kaplan suggests distributions with scores accumulated in the tails (*platykurtic*) are more likely to have TR proposals
- Kuhnian theory supports Kaplan's assertion; new paradigms create controversy and show dispersion in scoring

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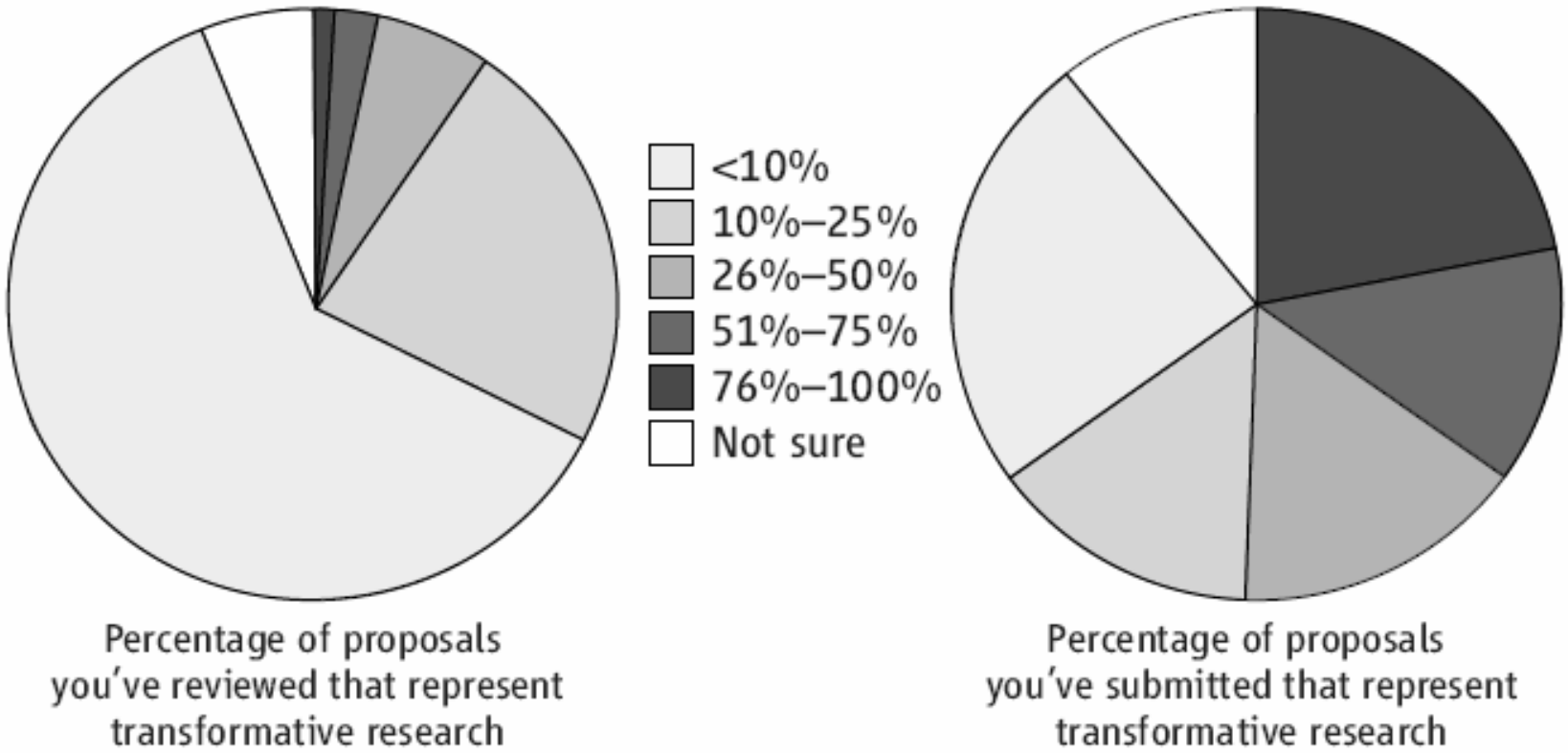
Next Steps

- E. Yglesias (STPI), D. Kaplan (Case Western) and S. Shah (Harvard)
- Test Kaplan's Statistical Hypothesis
- Negotiate use of data from science funding programs
- Confirmation of the hypothesis would (1) provide new statistical tool to assist TR programs, and (2) validate Kuhnian theory

Road Ahead for TR Programs

- As programs mature and their impact evaluated (*ex post*), more precision is expected from proposal evaluation of TR (*ex ante*)
- However, recent survey evidence indicates scientific community is not capable of identifying TR

NSF PIs Like Themselves More!



Citation: Science, 17 August 2007: Vol. 317, no. 5840, pp. 880-881 / NSF PI Proposer Survey

Conclusions

- Despite proliferation of TR programs, current methods of identifying TR are rudimentary and pro status-quo
- There is value in identifying TR prospectively by using tested statistical methods
- Formal testing and reporting of Kaplan's hypothesis is forthcoming

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