Subject: resistance to scientific discovery
To: Juan.Campanario@uah.es
From: lederberg@mail.rockefeller.edu
Fcc: TXT, /jm/BIOG

Sun Feb 8 15:17:19 EST 2004

Dear Prof. Campanario:

I was interested to get your letter dated 15 Jan '04.
I have been interested in this topic at least since Bernard Barber's paper (perhaps earlier statements on his part):

AU BARBER, B
TI RESISTANCE BY SCIENTISTS TO SCIENTIFIC DISCOVERY - THIS SOURCE OF RESISTANCE HAS YET TO BE GIVEN SCRUTINY ACCORDED RELIGIOUS AND IDEOLOGICAL SOURCES
SO SCIENCE
VL 134: 596 - & ; 1961

This enjoys 173 citations on the Science Citation index; so there is indeed a venerable history, in sociological discussion. For some scientometric approaches to assessing resistance and delayed recognition, you surely must engage: (and visit his home page)

Eugene Garfield, PhD.
Chairman Emeritus, ISI  www.isinet.com
Publisher, The Scientist  www.the-scientist.com
email: garfield@codex.cis.upenn.edu
home page: www.eugenegarfield.org

The related theme: "premature discovery" has been ventilated at length by Gunther Stent, see the Festschrift:

Ernest B. Hook, Editor
Prematurity in Scientific Discovery
On Resistance and Neglect
Publication Date: October 2002

And you can search the web under the rubric and find 476 hits.

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My own sociological research has been on postmature discovery. I send you some reprints bearing thereon
See also:
http://profiles.NLM.nih.gov/BB/A/B/P/N/
The outstanding examples of (alleged) resistance in biology are

Mendel 1865 -- buried until 1900
Avery et al 1944 -- genes as DNA -- contentious until about 1953
McClintock 1951 -- jumping genes.

I accept the Mendel "story" There is a large literature on it.

I have extensive debate with Stent about the Avery "story"
N.B.
http://profiles.NLM.nih.gov/CC

McClintock is more complicated; Nat Comfort has gone into some
detail in his book:
Nathaniel Comfort
Barbara McClintock’s Search for the Patterns of Genetic Control

No one doubted her data; but her interpretations had nuggets of brilliance
too easily overshadowed by unproductive speculation.

I believe resistance is indispensable, though scientists are often too
quick to dismiss or accept problematical work on a black/white dichotomy.
Peer review works pretty well. It’s unimportant that journals like
Science and Nature are sometimes arbitrary in what they accept, as long
as innovative work can be published somewhere. The same principle
should also govern funding: appeal to multiple founts, and some way
to discount a veto by a single skeptic. The internalized dampening
of ambition is probably the worst consequence of failings in peer review.
I agree with what you write about persistence.

As to my own experience:

1. I’ve encountered reasonable skepticism, but nothing daunting.
   Cf. my disputation with Andre Lwoff (1946) -- I send you reprint.
   I’ve been turned down by Nature/Science on relatively unimportant
   matters, but never by a professional specialty journal. Having won
   a Nobel Prize at age 33 (12 years after I did the work) I can scarcely
   complain about delayed recognition. My work had the advantage it
could be easily repeated in other labs, and it was. Worse to be ignored!
than to be disputed!

As to 3.1: see ellisonfoundation.org to see how that private funding
agency tries to encourage innovative ideas. Basically it’s another
appeal body, committed to seeking diamonds in the dust and willing
to take risks.
After you’ve received my reprints, let me know if you have more questions.

Prof. Joshua Lederberg
Raymond and Beverly Sackler Foundation Scholar
Suite 400 (Founders Hall)
The Rockefeller University
1230 York Avenue
New York, NY 10021-6399
phone: 212: 327-7809
fax: 212: 327-8651
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