

## 2005: A Remarkable Year to Remember

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### HIGHLIGHT

In communicating scientific discoveries and ideas, *Logical Biology* sets a new standard and format. Its adherence to logical reasoning as a major principle, as compared with obeying the authority as the only principle, for selecting a manuscript for publication helps it bravely and early report many important discoveries. This logical reasoning also helps it become an active “watch dog” for monitoring “top” journals and identifying their mistakes when they are still perceived as glory scientific advancements. Using a personal reflection this closing remark shows some examples of such achievements in *Logical Biology* in 2005. It is hoped that by objectively evaluating some publications in *Logical Biology* and some related publications in the “top” journals one can come to a reasonable appreciation to this new journal and raise some cautions against blindly trusting the “top” journals.

### KEY WORDS

*Logical Biology*, Top journal, Communication, Truth

Sitting on the airplane for a cross-ocean trip I had no thing to do but to reflect what happened in this year to me, to *Logical Biology*, and to the science that I care of the most.

To me, 2005 represents a very productive year of my spare time scientific activities. This was not because that I had done a lot of experimental studies this year but rather finalized many half-done manuscripts accumulated over the last fifteen years.

In 2004 and the beginning of 2005 (with the manuscript submitted in 2004) I published a series of papers on bacterial/cell age synchronization (*Logi. Biol.* 4: 1-6, 7-15, 16-27, and 88-101, 2004; *Trends Biotechnol.* 23: 9-10, 2005). Then, my sixth sense told me that I should also publish my other more insightful ideas along those lines because I felt something important might happen in 2005. A submission of a hypothesis paper to *Nature* at the beginning of 2005 was intended to serve as the opening of a new round of my information flow into the public domain, hopefully via the communication channels of the “top” journals. However, I was wrong again in my assessment of the likely perception by the “top” journals to my scientific ideas. My plan of “sleeking” into the

“top” journal was again blocked at its very beginning (*Logi. Biol.* 5: 254-271, 2005). This time, even a very reasonable and greatly important hypothesis was not allowed to be communicated there. The reason for rejecting my hypothesis paper by *Nature* was very interesting and truly amazing; *Nature* believed that “the ideas presented do not seem to be of sufficiently broad interest or topicality to our general readership”.

However, that conclusion made by the *Nature*'s editors was in sharp contrast to the great interest shown even by the other top journals on the exactly the same topic. *PLoS Biology*, a leading new “top” journal, actually published a so-called “new” discovery (3: 230-235, 2005) along the line of my thinking right at the same time that *Nature* rejected my paper. More interestingly, a topic that *Nature* considered as of no general interest was heavily “media-spun” by the other top journals such as *Science* (307:656, 1015-1016, 2005) and other magazines and newspapers (e.g. *The Scientist* on February 3, 2005; *The New Scientist* on Feb. 5, 2005). The *PLoS Biology* paper on bacterial aging was in fact such a big hit that it remained as the number one read paper for quite a while in *PLoS Biology*. *Nature*'s editors were fully aware of those developments (I actually mentioned the above publications when I appealed to their initial decision and clearly stated that I would accept a rejection decision on the ground that *Nature* did not believe in my hypothesis but not the so-called reason of “no general interest”). However, *Nature*'s final decision on rejecting my hypothesis was still on the same reason of no general interest (speaking of the toughness of the “top” journals, I could find no other better example than this).

Mainstream journals' warm reception toward a later flawed work (*Logi. Biol.* 38-47, 2005) formed a sharp contrast (*Logi. Biol.* 5: 73-75, 2005) with their cold attitude toward my earlier pioneering works (*Logi. Biol.* 5:254-271, 2005). Worse than this, no person seemed to care about the lies presented by the authors of that *PLoS Biology* paper and the erroneous analyses contained in that paper. My appeals to expose the ethical misconduct and the scientific mistakes by the authors of that paper were repeatedly rejected by the various top journals and magazines (*Logi. Biol.* 70-72, 76-78, and 226-234, 2005). As to the authors of that paper, they just pretended that nothing had happened by ignoring my direct criticism and my request for them to respond to my criticism. These situations made me wonder whether these authors are true scientists and whether some of the so-called “top” journals really care about the truth and ethics in science.

It turned out that 2005, rather than becoming a year for my shining moment in the “top” journals, was a year that I collected the most numbers of the “shameful” rejection letters from the “top” journals. However, I am not ashamed of receiving so many rejections at all. In fact, I was determined to collect more and more such “stamps” so that I can present a meaningful display of them.

After a year of hard work to get these many depressing rejection “stamps”, now it is a high time to review these valuable collections especially in the holiday period with an elevated spirit. In doing so, I tried to compile this list of rejections by some categories (Table 1). I also listed the relevant papers side-by-side. I hope that readers can get some ideas on what went wrong in today's scientific communication, especially in some “top” journals by looking at what were rejected (for me) and what were published (by them).

**Table 1. Side-by-side comparison of selected papers by Liu published in *Logical Biology* and others' papers published in some "top" journals in 2005 (with a few 2004 exceptions).**

Liu's papers			Others' papers	
Titles	Rejected by	Published in Logi. Biol.	Titles	Published in "Top" journals
Linking DNA aging with cell aging and combining genetics with epigenetics	<i>Nature</i>	5:51-55	(No publication has made such firm and detailed prediction)	
Understanding the limit of the Hayflick limit	<i>Nature</i>	5:58-65	(No new insights given by others in 2005)	
Right direction but backward movement: A new finding or a flawed repetition in bacterial aging study?	<i>PLoS Biol.</i> *	5:38-47	Aging and death in an organism that reproduces by morphologically symmetric division	<i>PLoS Biol.</i> 3:295-300
Searching for the deep root and fundamental mechanism of biotic aging	<i>Cell</i>	5:89-91	Reviews on Aging (a collection of multiple reviews)	<i>Cell</i> 120 No. 4
Counting human age: real versus hypothetic	<i>Nature</i> **	5:203-209	Average remaining lifetimes can increase as human population age	<i>Nature</i> 435:811-813
Stop reinforcing misinformation in microbiology	<i>ASM News</i> **	5:289-293	Bacterial senescence, programmed death, and premeditated sterility	<i>ASM News</i> 71:363
An illogical and unscientific argument against Liu's bacterial/cell life model and its implication for cell synchronization	<i>Trends Biotechnol.</i> ***	5:335-349	( <i>Trends Biotechnol.</i> published a series debates on this issue in 2004)	<i>Trends Biotechnol.</i> 22: 266-269, 270-273, 274-276, 277-278
"Cellular senescence": what does it really mean?	<i>Cell</i>	5:308-310	Understanding the odd science of aging ; Senescent cells, tumor suppression, and organismal aging	<i>Cell</i> 120:437-447; 513-522
			Tumour biology: Senescence in premalignant tumours	<i>Nature</i> 436:642-643
Suppressing cancer: do not confuse inhibition of cell reproduction with cell senescence	<i>Science</i>	5:311-312	Suppressing Cancer: The Importance of Being Senescent	<i>Science</i> 309: 886-887
A high time to unify biology under common life principles	<i>Science</i>	5:66-69	(Too many to list)	Many "top" journals
Caveats for pattern formation in synthetic "multicellular" system	<i>Nature</i> **	5:175-179	A synthetic multicellular system for programmed pattern formation	<i>Nature</i> 434:1130-1134

Cell differentiation in <i>Caulobacter</i> : fact or fiction?	<i>Science</i>	5:313-319	Too many such papers to be individually listed here	E.g. Shapiro and Losick's papers
A theoretical framework for understanding biotic aging from molecule to organism in multicellular life	*****	5:109-116	(No prior publication has made such a clear depiction and presented such deep insight)	
Understanding our differences: The full-house view of whole genomes	<i>Nature</i>	5:184-187	Large genomic differences explain our little quirks	<i>Nature</i> 435: 252
See cells but think beyond cells	<i>Nature</i>	5:210-211	Now you see it, now you don't	<i>Nature</i> 435: 1165
DNA barcoding enhances traditional taxonomy	<i>Nature</i> ****	5:140-141	DNA barcoding does not compete with taxonomy; DNA barcoding generates information, not knowledge	<i>Nature</i> 434: 697; 435: 17
Not just a name game	<i>Nature</i>	5: 212-213	Playing the name game	<i>Nature</i> 436: 2
Correcting false impact: Actions speak louder than words	<i>Nature</i>	5:217-218	Not-so-deep impact	<i>Nature</i> 435:1003-1004
Chasing trends and pressing hot buttons: A typical case of top journals' low scientific standard	*****	5:235-237	Ivory-billed woodpecker persists in continental north America	<i>Science</i> 308:1460-1462
Communicating hypotheses: Barrier and bias	<i>Science</i>	5:107-108	Testing hypotheses: Prediction and prejudice	<i>Science</i> 307:219-221
Remember the true character and inherit the right spirit of Einstein	<i>Science</i> <i>Nature</i>	5:104-106	Collections of papers celebrating the "Year of Einstein"	<i>Science</i> and <i>Nature</i>

\* A request for publishing some kind of corrections on the missing citation of my early works was firmly denied by the editor of *PLoS Biology*. Under such situation, I decided not to try submitting this criticism to *PLoS Biology*. The authors and editors of the criticized *PLoS Biology* paper were informed on this publication but so far they have chosen to ignore it.

\*\* This symbol indicates that the authors and editors of the publications in the "top" journals were both aware of my criticism. After my criticism was rejected but published in *Logical Biology*, the authors of the "top" journal papers were invited to join the debate in *Logical Biology* but such invitations were denied or simply ignored.

\*\*\* This symbol represents a situation where a paper criticizing my proposals was initially accepted for publication by a "top" journal but then that decision was retracted after the journal fully evaluated my counter criticism submitted to that journal (against the will of the journal). I appealed the journal to publish the criticism on my proposals even if my counter-argument is not published there. However, this suggestion was not taken. The invitation to the author to publish his criticism on me entirely as he had written has not been accepted even after months of repeated requests.

\*\*\*\* This symbol indicates a later submission was published by the "top" journal which had rejected my earlier submission on the same topic.

\*\*\*\*\* This symbol indicates a decision to publish in *Logical Biology* was made because of the very "fresh" rejection experience from the "top" journals on the similar papers or situations.

The above “long” list is actually a partial (a very tiny part) reflection of “top” journals’ long tradition of riding on trends and pushing on “hot buttons” but rejecting truly novel and enormously great discoveries (*Logi. Biol.* 5:235-237, 2005). Now with enough first-hand and very fresh rejection experience, I can confidently say that “top” journals often do not have a high scientific standard and some times even had no scientific standard at all in their selections for publication.

It is not a secret that “top” journals have their “clubs” (of favorite or even commissioned authors). I first heard of this “club member” preference in 1997 when I tried to convince my boss to submit a discovery to a “top” journal. He told me that we would have no chance because he was not a member of that “club”. Now, with so many direct experiences, I fully understand the practices of some “top” journals to allow authorities or their friends to say anything even if it is extremely ridiculous but to decline “nobody” or their opponents to say something even if it is very logical. In fact, this “club” member policy has been expressed one way or another by some “top” journals in their Instructions to Authors. Some sections of the “top” journals are actually the reserved areas for those invited or commissioned “experts”. The chance of other ordinary authors to make their expression there is very low and may occur only under the “mercy”. However, if the content of the submission is offensive to the “top” journals or to the high-level figures in the “top” journals, it is unlikely any ordinary author will get such a “mercy”.

“Top” journals had instituted not only an authority-orientated criterion for selecting who’s paper to publish but also an authority-protecting iron wall for rejecting reasonable challenge to the authorities’ publication in their places. From the small collections of the communication arising or technical comments that I had submitted to but were rejected by the “top” journals (Table 1), it becomes self-evident that “top” journals often do not care about whether their publications contain the truth or not. Even if their publications contain obvious mistakes, they usually do not publish or do not publish as many as necessary criticisms on those papers. Once a while, “top” journals also publish corrections and even retractions. But, many such retractions were the unavoidable consequences of some misconducts or scandals already highly exposed (often initially by the other journals but not the “top” journals). Interesting, once a scientist was implicated in a scandal in one publication, very often his/her publications would suddenly all become suspicious and were even indiscriminately retracted in a “batch”. This “snowflake” publishing (of the “hot” researches by the “rising stars”) and then a “landslide” retraction (of the “tarnished” researches by the “falling stars”) has become a “hallmark” for some “top” journals. I do not deny the fact that “top” journals have published many “top” discoveries. But they also published some “top scandals”. Thus, when “top” journals proudly speak of their “top” impact, they should realize that their total impact also includes their huge negative impact on science. The careless or even unethical scientists surely should be held responsible for their incorrect or even false publications. But the editors and reviewers failing in identifying these bad “science” should also share some responsibility. “Top” journals should actively teach public that the publication per se in fact cannot verify the truth or false of the published work even with the most knowledgeable and seasoned reviewing panels. Unfortunately, it has become an infection of mind that a stereotype forms which automatically equates the place of publication with the quality (truthfulness) of the publication. People even make

arguments in such way as “This is published in so and so “top” journal (meaning it has to be very right). Such a blind confidence on the “top” journals should be stopped. I hope by comparing the selected content published in some “top” journals and in *Logical Biology*, one should come to such an understanding - “top” journals, so what?

Because of my frequent submissions to the “top” journals in 2005, especially the submissions of short letters/comments on the papers published in the “top” journals, I could literally felt me being increasingly disliked by the “top” journals. Most times, I submitted my short letters/comments right after (on the same day) of the publication of those papers. However, my submissions were rejected but some later submissions by the other authors were published by the “top” journals which actually contained some same or similar views as in my prior submissions. To me, such selection of later submission of similar contents for publication is a misconduct of the editors and really showed to me that there is no ethical rule in editor’s selection for publication.

In making the final touches on this reflection, I was talking with my old classmate with whom I shared a double-level bed during our medical school study 28 years ago. After hearing these many rejections that I received in a short year from the “top” journals, especially the rejections of my earlier submissions but the publications of the others’ later submissions by some “top” journals, he told me that I might have been included in the “black lists” of these “top” journals. I am not sure about this. But my almost once-a-week and year-long submissions should leave some impression on some editors. As my wife had joked, some editors might have already remembered me as a crazy person and would simply trash my submission without a looking into its content.

Well, someone’s trash may be other one’s treasure. “Trashes” discarded by the “top” journals have all been collected into my “jewel box” of *Logical Biology* and the other new generation scientific journals such as *International Medicine* (<http://im1.biz/im>). The values of these collections may still very low because the low visibility of *Logical Biology* which so far still remains as a lonely star in scientific sky. However, I am confident that the bright shining moment of this star will come eventually because truth will finally defeat falsehood.

(This “global” reflection was started on the west side and finished on the east side of the Pacific Ocean.)