90% of the opinions of the top journals of Cell/Nature/Science are incorrect.

English translation of the views of Nobel laureate Tasuku Honjo from Chinese web sites (by machine translation)

For ref. [19] in Ying Liu, Yue Liu, and Michael GB Drew, A theoretical investigation on the quarter-wavelength model — part 1: analysis, Phys. Scr. 96 (2021) 125003.

[19] Nobel laureate Tasuku Honjo: **90% of the opinions of the top journals of CNS are incorrect**, in Chinese. It can be translated into English easily by machine translation. (https://sohu.com/a/423577113_788170)

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诺奖得主本庶佑：CNS 这些顶刊观点有九成不正确，不要盲从迷信，搞科研做到六个 C 更重要

Nobel laureate Tasuku Honjo: **90% of the opinions of the top journals of CNS (Cell/Nature/Science) are incorrect.** Don't blindly follow superstition. It's more important to do scientific research with the six C's

01. 真正一流的工作往往没有发在顶刊上

现在，特别是在 **年轻人中间，有一种倾向，认为文章发表在有名的刊物上就是一流的工作。**确实，以前日本的学者在顶级刊物上发表的文章不多。但是， **与许多人的想象不同，真正一流的工作往往没有在顶级刊物上发表。**这是因为，**一流的工作往往推翻了定论，因此不受人待见，评审员会给你提很多负面的意见，你的文章也上不了顶级刊物。**迎合时代风向的文章比较容易被接受，否则的话， **需要花费较长时间才能获得认可。**（2000 年）

1 Really first-class jobs are often not published in top journals

Now, especially among young people, there is a tendency to think that publishing articles in famous journals is first-class work. Indeed, in the past, Japanese scholars did not publish many articles in top journals. But, unlike many people's imagination, real top-notch jobs are often not published in

top journals. This is because first-class work often overturns the established conclusion, so it is unpopular. The reviewers will give you a lot of negative opinions, and your articles will not be published in top journals. Articles catering to the trend of the times are easy to be accepted, otherwise, it will take a long time to get recognized. (2000)

如果你的研究不能推翻定论，科学也就不能进步。当然，你的研究也不会载入史册。 **学术的世界是保守的。如果你不按现有的定论来写论文，你的论文就很难获得肯定，你也会吃到不少苦头，但能够载入史册的研究都是这种研究。（2013 年）**

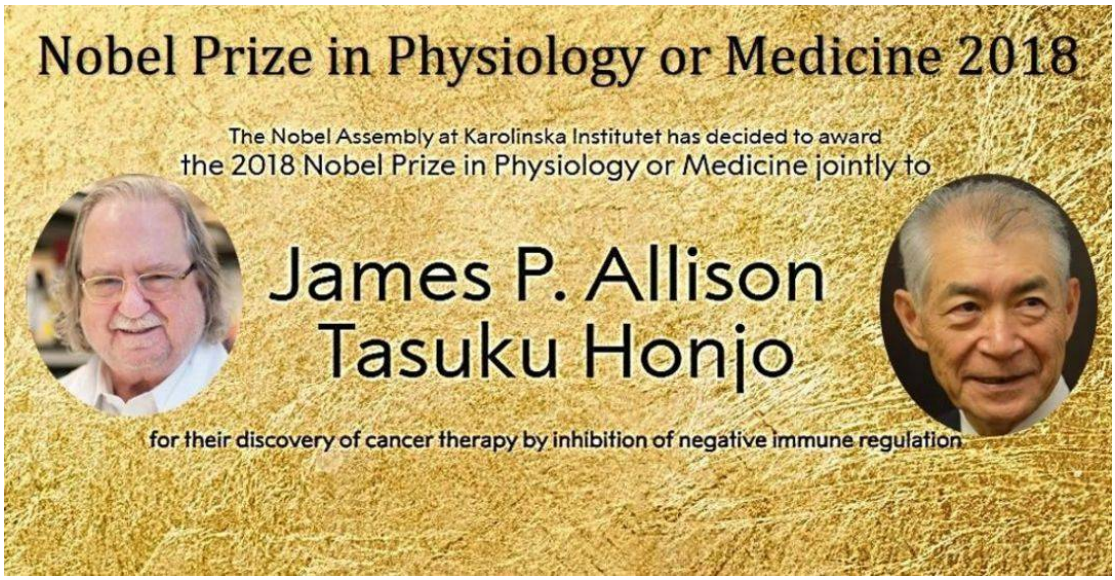
If your research can't overturn the established conclusion, science can't progress. Of course, your research will be not recorded into history. The academic world is conservative. If you don't write your paper according to the existing conclusion, it will be very difficult for your paper to be accepted, and you will suffer a lot, but the research that can survive in history is exactly this kind of research. (2013)

我认为，发表在 CNS（Nature/Science/Cell）《细胞》《自然》和《科学》上的工作未必就是好研究， **倒是被《细胞》《自然》和《科学》拒绝的时候，你的研究或许才是真正一流的工作。**你既然选择了做一名研究者，就应该力争打开新的局面，做别人从没有做过的工作，或力争将现有的定论推翻。研究者要认识到，这才是第一流的研究。我认为， **为了让论文更容易被知名刊物接收而做的研究，绝不会是很好的工作。**（2000 年）

In my opinion, the work published in CNS (Nature / Science / Cell) may not be good research, but when it is rejected by Cell, Nature and Science, your research may be the real first-class work. Since you have chosen to be a researcher, you should strive to open up a new situation, do work that other has never done, or try to overturn the existing conclusion. Researchers should realize that this is the first-class research. I don't think it's a good job to do research to make papers easier to be accepted by well-known journals. (2000)

我认为 **《自然》《科学》这些杂志上的观点有九成是不正确的，论文发表十年之后，还能被认为是正确的只剩下一成。首先，不要相信论文里写的东西。对于研究，要一直钻研到眼见为实、让自己确信为止。这是我对科学采取的基本做法。**也就是说，用自己的大脑思考，一直做到自己完全想通、完全认可为止。（2018 年）

In my opinion, 90% of the views in the journals such as Nature and Science are incorrect. Ten years after the publication of the paper, only 10% can be considered as correct. First, don't believe what's written in the paper. For research, we should always study until you are sure about the conclusion. This is my basic belief in doing science. Think with your own brain, until you have fully figured it out and fully recognized it. (2018)



02.关于研究：“与其第一，不如唯一”

2 On research: "it's better to be unique than the foremost"

京都大学有一个传统，那就是 **“与其第一，不如唯一”**。这一点对于生物学研究来说非常重要。对于自己的发现，持续地研究下去，从那里扩展开来，世界就会变得很宽阔。这一点就是我做研究的乐趣。不是说我看到别人挖到了金矿，就马上跟进去凑热闹，成为众多淘金者中的一员，而是 **沿着自己已有的发现**，持续地深挖下去。这样，反而是其他研究者会凑过来研究你的课题。（2000 年）

Kyoto University has a tradition that "it is better to be unique than to be the foremost among others". This is very important for biological research. If you continue to study your findings and expand from there, the world will become very broad. This is my pleasure in doing research. Don’t be in such a practice that when I see someone else digging a gold mine, I immediately follow in to join in the fun and become one of the many gold miners. Instead, I continue to dig deep along with my own discoveries. In this way, other researchers will come to study your project. (2000)

研究者最大的乐趣，打个比喻来说，就是发现其他所有人都视而不见的小涌泉，把它培养成小河，再拓宽成大河。或者说，就像是闯入深山， **在无路处开出一条路，第一个在那里搭起了一根独木桥，而绝不是把别人已经搭好的独木桥改建为钢筋水泥筑成的大桥。**（2003 年）
The greatest pleasure of researchers, to put it metaphorically, is to find a small spring that everyone else turns a blind eye to, cultivate it into a small river, and then broaden it into a big river. In other words, it's like breaking into a deep mountain and opening a road where there is no road. The first one is to build a wooden bridge there, instead of converting the bridge already built by others into a bridge made of reinforced concrete. (2003)

生物学的研究领域很广，有很多新的东西。在这种情况下，如何找到以前人们从未曾想到的新东西来？实际上，生物学也有困难的一面。单纯演绎的手法是行不通的。也就是说，不能

说如此这般那般地做了，就会有新的发现。因此， **不管是多么小的事物，只要是别的研究者还没做的研究，都可以去做。这一点很重要。**（2000 年）

There are many new things in biology. In this case, how to find new things that people have never thought of before? In fact, biology has its difficult side. Simple deduction is not feasible. In other words, we can't predict that if we do it like this, we will have new discoveries. Therefore, no matter how small a thing is, if it is not done by other researchers, it can be a subject for research. This is very important. (2000)

03. 论文很重要，成果出来应尽快发表

3 **The paper is very important and the results should be published as soon as possible**

怎样才能写出好的论文，这是一个非常重要的问题

…..

How to write a good paper is a very important problem. …

在学术的世界里，论文非常重要。只靠口头发表，是不能获得国际的认定。也就是说，仅凭口头发表是不能拥有某个科学发现的优先权，人们不会因此认定这项工作是由你最早做出来的。所以， **研究成果一定要写成论文发表。**因为是某月某日的投稿，相差一两天就判定某人具有优先权，我个人认为这是很无聊的。 **但学术有竞争，取得的研究成果，应该尽快写成论文，应该尽快投稿。**

In the academic world, papers are very important. It is impossible to obtain international recognition only by oral presentation. Oral presentation alone does not have the priority of a scientific discovery, and people will not assume that the work was done first by you. Therefore, research results must be written into papers and published. I think it make no sense to judge someone's priority by submission one or two days early. However, there are academic competitions. The research results obtained should be written as soon as possible and submitted as soon as possible.

…

08. 关于抉择：选择“自己想做的事、擅长做的事”

8 About choice: choose "what you want to do, what you are good at"

我做了这么多年研究，一直都觉得做研究很快乐。我从没有感到痛苦而想要放弃研究的时候。不过，不管是谁，或多或少都曾考虑过自己生命的意义。有的人想赚钱，也有的人想把一生倾注在自己喜欢的事情上。人各有所好。因此，并不是所有的学生都成为研究者就是一件好事。我倒是希望大家不要做出错误的选择。也就是说， **作为一名研究者，你要坚持下去的话，必须有“我很想弄明白……”这样强烈的原动力。**

I have been doing research for so many years, and I always feel happy doing research. I never felt the pain and want giving up research. Some people want to make money, and some people want

to devote their lives to what they like. However, no matter who have considered the meaning of their own life, every man has his own taste. Therefore, it is a good thing that not all students become researchers. I hope you don't make the wrong choice. As a researcher, if you want to persist, you must have "I want to understand..." , such a strong driving force.

如果你觉得“我不知道自己要做什么”的话，那你要一直坚持下去做研究就会比较困难。 **如果只是把研究当做谋生的职业而不能从中找到乐趣，我认为，世界上还有很多比起做研究更为舒适的工作。你不妨考虑其他职业。**
If you think "I don't know what I'm going to do", it will be more difficult for you to keep on doing research. If you just take research as a career to make a living and can't find pleasure in it, I think there are many more comfortable jobs in the world than doing research. You might as well consider other careers.

在孩提时代，每个人都拥有着许多种可能性。你既可能成为职业棒球选手，也可能成为歌剧演唱家。但是，到了二十岁左右，我们就知道了自己的局限，就会因种种原因而将各种可能性自行消去，同时也会追问自己到底想做什么。无论是谁，都要过这一关。此时，你只能严肃认真地对各种可能性加以权衡， **选择“自己想做的事、擅长做的事”。**（2019 年）
As a child, everyone has many possibilities. You can be either a professional baseball player or an opera singer. However, when we are about 20 years old, we will know our limitations, eliminate all kinds of possibilities by ourselves for various reasons, and at the same time, we will ask ourselves what we really want to do. No matter who you are, you must go through it. At this point, you can only seriously weigh the various possibilities and choose "what you want to do, what you are good at". (2019)

09. 关于日本：缺乏健全的评价体系

9 In Japan: lack of sound evaluation system

…
但是，非常遗憾的是，日本仍有不足之处。那就是， **日本还缺乏健全的评价体系。当然，这不只是日本的问题，全世界都存在着同样的问题。**特别是日本，可能是因为没有批判性评价的习惯，目前日本的评价体系并不高明。仅仅因为某人受到了一些人的好评或者他的论文出现在某些有名的刊物上，许多人就认为这个人做了一流的工作；相反地，也有些人因为某人的研究领域和自己的很接近，就用异常严格的标准来评价，在鸡蛋里挑骨头。建设性的，但同时又是批判性的评价体系在日本还没有稳稳地扎下根来。（2000 年）
However, it is a great pity that Japan still has problems. That is, Japan still lacks a sound evaluation system. Of course, it's not just Japan's problems, it's the same problems all over the world. In Japan it may be because there is no habit of critical evaluation, and the current evaluation system in Japan is not very good. Just because someone has been praised by some people or his papers appear in some famous journals, many people think that this person has done first-class work; on the contrary, some people use extremely strict standards to evaluate and pick the bone from the eggs because someone's research field is very close to their own. You cannot expect experts from the old theory in a field could be self-denied regarding your new theory. A constructive but critical evaluation system has not yet taken root in Japan. (2000)
…

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(95% 的科研论文是垃圾)

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Stigler's Law: Why nothing in science is ever named after its actual discoverer
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On teaching mathematics
The Arnold Principle. If a notion bears a personal name, then this name is not the name of the discoverer.