

Text extracts of selection of the comments posted online to the Chronicle of Higher Education article titled ‘*We Must Stop the Avalanche of Low-Quality Research*’

2. We need more publications, not fewer - but more robust, instant, open, social mechanisms for sorting.
4. Some of the most important research published in science, when first published, was either seen as useless or plain wrong.
8. VERY disagree:
9. Growth in every sector is considered desirable by economic thinking, which dominates research as much as anywhere else.
12. any research that is not fraudulent is not useless. Just publish *everything* and work on better ways to connect the dots and to search (google?) for relevant research. The good stuff will rise to the top by itself.
14. The fact is that experiments are being performed, using enormous resources - the least one could expect is for the results to be published, so that at least somebody, sometime can make sense of it all. In fact, even negative results should be published - they would help others to avoid the same mistakes in the future.
16. Frequently, it takes many, many years for the great works to be appreciated; you won't always know the good from the bad for many, many years
17. The article states: "In the many discussions of the value of research, however, one rarely hears any mention of how much publication of the results is best." This makes no sense. I'm not even sure it is a sentence.
19. This is one of the most ignorant articles I have read in the Chronicle in years.
21. The practical problem would be sorting the useless research from the useful. Who will be the judge? We must acknowledge that findings that some might find "useless" today could in fact be found to be useful in the future.
23. So we have an english professor and a professor of management as co-authors on an article that bashes science publishing, as well as no indication that the remaining authors who appear to actually do science have the faintest idea what they're talking about
24. impact factors and citation rates are meaningless statistics -- they reflect something real about journals and articles. But they're not the only way to judge the quality of research, and in some cases they're very poor ways. To restructure academic publishing around them, as the authors here suggest, would be a very serious mistake.
25. The authors of this article begin with the assumption that there are two types of publications (and, by extension, research programs): "useful" (i.e. cited within 5 years post-publication), and "not useful/useless" (not cited within 5 years). This assumption is based entirely on a "utilitarian model" of thinking (i.e. the publication must be useful to others within a relatively brief (5 years) period of time to be of value).....the authors' arguments are effective as long as they are limited to applied research fields such as biotechnology, medical science, industry, etc. The paradigm fails entirely, however, when applied to academics whose research interests are driven entirely by a pioneering sense of curiosity and a

desire to go off the popular beat and path and venture into completely unknown territory simply because it is interesting and adds new knowledge to the scientific pantheon.

27.: "The main cause: the growth in the number of researchers." After reading the article this cause was never truly addressed and I didn't see it mentioned much in the comments. Is the conclusion that academia should be limiting the number of researchers or lessening the amount of research per researcher. Not a particularly good recommendation either way. Who decides what research (discussed in much detail here) and who gets to do the research (not really discussed). We should be happy that so many new researchers are in society to help us better understand and evaluate research. Doing your own research and publishing it, is something that is valuable not only from an outcome perspective, but also from a process perspective. Seeing others research (poor and good) helps us all learn. I would be very hesitant to limit research due to problems of homogeneity that can be caused by the 'gatekeepers', both in terms of research and in terms of researchers. Let diversity and chaos reign. Evolutionarily speaking, it will make us all stronger in the end.

28. You are assuming that, if something is not cited, then it is not useful. Verify that claim. The potential usefulness of the findings is the key, and you have no way of assessing that. We face an information overload and have faced it for decades. Cutting off research funding does not solve the problem of how to deal with the vast quantity of information. You have offered no constructive suggestions for doing so, and how could you?

29. I don't publish anything because I want it to be read. I don't care about readers. I publish for my own jollies. The journal system should re-evaluate its formal outlook and dispense with the fiction of readership and concede and celebrate and streamline production to reflect merely the formal vetting of academic papers.

30. Trying to stop bad research from happening by restricting its access to traditional formal peer-reviewed journals seems like an inefficient way to go about it, to say the least, given the wide variety of options available through the Internet to go around such restrictions.

32. "at what point does output overwhelm quality control?" Many of us do not believe that we are even close to that point. Some of us have learned how to be quite efficient in identifying these things. With the explosion in research, there has also been an explosion in the technology to sort for the research we are interested in. We do not live in a unidimensional world of growth. "When does the volume of publication hinder cream from rising to the top?" We are not close to that volume. The hindering of the cream does not and will not occur. There are many ways to find this cream. Also, there are also many more seeds where fruit will bear. Maybe part of the issue here is that the basic premise is something we do not agree with. We don't believe there is a problem.

36. Why not take the more positive approach and say: "I can't possibly read all that's out there; is there a way that we can organize this information?". Yes, it is hard. But we must do it, sooner or later.

37. A crucial issue that has not been addressed in the article is the immense growth of higher education institutions around the world, especially in emerging economies but also in the developed world, in the last 2-3 decades.

38. And in the meanwhile, all research in sciences is vitally important? All of it, every last bit of it.

39. I believe that much of the research that is generated does supply new knowledge. Even rehashing old knowledge is valuable to reinforce the old knowledge. I do not think that new knowledge should only be high impact, short term knowledge. I do not think we have a knowledge overload.

47. But the practicalities of career requirements dictate publishing in a mix of journals. Be fruitful and productive.

49. The sheer volume of remarks on this matter proves the article's main point - academics like to hear themselves speak.

50. No. It is, "Academics like to see their words in print".

51. Most research out there could be viewed as irrelevant to a particular individual or group...but what is one man's irrelevant article might be the article another person needs! We need more voices and venues not less -- perhaps we also need better mechanisms for searching, collating, and evaluating all the information out there, but we don't need less opportunity for people to share their research and curiosity-satisfying explorations!

54. While no one would support taking up valuable journal space with poor science, the premise of this article is questionable. The authors site a reduction of cited articles over two decades from 45% to 41%, hardly alarming in light of the rapid expansion of scientific journals. This is not strong evidence of an upswing in poor quality research. Seems to me that the cup remains about half full. Let the reader decide what is valuable and what is not.

55. There is no way to assess the long-term impact or latent benefits of a scientific paper at the point of publication, and the current funding and evaluation systems force practitioners to keep an open mind regarding the impact of scientific research.

56. if as others have stated, once-ridiculed research later turns out to be useful, then it would seem dangerous to cut off publication for research that might, at the moment, seem ridiculous.

58. I strongly suggest the "Sort Through the Junk" approach: Publish anything that is not fraudulent (and have gatekeepers to check on that aspect alone). Data, after all, speaks for itself (it may take a while, but it does). Instead of implementing policies that artificially restrict research, fund research that addresses the "sorting question" (how to find data, ideas, hypotheses that are relevant for one's research).

60. The Chronicle will certainly win an award this year in the Comic Category for this piece of ----.

63. Progress is bound to be incremental - until (maybe) a BIG IDEA comes along. But since research doesn't happen in a vacuum, it is hard to predict which ones of the little "insignificant" steps along the way helped out...

64. Blind reliance on a single number is the best way to go bankrupt (see crisis, financial, involvement of Wall Street Quants in). Reliance on the number of published articles is foolish, but so is relying on "impact factor" or its gussied-up relative, the H-index.

65. There are no methods of today which will stop us from "killing" research as useless that the future will find to be worth reanimating as the core of some new area of study. The only solution is to increase publishing, to insure a record of such work is created and maintained. In short, broad inclusivity is more important than making it easy for Yale prof X to find just the right article and making sure it is short

enough for them to easily get the info that they want. Modern search engines have made research through article databases easier than ever, yet now we have calls for restricting the amount of those articles? Why?

66. Counting publications is sadly a sign of administrator bean counting, and leads to lowest publishable unit, etc.

68. Economic factors will force a change. Most of this publication is supported by institutional funds (library subscriptions/purchases, page charges/open access fees, etc.). That money is drying up. Like stock market and real estate bubbles, the publishing bubble can and has gone on far longer than any rational person would have predicted. But the reckoning will come eventually.

70. Of all the arguments here, perhaps open access has the most merit. Let's submit everything to Google. They will figure out how to organize and sort it.

73. I don't think there's any stopping avalanche, natural or otherwise, and I wouldn't suggest trying. There's no doubt that there's one hell of a lot of published research, most of it less than essential to civilization, but who's to say what should or shouldn't be published? But you have to be willing to shovel dirt if you want to find gold. I think it's a good thing that belling this cat is impossible, because I really don't think it should be done at all. What authority would have the right to decide what could and could not . . . you get the idea.

74. Science is the most solid, because it's easier to demonstrate that something is new and correct. It's the rest that can easily devolve into a slippery slope of insipid drivel, especially when some correlation between two parameters is found, which is often simply stating the obvious, and then all kinds of speculation on causes ensues with no substantive supporting data when there are dozens of possible relevant parameters. A lot of this makes the press, and those are the relatively good studies -- I shudder to think of the iceberg of junk beneath!

78. I think the topic of this article is important, but the authors have written it in such a way that it is of no value.

79. the fact is knowledge expands exponentially and so does the population of students and experts needed to discover, learn and teach that knowledge. At the very least, what is proposed in the article can be considered an elitist approach by the 'old guard'. Higher education and research, much like the rest of the world, has marched on. Let's concentrate on what our world needs today and into the future and how to best communicate answers to these needs.

81. I don't believe that there is a paucity of problems in the world for researchers to tackle, so I disagree that the volume of publications is a problem. If quality rises through a reformed publication process, the volume level would be a welcome problem we could live with (and benefit from).

83. It's becoming unmanageable precisely because obsolete hierarchical review structures can't cope anymore. I can suggest some light reading, for example, "The Starfish and The Spider" (<http://www.scribd.com/doc/10521739/The-Starfish-and-the-Spider>) or "Here Comes Everybody" (<http://www.shirky.com/herecomeseverybody/>). Alternatively, as a case study, I suggest perusing the history of Wikipedia (from 20 articles the first year, when they used the traditional review methods, and on): http://en.wikipedia.org/wiki/History_of_Wikipedia

85. I quickly googled the publication lists of all the authors of this essay. All of them have more than 500 'research/publications' citations granted some of it is repetition of the same material. How many of their

publications are high quality, high impact, used less capacity of resources etc., etc. I think they should practice what they preach. Quality/junk is in the eyes of the beholder.

88. The current situation is an avalanche, or a flood, but I think that's better than a desert. Consider the situation of biological sciences in the USSR when Lysenko was in control of what could and could not be done, just to take an extreme example.

91. Are the authors assuming that the other 55% are junk? Why? As others have pointed out, important findings can go unrecognized for ages. A senior colleague of mine takes great pleasure in digging up obscure papers that presaged (and refuted) the fashionable new theory of the moment. Why is this bad?

97. The difficulty is figuring out what to do about it without infringing on free publication. In consideration of all the perils that societies face, the publication overload may not be such a big deal.

98. This article is the usual narcissistic arm-waving. If these guys have such quality, how come no-one ever heard of them?

103. Science is a construct of many small parts - no knowledge derived from properly constructed research is "useless".

104. I have read a lot of garbage, so tend to agree that there is a problem. Whether it's growing or shrinking, I have no idea, and I am not enthralled by the proposed solutions. One thing that has always bothered me is that citations all get the same credit - some are made in passing and some are the foundation on which a paper rests. If authors could choose a few "foundational citations" I think it would be easier to identify quality, and more importantly to me, easier to track the key work in an area of research using ISI.

107. I am often for outsiders taking a critical look at something that experts might have long since adopted to and even stopped noticing (or evolved with the changes and never paid critical attention to them). But the above article only shows that the lack of knowledge does not necessarily guarantee success. So in sciences we have too many papers of questionable quality. Does this fact hinder scientific progress? The answer is simple "no": 1) There has never been a good work overlooked because of the flood of papers, rather than because of the dominating school of thought, existing priorities, or simply fashion, i.e. "valid" scientific trends. 2) There has never been an instance where a large number of second-rate papers formed a dominating school of thought or defined scientific priorities (quite the contrary, "me too" scientists always follow trends). The problem of too many papers does not exist now thanks to efficient search engines.

112. Internet technology offers a solution to the problems:

[http://dss.secureid.org/stories/storyReader\\$14](http://dss.secureid.org/stories/storyReader$14)

[http://dss.secureid.org/stories/storyReader\\$19](http://dss.secureid.org/stories/storyReader$19)

113. Over time communities got larger and larger. First, it is true that the size of the community relative to the feasible size of personal networks has become too great in nearly every field for the second to serve a regulatory role over the first. The second factor is that political and administrative decision-makers -- They want quantitative metrics for ranking scholars and departments, and publication data provide this alternative. The deeper question is whether any quantitative algorithm can successfully substitute for the sort of comprehensive evaluation that personal networks used to perform.

114. you're missing a HUGE piece of the picture. I understand your conservatism but the definition of insanity is to try the same thing over and over again when history has shown just how well it works in terms of the SUSTAINABLE health of society. I always ask conservatives "What is it about the past you really want to conserve?" I don't think our past worked very well. I hope the future will work better.

115. Unfortunately you repeat a falsehood that has become folk wisdom, the notion that the vast majority of published papers go completely uncited. The original article in Science that made this claim, to which I believe you refer, included in the "denominator" literally everything: book reviews, obituaries, errata, letters to the editor, editorials, and other marginalia. So, no surprise that the picture looks so bleak. An ISI representative tried to clear up this error, but apparently to no avail. See David Pendlebury, "Science, Citation, and Funding," in Science 251: 1410-1411.

116. I find it extremely troubling that an English professor is the lead author on this.

119. As a co-author of the "avalanche" paper, I'm gratified by the responses, some of which have been quite constructive. Perhaps you will be interested to know that we have a much longer and better documented paper on the same subject appearing in the fall issue of ACADEMIC QUESTIONS.

122. I'm not the "lead author" the piece. The names are in alphabetical order.

125. Move beyond the elitism of academic publication and provide platforms for anyone to publish anything e.g. academic blogging. I believe that over time, better quality work will "rise" and poor quality work will be fade off the radar.

126. Let there be more and more until no one has time to read everything. That way, we'll be forced to find papers through other selection criteria, which is what's happening now, anyways.

127. A Very Communist Idea!!! Only communist were saying that, too much knowledge and intellectuals is a bad idea!

128. Our work does not get cited as much simply because there are several orders of magnitude less people working in the field, and you cannot in any sane frame of mind claim that work is now unworthy of publication. Hype does not suddenly make the research higher quality. In fact, more often than not, hype contributes to errors getting overlooked even more than usual, and enables crappier-than-usual work to get through because it sounds cool. Note also that it may well be that older (and by now more obscure/less cited) research is higher quality than modern stuff, as there was more time and money (purchasing power, that is), and less pressure to churn out crap at the rate of X papers a year. Also, people back then actually had the time, desire and capability to understand the equipment they used, unlike now where nearly everything is done through one Expensive Shiny Black Box or another. The older research is still very useful, and is not IN ANY WAY a hindrance to modern research. In fact, a great hindrance to modern research is the failure to read the less-cited works. I think an important step would be to teach basic lit research skills and hang up the following wonderful quote in every research lab out there: "Two months in the lab can save two hours in the library." - source unknown

131. Cole, J. R., & Cole, S. (1972, October 27). The Ortega hyposthesis. Science 178, 368-375. The authors argue, based on citation analysis of physics articles, "that only a few scientists contribute to scientific progress" (p. 368) and ask "whether it is possible that the number of scientists could be reduced without affecting the rate of advance" (p. 372).

132. With regard to the question of the cream rising to the top, I don't really understand the problem with sifting through the information 'avalanche'. In the space of about 20 minutes I can find a lot/most/all of the top cited articles for any keyword I choose. Another 25 minutes reading the abstracts. Perhaps 2-3 hours reading through and annotating the articles I actually need to read and archiving them digitally in a relevant folder. Where, therefore, is the avalanche problem? Perhaps the problem is not the volume of information but the poor secondary research skills of researchers to find what they need.

133. Journals can grade their articles - I've seen this in a handful of journals where they have papers that are earmarked as excellent, like front page headlines, followed by regular articles, discussion articles and forums pieces. By differentiating the excellent from the pedestrian based upon editorial review, this helps the cream rise to the top

135. But science does not create reality. It describes reality. An uncited physics paper that described a route to a destructive weapon would be of far more value to life on earth than a many cited geology paper that led to the current oil spill. We have lost sight of what science does for us in the race to patent, save lives and create technology --> science creates a description of nature, nothing more. It allows us to see ourselves and our place in nature. The more science, the better, and even if it goes uncited, it will not go unread.

136. Impact factors, as Thompson ISI warns, are for evaluating journals, not authors. It's a poor proxy for the quality of an individual article. Nature and science articles are often close to worthless. They sometimes act almost as press releases, with the meat of the research forced into some other journal. Yes, some of them are highly cited because they announce breakthroughs, but that doesn't mean they really communicate those breakthroughs - that's left for the follow-up article.

137. The print legacy bundle that is a journal is the main problem. Libraries ought to be able to buy only the articles that their patrons actually want to read. Then there would be an economic cost to the publisher for including a poor quality paper in their journal.

139. There is more at work here than the sliver of skin (philosophy) you have lifted (apologies to Shakespeare).

140. no one can determine what is important, and what isn't. Of course there's junk out there, in every field, driven by publish or perish mentality. BUT only time, and MORE research, will separate the wheat from the chaff. (If you think you or anyone else can, hubris is rearing its ugly head). I also agree that most Deans and administrators count but don't read - and are a huge source of the problem. I agree with publishing a lot of it on the web, and letting it be sorted out by time.

142. I never had to read past the title and the author affiliations. The authors are not scientists, or they would know that scientific discoveries are like orgasms: there are no bad ones. Incompetent drivel of people who try to talk about something they don't understand.

144. This failure to identify "importance" doesn't mean peer review isn't working. Peer review main purposes are to evaluate whether researchers claims are reasonable given the data and to evaluate whether researchers have gathered all the data they should before making such claims. Judging importance is low on the list of things peer review is meant to do.

147. When people publish "inconsequential" research it may spare others the trouble of going down blind alleys and dead ends. So "inconsequential" research may serve the greater good, even if inconsequential

papers are not cited. I think that the quality issue sorts itself out over time. Thus, it seems to me that the authors are making much ado about a non-issue.

148. We must discourage the use of the JIF for assessing quality of papers, and most certainly discourage the use of JIF for measuring the quality of science and scientists.

150. it is hard to argue with human nature: journal articles show the sweat. The present system may be the best of which we are capable.

160. Many are familiar with Dunbar's number - the optimal number, proposed by Robin Dunbar, of people with whom any given person can maintain a stable social relationship is around 150. Indeed, recent research in human evolution seems to show that there is something like an optimal size for a human community and that once we expand vastly beyond that, all kinds of inefficiencies spring up. And large, inefficient systems are certainly more vulnerable to collapse. So when the authors say the status quo is unsustainable, I am inclined to believe them. This of course has all sorts of unsavory political implications (Whose voices will be heard? I hope mine!), but I think it's an important reality that cannot be ignored. In any case, whether there comes to be a consensus on this issue or not, people will find ways to deal with the overabundance of information, some of it of higher quality or utility than other parts. I will be watching to see how the process unfolds.

163. The authors' argument is incoherent. On the one hand, they argue that "anything more than a few years old is obsolete. Older literature isn't properly appreciated, or is needlessly rehashed in a newer, publishable version." On the other hand, they promote a science-inspired system of pure quantification of citations, premised on the assumption that the academic community's judgment is authoritative.

164. Rewarding citations and impact factors will be fiercely resisted by those who have done well under the present counting regime.

165. A PIONEERING PIECE OF DISTILLED STUPIDITY. None of the authors are scientists---that is APPARENT

167. This is called reductionism. The irony of this articles' title and this response is like bringing chloroform to a yoga sitin.

168. The system much change. A better (cheaper and more accessible) method must be established. What about an iTunes-like system for .pdf copies at a reasonable price (99 cents sounds good) for access.

170. First, we need more publishing, not less. Second, in the era of the internet there is no excuse for not being fully transparent. All of the data and the methodology should be accessible to anyone who wishes to look at it. Third, the AGW debate has shown how the peer review process is broken. Having small cliques control most of the publishing in a particular area is not beneficial and can easily lead to abuse. Better to have a smaller role for reviewers and allow full transparency and feedback to dominate the publishing process.

172. Balderdash, I say! I've made my career on "useless" science. You can't have everyone discovering things of massive import, or the pressure to fabricate would go through the roof and the whole enterprise would come to a grinding halt. You need to have us serfs out there rooting through the withered tubers in the fields surrounding the castles of the nobles (Nobels?). Who knows, one of us might find the buried treasure or the mold that turns out to be penicillin and saves a million lives. Science is a CHEAP enterprise compared to, say, the military or petrochemical exploration. It creates jobs (even publishing

jobs!) and gives us something to be proud of. Are "science blogs" the answer? Hardly! You see, the point isn't just to force people to fill the library shelves and online databases with minutiae that no one cares about, but to subject the work of scientists to PEER REVIEW. The system has problems to be sure, but too many scientific publications ain't one.

173. I am on the side of being a consumer, not a producer, of research information. From the consumer's perspective, I find that I generally get the best results from the Web of Science (Thomson Reuters), then Scopus, then Inspec, then Google Scholar. Yes, in this specific order. My opinion based upon my own needs as a consumer of research papers. However, for the money, Google Scholar is tough to beat: Free isn't a bad thing. And Inspec Direct is RELATIVELY cheap for individuals this year: Unlimited access to the entire database, unlimited SDIs. GREAT deal for USD 320.

175. Nowadays there are a lot of free search engines available but still research should not be taken too shallow.

176. A commentary on the Impact Factor can be found the Nobelist, R. Ernst, The Follies of Citation Indices and Academic Ranking Lists. A Brief Commentary to 'Bibliometrics as Weapons of Mass Citation' in *Chimia*, 2010, 64, 90, doi:10.2533/chimia.2010.90

177. Quality of a paper is hard (may be impossible) to assess objectively - it as be a subjective matter.