Journal of Ethics and Emerging Technologies

IEET

Article

Publish or Perish in the Era of ChatGPT

C. D. Brewer [1*]

[1] Central Connecticut State University; cbrewer@ccsu.edu
* Correspondence: cbrewer@ccsu.edu

Abstract: I argue that rather than being viewed as the main goal of academic creativity, publications should be viewed as simply another source of academic creativity and treated like conference presentations, teaching, and, most importantly, paper reviewing. We need to stop incentivizing the production of numerous publications. The number of published articles is increasing at an alarming rate, and so are retractions. Changing the system is especially important now that LLMs such as ChatGPT have entered academic publishing. LLMs exacerbate the practical problems of our current publication model and raise important ethical concerns. By examining the epistemic goals of academic publishing and the reward structure tied to publishing, I argue that providing new pathways to promotion and tenure can mitigate some of the problems with our current model.

Keywords: Tenure, Publishing, Large Language Models, AI, Academic Honesty

1. Introduction

In this paper, I argue that given the current state of academic publishing, we need to make some fundamental changes in how publishing is viewed and rewarded by tenure and promotion committees. We need to stop incentivizing the production of numerous publications and start incentivizing other paths to promotion and tenure. Incentivizing numerous publications leads to far more drawbacks than benefits. We may need to start these changes at institutions with a heavier focus on teaching, but if we could begin to change some of the structural and institutional mechanisms, perhaps more research-oriented schools could follow suit. While I understand the irony of publishing a paper about decreasing publishing, I want to be clear: the goal is not to artificially limit the number of publications that are produced; it is first to disincentivize publishing as the main form of promotion and tenure because this gives rise to what I will call "unnecessary" publications. Second, it is to try to realign the desire to publish with the desire to advance one's field. While some of my claims are not new (others have highlighted the pitfalls of the publish-or-perish system), I show that changing the system is especially important now that Large Language Models (LLMs) like ChatGPT have entered academic publishing.

I begin by highlighting some of the issues that accompany the publish or perish mentality in both the sciences and the humanities. While I highlight many challenges arising in the different sciences, I focus mainly on philosophy when I consider the humanities. This is not because I think philosophy is a special case in the humanities; it is rather that I have been tracking many of these issues, and I have faced some of them myself. However, I believe very similar issues occur in every academic field, and every

Citation: Brewer, C. D. 2026. Publish or Perish in the Era of ChatGPT. *Journal of Ethics and Emerging Technologies* 36: 1. https://doi.org/10.55613/jeet.v36i1.189

Received: 02/14/2026
Accepted: 16/02/2026
Published: 17/02/2026

Publisher's Note: IEET stays neutral with regard to jurisdictional claims in published maps and institutional affiliations.

field needs to move away from heavily incentivizing publications. I focus on the sciences in general (rather than on a specific discipline) because numerous articles address issues across the sciences. For instance, one can find research on the number of scientific papers published and the impact of many of them. Furthermore, while there are dangers of misinformation being published in any field, its dissemination across multiple sciences may be particularly problematic, especially in fields that affect human and/or environmental health.

In what follows, I begin by highlighting some of the problems with the current system. In section three, I examine and challenge the "purist view" of publishing, which is perhaps the strongest epistemic argument supporting the current publication model. In section four, I highlight ethical worries arising from the introduction of LLMs into the current publication model, and in section five, I offer possible alternatives to using publishing as the main pathway to tenure and promotion.

2. The Practical Problems with Publishing: ChatGPT, Academic Fraud, and Paper Bloat

"Certainly, here is a possible introduction for your topic: Lithium-metal batteries are promising candidates for high-energy-density rechargeable batteries due to their low electrode potentials and high theoretical capacities" (Zhang et al., 2024). Thus begins the now retracted paper, "The three-dimensional porous mesh structure of Cu-based metal-organic-framework-aramid cellulose separator enhances the electrochemical performance of lithium metal anode batteries." According to the Editors-in-Chief of *Surfaces and Interfaces*, the article was retracted because an "investigation by the journal confirmed substantial duplication of text and image data" between it and another article published by the same authors "in close succession." As one might expect from the opening statement, the Editors-in-Chief also highlight that "the authors appear to have used a Generative AI source in the writing process of the paper without disclosure, which is a breach of journal policy" (Zhang et al., 2024).

We have empirical evidence that authors are using ChatGPT and other LLMs without attribution, and some of these articles are being accepted through the peer-review process. The fact that these articles can pass through a review process highlights the risk of our current publication model. *Retraction Watch* is tracking articles that appear to use LLMs without attribution. There are currently ninety-two papers on the list ("Papers and Peer Reviews," n.d.). Ninety-two papers are merely a drop in the bucket; a recent study found that around 2.82 million *scientific* papers were published in 2022 alone (Hanson et al., 2024). While it may be worrisome that any paper generated by artificial intelligence made it through a supposed peer review process, one might expect the number to be far higher than ninety-two. While the number may be higher (as AI-generated papers have likely evaded detection), I agree that LLMs are not the primary issue; the real danger lies in the 2.82 million scientific papers published in a single year. The problem with LLMs like ChatGPT is that they will likely increase that bloated number exponentially. They also create new ethical worries concerning justice or fairness (we will consider this is greater

detail in section four). The increase in paper submissions will make fraud harder to detect, and it will make it harder to find good papers due to the volume of papers one will need to sort through. Technological advances in AI should worry us because the number of published papers was increasing rapidly before advanced LLMs were available to the public, and these LLMs will make fabricated papers more sophisticated and easier to produce.

If we do not take serious steps to reform academic publishing, the problems associated with our current model will only worsen. As the authors of "The Strain on Scientific Publishing" indicate, "The last few years have seen an exponential growth in the number of peer-reviewed journal articles, which has not been matched by the training of new researchers who can vet those articles. Editors are reporting difficulties in recruiting qualified peer reviewers, and scientists are overwhelmed by the volume of new articles being published" (Hanson et al., 2024, p. 823). The dramatic increase in publications *might* not be worrying if quality assurance were possible. But that does not appear to be the case.

As the number of published papers has increased, so has the number of retractions. According to *Retraction Watch*, in 2002, journals retracted 119 scientific papers. In 2022, there were more than 4,600 retractions (Oransky, 2022). In 2023, more than 10,000 research papers were retracted. Of the retracted papers in 2023, 8000+ were from journals owned by Hindawi, a subsidiary of Wiley (Van Noorden, 2023). While 2023 may be a statistical outlier, we must still grapple with the fact that retractions "are rising at a rate that outstrips the growth of scientific papers" (Van Noorden, 2023, p. 480). A recent study focusing on the publication of biomedical research papers in Europe found that Retraction rates increased from 10.7 per 100,000 in 2000 to 44.8 per 100,000 publications in 2020. The authors found that research misconduct accounted for most retractions (66.8%) (Freijedo-Farinas et al., 2024). These numbers are concerning in themselves, but they should worry us even more because, with the advancement of LLMs, such misconduct is likely to become more common and harder to detect.

The increase in retractions may also be due to the rise of "deception sleuths," like Guillaume Cabanac and Cyril Labbé, who have made a career out of exposing fabricated papers. *Nature* named Cabanac one of the top ten people who helped shape science in 2021 for his work exposing fake papers (Kwon, 2021). Cabanac, Labbé, and Alexander Magazinov developed the Problematic Paper Screener (PPS), publicly available software that scans scientific literature weekly, looking for "tortured phrases" (O'Grady 2024). Torture phrases are "unexpected weird phrases in lieu of established ones, such as 'counterfeit consciousness' instead of 'artificial intelligence'" (Cabanac et al., 2021, p.1). These occur when probabilistic text generators are used to create scientific papers based on existing papers. The text generators change the wording of the original paper to avoid plagiarism detection. They often simply replace words with synonyms, which gives rise to these tortured phrases. Examples of tortured phrases they discovered include "discourse acknowledgment" for "voice recognition" and "counterfeit/human-made consciousness" for "artificial intelligence" (Cabanac et al., 2021, p.3). Using their list of tortured phrases, they investigated papers published by the journal *Microprocessors and Microsystems* from

February 2018 to June 2021. Although they did not systematically screen the papers, they identified approximately 500 questionable articles that had been accepted (Cabanac et al., 2021, p.24). In the same time period, they noted that the journal underwent several changes, including an "abrupt drop of average/median duration of editorial assessment," and an "abrupt surge in the number of articles accepted" (Cabanac et al., 2021, p.24). It is worth noting that Elsevier, the publisher of *Microprocessors & Microsystems*, later issued an expression of concern regarding six special issues that involved as many as 400 publications. In each case, Elsevier claimed, "the integrity and rigor of the peer-review processes were investigated and confirmed to fall beneath the high standards expected by *Microprocessors & Microsystems*" (Marcus, 2021).

While software such as PPS will likely help uncover fabricated papers, as LLMs become more sophisticated, bad actors can train them to avoid tortured phrases. And while the work of scholars like Cabanac, Labbé, and Magazinov, is a step in the right direction, we do not have anywhere near the number of deception sleuths needed. To make matters worse, even when there is a good reason to suspect misconduct, journals may respond slowly, if at all. In a recent article, "Ten Years later: Assessments of the integrity of publications from one research group with multiple retractions," authors Andrew Grey, Alison Avenell, and Mark Bolland document the challenges associated with getting journals to scrutinize published papers. Between March 2013 and February 2020, Grey et al. raised concerns about 300 publications by a research group whose principal investigators, Yoshihiro Sato and Jun Iwamoto, had numerous retractions and were known for serious research misconduct. Sato and Iwamoto are currently listed on *Retraction Watch's* "Retraction Watch Leaderboard," which tracks researchers with the most retractions. Sato is currently third on the list with 124 retractions, while Iwamoto is seventh with 91 ("Retraction Watch Leaderboard," n.d.). Of the 300 papers Grey et al. flagged, eight were retracted before their concerns were raised. By April 2023, a total of 121 papers were retracted, 12 had an expression of concern, and 3 had been corrected. However, the remaining 164 publications did not have "an editorial notice of reassurance that the integrity of the paper was intact" (Grey et al., 2025, p. 492). Even more concerning, they note that for "four of the largest publishers of biomedical research, Elsevier, Springer Nature, Wiley, and Wolters and Klower, 16–40% of publications had not been assessed, 6–8 years after concerns were first raised at one of their journals" (Grey et al., 2025, p.496).

Let me be clear: I am not arguing that journals should immediately issue expressions of concern. We might worry that journals are too quick to comply when possible fraud is reported; knee-jerk reactions could damage the reputations of honest researchers, and there is a possibility that an honest researcher could be targeted for spurious reasons (e.g., jealousy or competition for funding). However, when the research group associated with the publication is known to have engaged in large-scale academic fraud, investigating its other publications should be required, and inaction after 10 years is inexcusable.

The increase in academic papers should not surprise us. Given the way publication works in many fields, there is a clear financial incentive for for-profit publishers to continue to increase acceptance rates. *Retraction Watch* also tracks instances of mass

resignations of journal editors. While the reasons for these mass resignations differ, many cite open access policies and author fees as the reason ("Retraction Watch Mass Resignations List" n.d.). Open-access journals can charge authors thousands of dollars to publish their findings (Dattaro, 2023). In some cases, the resignations occurred because the journals were "'pushing' the editors to move the journal from a hybrid subscription/open-access model to a fully open-access model funded by charging fees to authors" (Kincaid, 2023). Unsurprisingly, when for-profit journals begin charging authors, they also seek more submissions.

A high-profile case in philosophy highlights how for-profit publishing can lead to a decrease in journal quality. In May of 2024, the editors of *Philosophy & Public Affairs*, published by Wiley, resigned *en masse* from the journal. The journal was one of the premier journals in social and political philosophy. In their resignation letter, the editors state that Wiley's move to an open access model that charges authors "reinforced academic inequality" and "incentivized commercial publishers to try to publish as many articles as possible and so to pressure rigorous journals to weaken or abandon their quality controls" (Weinberg, 2024). In 2023, Anna Stilz, the Editor-in-Chief of *Philosophy & Public Affairs*, documented similar problems with Wiley after Robert Goodin, the founding Editor of the *Journal of Political Philosophy,* was removed from his editorial position by Wiley. She wrote:

> Wiley has recently signed a number of major open-access agreements: this means that increasingly, they get their revenue through author fees for each article they publish (often covered now by public grant agencies), rather than library subscriptions. Their current company-wide strategy for maximizing revenue is to force the journals they own to publish as many articles as possible to generate maximum author fees. Where editors refuse to do that, they exert all the pressure they can, up to and including dismissal, as in this case. (Weinberg, 2023)

The role capitalism has played in exacerbating the publication problem cannot be ignored. The academic publishing model now often involves for-profit publishers that seek as many acceptances as possible because each accepted article generates revenue, even if that article does nothing to advance the field. Furthermore, "publishers advertise total articles published as a desirable quality that augments their brand, as bargaining chips to negotiate subscription fees" (Hanson, et al, 2024, p.824). The metric "total articles published" is quite different from "articles that have advanced the field."

A growing number of scholars see the dangers of our current system. Recently, Cambridge University Press "conducted a radical, community-led review of the open research publishing ecosystem" (Publishing Futures, 2025, p. 2). The report titled "Publishing futures: Working together to deliver radical change in academic publishing" involved "extensive evidence gathering from across the academic community – including researchers, librarians, publishers, funders, and publishing partners – through workshops, interviews, and an online survey with over 3000 responses" (Publishing Futures, 2025, p.7). When asked, "To what extent do you agree that the move to more open access journal publishing so far has "[e]ncouraged publishers to prioritise publication volume and profitability over editorial rigour and quality control," fifty-five percent of respondents

either agreed or strongly agreed (Publishing Futures, 2025, p.23). When a publisher's goal is profit, and the profit model is tied to acceptances, quality control becomes an afterthought.

Meanwhile, we have incentivized individuals to publish by linking job security and professional success to publication. While some institutions may reward fewer publications in more prestigious journals, in many cases, quantity takes precedence over quality. This seems to be a common perception among academics. In the Cambridge study, respondents were asked, "To what extent do you agree that these academic reward and recognition systems emphasise quantity over quality in terms of publishing outputs." Sixty-four percent of respondents either agreed or strongly agreed (Publishing Futures, 2025, p.43). The system is designed such that the more publications one has on one's CV, the more successful one is considered. Furthermore, even when a journal prioritizes quality control, there may not be enough qualified individuals to carefully review the submitted papers. While the exponential growth of journal articles, coupled with a decrease in quality control, is the primary issue, it is also clear that LLMs such as ChatGPT will exacerbate this problem. When publishers seek more publications, authors may publish and ask an LLM to generate content; if qualified peer reviewers are unavailable, we set the stage for academic fraud and misinformation to thrive.

Problems in the world of academic publishing are nothing new. Predatory publishers, paper mills, self-plagiarism, and falsified data have long been issues. But the increase in open-access models, coupled with the rise of LLMs, should worry us. The rise of LLMs is significant: they compound each of the previous issues. Predatory publishers can entice researchers to create papers in a matter of hours. Paper mills can generate publications even faster than before, and the grammar and style will often be improved, making it harder to detect fraud. Researchers can put their own work into an LLM and generate content that mimics their style (Mann et al., 2023). Dishonest actors can use LLMs to generate false citations and images. The question is then: what do we do? The genie is out of the bottle. While some researchers will use LLMs in legitimate ways, the unscrupulous (and perhaps even those with scruples, but who worry more about job security than their principles) will use them, not to fix phrases or edit grammar, but to generate content.

While I do not believe a quick fix is possible, I argue that there is something we can do: we need to disincentivize publications. We need to stop rewarding individuals for producing numerous publications because not all publications are equal. Furthermore, I contend that if an author is hyper-prolific, rather than celebrating that fact, we should treat it with greater scrutiny. To make this case, it will be helpful to consider why some believe we should publish and why we should reward publication.

3. The "Purist" View of Publishing

In "What is The Value of Publishing?," Warren Chan summarizes what he calls the "purist" view of publishing and the reward system tied to it. While Chan recognizes the problems

with this idealistic view of publishing, his description seems to accurately convey why we might reward prolific paper publishers. He writes,

> In its purest form, the purpose of publishing original research studies is to disseminate the results of experiments to inform the audience about a new concept or about advances in a technology or scientific field. By contrast, review articles summarize original studies in a particular field and provide direction to researchers by outlining the progression of research in that field. Lastly, perspective articles highlight the reflections and opinions of researchers on original research studies conducted by their peers. It is generally expected that the authors of such publications will have published sufficient original studies to be deemed experts in the field. Based on this "purist" view, the more papers a researcher publishes, the more productive the researcher is deemed to be. As a reward, the researcher is recognized for his or her work, and the volume of his or her publication record is regarded as supportive evidence of the researcher's expertise in the field and as justification in grant applications of his or her ability to solve important scientific problems (2018, p.6345).

Seema Rawat and Sanjay Meena highlight another feature that seems to stem from this purist view: publications also bring attention to a scholar's institution. Institutions with active scholars may, in turn, receive more funding (Rawat and Meena, 2014, p. 87). Thus, we could attempt to justify the current reward structure involving field renown and job security because those who publish advance their fields, demonstrate their expertise, and aid their institutions. Unfortunately, as we will see, the desire for individual and institutional renown (and funding) and even for job security may conflict with the advancement of the field.

Before challenging the "purist view," it is important to note that there is something correct about it: whether we consider the hard sciences, mathematics, or the humanities, theories and methods are developed through articles in the field. We need articles to develop and clarify concepts, theories, and experiments. Citing previous research provides justification for our claims, and while some research may be problematic, fields would advance much more slowly if we had no access to each other's work. While all of this is true, the question at issue is not really whether fields advance from publications; the question is whether fields advance faster from access to *more* publications. This question is much harder to answer in the affirmative.

Perhaps the most common way to show that particular articles advance their field is by tracking how often they get cited. While we will soon consider some of the problems with using this metric, even if we were to think citations indicate field advancement, some papers are never cited. A 1990 article by David Hamilton claimed that statistics "indicate that 55% of the papers published between 1981 and 1985…received no citations at all in the five years after they were published." Hamilton went on to claim that "Even those papers that do get cited aren't cited very often" (Hamilton, 1990, p. 1331). A more recent study argues that the number of uncited papers is far fewer than Hamilton asserted.

Richard Van Noorden argues that new figures, which count both research articles and reviews, show that:

> Of all biomedical-sciences papers published in 2006, just 4% are uncited today; in chemistry, that number is 8% and in physics, it is closer to 11%. (When cases of researchers citing their own papers are removed, these proportions rise — in some disciplines, by half as much again.) In engineering and technology, the uncitedness rate of the 2006 cohort of Web of Science-indexed papers is 24%, much higher than in the natural sciences… For the literature as a whole — 39 million research papers across all disciplines recorded in the Web of Science from 1900 to the end of 2015 — some 21% haven't yet been cited. Unsurprisingly, most of these uncited papers appear in little-known journals; almost all papers in well-known journals do get cited. (Van Noorden, 2017, p. 163)

While 21% remains a very high proportion of papers, it may also be inflated. Van Noorden argues that discovering the true number may be impossible due to "data-entry errors or typos," so determining the actual number of uncited papers is likely out of reach (2017, p. 163). However, even if the number of papers that go uncited is lower than these statistics suggest, the evidence suggests that millions of papers go uncited.

The bigger issue, of course, is that having citations does not mean an article advances the field. Ludo Waltman and Dahlia Remler both highlight that citations alone mean very little. Waltman notes that "many citations are quite superficial or perfunctory" and claims that he "would not tend to interpret these figures as reassurance that more of our scientific work is providing a useful purpose" (Van Noorden, 2017, p. 163). Remler argues that "Even highly cited research could be a game that academics play together that serves no one's interest" because they could simply be citing each other's work to artificially inflate citations (Van Noorden, 2017, p. 164). Furthermore, papers could be cited because the content of the paper is misleading or incorrect.

There are also journal conventions that force citations of articles. For example, consider the *American Journal of Bioethics*. The journal's structure means that "target articles" are released to scholars before publication. Scholars then provide several critical commentaries on the target articles, which are published in the same volume as the target article. Since these commentaries discuss the target articles, the target articles are cited by every commentary. Thus, even if no one else reads the article, it is automatically cited by each commentary. Cases like this, involving self-citation, can clearly inflate citation counts for articles and the journals that contain them. While we may be able to police self-citations, we know that citations can be artificially inflated in other ways. "Citation farms" exist, in which "clusters of scientists massively cite each other" (Van Noorden & Chawla, 2019, p.578). In 2013, Thomas Rueters, the firm that calculates impact factors, suspended 14 journals because they were engaging in "citation stacking." The journals effectively traded citations with other journals to avoid excessive self-citation (Van Noorden, 2013).

Thus, while some cited papers clearly do advance fields and should be heralded for doing so, there is ample reason to think that many papers do not. Some are detrimental to a field. If an article contains misinformation and is cited by other articles or picked up by

the media, that misinformation is further disseminated. This, of course, can have a dramatic impact in some instances, like the case of Andrew Wakefield's discredited autism study. Other papers appear to have little or no impact on the field; they are never cited, or, even when cited, those citations may be perfunctory.

Even if an article is cited and that citation is not superfluous, we still know that the information cited may be incorrect because the original study is not reproducible. According to a survey in *Nature,* of the 1,576 researchers who took an online questionnaire about reproductivity in research, "70% of researchers have tried and failed to reproduce another scientist's experiments, and more than half have failed to reproduce their own experiments" (Baker, 2016, p.452). When asked about the cause of the reproducibility crises, "More than 60% of respondents said that each of two factors — pressure to publish and selective reporting — always or often contributed" (Baker, 2016, p.454). When a study's findings cannot be reproduced, it is hard to claim that it has advanced the field, even if it is cited by others. It may even hold the field back by making unreliable results publicly available.

This raises another concern about the argument for advancing the field: in many cases, a researcher's intention is simply to be published, not to advance the field. That is, the goal is simply the renown associated with hyper-productivity. While a paper could advance a field even if that were not the intent, fraud and carelessness are far more likely to occur if the goal is publishing as much as possible. When both job security and field renown are tied to publications, there are clearly other incentives to publish than advancing the field. As Benjamin Davies and Giulia Felappi argue, "Pressure to publish may also encourage harmful pragmatic strategizing" (2017, p. 747). They cite a blog post by Michael Huemer on the best strategies for publishing in philosophy. He writes:

> To get published, you...need an idea that the referee thinks is likely correct, yet not obviously correct, interesting, and tied to the literature, yet hasn't been said before. That rules out practically everything. So it's almost impossible to get published. Here is the loophole: you can take a point that is so narrow and so tied to the specific course of the recent literature that it hasn't been made before." (Cited in Davies & Felappi, 2017, p. 747)

I would like to stress two points about Heumer's post. First, Huemer's post, in part, provides advice for young scholars in the field. He informs young scholars about some of the pitfalls of publication and provides strategies to avoid them. His advice may be useful for getting published, but it is not clear that it will advance the field. Second, while Huemer's point here concerns publishing in philosophy, it has broader implications. Each field has its own strategies for publication, for the sake of publication. While some of these strategies may be compatible with seeking to advance the field, others are not.

In the sciences, the practice of "salami slicing publications" or "salami publication" exemplifies one such problematic strategy. Vesna Šupak Smolčić defines salami publications as "a publication of two or more articles derived from a single study," which "report on data collected from a single study split into several segments just large enough to gain reasonable results and conclusions" (2013, p.237). Salami publications are thus

redundant publications. Šupak Smolčić highlights several problems with these publications: they artificially enlarge the number of one's scientific work affecting career advancement and funding; they waste the time of editors, reviewers, and the scientific community; and they may involve copyright violation. But perhaps the most worrying issue is that that, "Taking the same data twice into result calculations can significantly distort the final outcome of meta-analysis or other systematic reviews and therefore have a direct impact on clinical practice" (2013, p.238).

Hendrik P. van Dalen and Kène Henkens argue that salami tactics and academic fraud are a direct result of publish or perish culture, which is based on a problematic reward structure. The reward structure is problematic because universities reward A (publications), while *hoping* for B (groundbreaking publications). They claim, "The danger with badly designed reward systems is that they may backfire…For scientific publications, this may imply that scientists back away from high-risk projects, apply 'salami tactics'…or practice out-right fraud or plagiarism (van Dalen & Henkens 2012, pp.1286-7).

Recent history also highlights that the incentive for many academics is simply to be published. Consider the influx of papers following COVID-19. According to *LitCovid* (a hub for tracking scientific information about the Coronavirus), there are currently 438,444 publications on Covid through November 2024 (LitCovid, n.d.). 125,00 articles were published in the first 10 months of the pandemic, and in that period there was "an unusual number of article retractions and withdrawals, with papers being retracted far earlier than the usual time period, which is typically 2 to 3 years" (Peterson et al. 2022, pp. 127-128). Currently, 456 articles have been retracted, and another 18 have expressions of concern ("Retracted coronavirus (COVID-19) papers," n.d.). In "How Covid-19 bolstered an already perverse publishing system," Jocalyn Clark examines how some researchers exploited journal interest, funding opportunities, faster turnaround times, and lowered standards simply to be published–and did so in an area where they lacked expertise. She quotes Ross Upshur, a pandemic governance expert at the University of Toronto, noting that the opportunism displayed during the pandemic is nothing new: it "reflects an already perverse system of academic reward that has little motive to change: the gold rush to publish was simply an extension of the usual 'publish or perish' culture. It's therefore unsurprising that 'people had to become a covid expert to survive, or at least a self-appointed expert'" (Clark, 2023, p.689). Elizabeth Gadd, a UK scholarly publishing expert, explains how publishing during the pandemic exemplifies that the goals of publication are often not aligned with the goal of advancing one's field. She states, "The pandemic just highlighted the problems of publishing. It's expensive, slow, and reinforces journal articles being the accounting unit of scholarship. It's not about contributing to scholarly conversation. It's scholarly fanfaring of results that doesn't align with the mission of science. If all authors got was feedback from reviewers and readers there would be no race to publish. As it is, the situation is a quest for glory" (Clark, 2023, p.689). While Gadd is correct that it is often a quest for glory, it is also often a quest for job security.

The issues that arose during COVID also point to a larger problem with the publish-or-perish mentality: this culture can affect which areas we choose to study. Davies and

Felappi believe that "pressure to publish encourages the strategy of working on fashionable topics. If a particular issue or area is currently popular, with a great deal of work being written on it, contributions on this area may be more likely to be deemed valuable" (Davies & Felappi, 2017, p. 749). This may lead to problems such as those mentioned above, in which one's expertise is not aligned with the fashionable topic, but it can also arbitrarily limit the areas that scholars investigate. In the worst cases, the two issues would combine: we would narrow the focus of our investigations, and even the unqualified would try to publish. If a journal is seeking articles on a fashionable topic and a scholar needs publications, article quality may be a secondary consideration for all those involved.

What I hope to have shown in this section is that while it is clear that fields do advance through the publication of articles, it is also clear that many articles contribute little or nothing to their field, some of the contributions are actually dangerous, and treating journal publications as the only means of field advancement and tenure and promotion may lead to fewer advances. The purist picture of publishing and its reward structure is implausible given the empirical evidence at our disposal. However, we can also use this purist picture to frame some of the ethical issues that arise when LLMs are introduced into our current publication and reward structure.

4. Some Ethical Issues with LLMs and Publish or Perish

The availability of LLMs has raised a range of ethical concerns. Here, I will highlight two worries: utilitarian worries and violations of justice or fairness. I turn to utilitarian concerns because we have already laid the groundwork for these worries. But the utilitarian concerns I raise focus on the role LLMs play in the current publication and reward structure. The concerns of justice or fairness I wish to highlight have a broader scope: they are closely tied to LLMs, such that the worries will remain even if we are able to reduce the pressure to publish.[1]

The utilitarian concerns I have in mind are tied to the empirical evidence currently available. The many drawbacks of LLMs are clear: the number of publications sent to journals will increase, putting more burden on an already strained process; fraudulent studies are easier to produce; and misinformation is more easily disseminated. One concern we have not yet highlighted is the possibility of LLM-generated papers being submitted to journals and LLMs being used to review them. Such a situation would remove the guardrails that the review process is supposed to provide. When examining the issues with our current publication and reward structure, LLMs provide more harm than benefit.

That said, we cannot ignore the possible benefits of LLMs. For instance, they can help non-native speakers hone their English to get their work published in English-language journals. Thus, they might help remove linguistic bias that could prevent good articles from being published. They also allow researchers to offload some tasks, thereby allowing them more time to focus on research, peer review, and writing. They may enable more

[1] I would like to thank the anonymous reviewer who pressed me to consider this point.

effective data analysis in certain situations. But if the rewards structure that arises from the publish-or-perish mentality were changed, none of these benefits would be removed. The main difference is that some researchers would not feel compelled to publish in the way they are now.

However, the ethical concerns raised by LLMs extend beyond the basic utilitarian calculus presented above. Consider the role of justice or fairness in publishing. While I do not wish to suggest that issues of fairness would disappear if we eliminated LLMs from the equation, I argue that LLMs pose an important new challenge to the idea of fairness embedded in the purist view of publishing and its reward structure.

When describing the "purist" view, Chan notes that authors will have published "sufficient original studies to be deemed experts in the field" (2018, p.6345). This is why "the researcher is recognized for his or her work, and the volume of his or her publication record is regarded as supportive evidence of the researcher's expertise in the field and as justification in grant applications of his or her ability to solve important scientific problems" (Chan, 2018, p.6345). As Chan indicates, this reward structure is based on desert: it assumes that good scholars deserve recognition because they have produced original research. For the reward structure to make sense, one must be the author of that research. Those who have tried to publish an original research paper understand the effort that goes into the process. Good research requires numerous hours of intensive work. The rewards that one reaps are based on the belief that scholars have conducted their research legitimately and examined both their own work and the work they are citing. LLMs undermine this process. They provide the appearance that the "author" has engaged in careful research.

The violation of fairness in publishing is nothing new. Outsourcing one's academic work to an LLM is similar to utilizing a paper mill. In both cases, the papers produced are unreliable, and the individuals who use them receive credit they do not deserve. But LLMs are more pernicious for several reasons. First, if one uses a paper mill, there is a cost associated with acquiring the fraudulent paper. For some LLMs, only internet access is required. Thus, violations of fairness are likely to become more common. Second, many paper mills engage in plagiarism or employ tortured phrases, thereby making violations of fairness easier to detect. While many LLMS still produce work that sounds stilted, (a) they can be trained and (b) they are continuously improving, making their productions harder to detect.

The final worry is more theoretical and perhaps most troublesome. In "Biomedical retractions due to misconduct in Europe: characterization and trends in the last 20 years," Freijedo-Farinas et al. note that southern European countries have higher rates of retractions due to duplication and plagiarism. To help explain this, they write, "It is possible that southern European countries…do not tend to perceive plagiarism as being a cause of research misconduct" (2024, p.2878). If one does not believe that what they are doing constitutes misconduct, they are less likely to avoid that kind of behavior. Whether or not this explains why plagiarism and duplication are more prevalent in some countries, a similar issue could arise with LLMs.

According to the purist view of publishing, individuals are responsible for what they produce. If I publish groundbreaking research, I should be rewarded. But that is not the full story: how that work is produced is at least as important as what is produced. If I publish a groundbreaking paper supported by evidence and argument, that alone is not sufficient to justify field renown. If, after publishing the paper, I announce that it was really someone else's work, I would likely be vilified. What I did was unjust; by publishing it in my name, I took credit for something I did not do.

My concern is that the use of LLMs could shift our views on justice and fairness in publishing. Some professors are allowing students to use LLMs under certain conditions, researchers are acknowledging the use of LLMs in various ways, and academic institutions are promoting LLMs. The more common LLM use becomes, the more likely people are to stop recognizing it as problematic. While some uses of LLMs may be legitimate, other uses constitute plagiarism. But as their use becomes more commonplace, the lines of what constitutes legitimate and illegitimate academic use will likely become fuzzier.

Aside from skewing our conception of academic fairness, this can have other real-world ramifications. When going up for promotion or tenure, researchers who have done their own research—those who mostly closely align with the purist view—can be compared to those who are surreptitiously taking advantage of the system using LLMs. If what matters to a tenure or promotion committee is the number of publications, we may lose legitimate scholars because of the prolific output of academic fraudsters. As noted above, these violations of fairness can persist even if we change the publish-or-perish mindset; however, by reducing pressure to publish and providing alternative avenues for promotion and job security, we might be able to decrease their frequency.

5. Possible Solutions to the Publish or Perish Problem[2]

The first step to fixing the problem is to disincentivize publishing. But rather than penalizing people for publishing (e.g., by charging authors to submit publications for review), we should disincentivize it by incentivizing alternative activities. I believe that this first step is necessary for every field. There are simply too many papers being published, too few reviewers to properly vet the publications, too much pressure from publishers to accept papers, too many people willing to take advantage of the system, and, with the rise of LLMS, too many ways to game the system. But the question remains: what should we do in lieu of publishing?

I do not believe that the answer to this question is "one-size-fits-all." Thus, different fields and different universities will need to carefully consider their goals and how publication aligns with those goals. For R1 institutions, where funding is tied to grants and grants are often tied to publications, large-scale changes will likely be more difficult to enact. Even if we have strong empirical evidence that more publications do not necessarily advance fields, the economic viability of these institutions remains too closely tied to the current model. Thus, some of these changes will likely need to occur within institutions that are more focused on teaching—at least until we can implement other structural

[2] I would like to thank the anonymous reviewer for providing numerous helpful suggestions in the section.

changes to educational funding. But if headway can be made in operationalizing different tenure and promotion mechanisms at teaching institutions, perhaps we could then find ways to translate them to more research-oriented schools.

While I do not have an answer for every field or type of institution, I do have some suggestions for how we could recognize academic expertise without focusing primarily on publications.[3] Before presenting these suggestions, I want to clarify the terminology I will employ. Below, I describe some publications as "unnecessary." When I use the term, I am referring to publications that engage in some form of fraud, or publications that the authors feel coerced to produce, and/or those they believe are not yet ready for publication but are sent out simply to help their case for tenure, promotion, or renown. That is, they are publications that only exist because the current system incentivizes scholars to produce them.

First, it will help to consider the epistemic goal of publishing. If the main epistemic goal of publications is to advance fields and to keep individuals aware of those advances, many institutions could give the same credit to presentations at conferences as they do to publications. While some published papers advance fields, it is also clear that the overabundance of publications is detrimental. Presenting at conferences could reduce paper bloat while also facilitating peer review. Assuming the audience comprises professionals who care, one can give a room of scholars the opportunity to assess their work. While one can still utilize LLMs to write a presentation (thus concerns of justice and fairness are still present), this might help cut down on AI-generated publications, salmi publications, and unnecessary publications in general. This would also likely reduce the burden on editors to identify peer reviewers. This may be the simplest solution, as many institutions already count presentations as a form of scholarly output. Whatever method promotion and tenure committees currently use to assess presentations could still be used, but greater weight could be given to them.

Second, and perhaps more controversially, certain institutions could place greater weight on attending conferences. For example, I teach at a teaching university. While some of the university's research advances fields, that is not its primary role; the primary role is teaching. The teaching load is four courses per semester. While our publication requirements are certainly lower than those of an R1, it is not clear why they exist. If the institution's goal is to teach students, then job performance should be tied to teaching. My university distinguishes between "creative activity," such as publications and presentations, and "professional activity," such as attending conferences and serving as a peer reviewer. The former are given more weight. But attending a conference on pedagogy might do a great deal more for one's ability to disseminate information to students than numerous publications ever could; it can also keep one abreast of advances in the field. However, to count conference attendance as meaningful, institutional mechanisms would need to be developed to enable tenure committees to evaluate it equitably. One possibility is requiring in-depth narratives. The required length and content could be set by

[3] For more possible solutions, see Davies and Felappi, "Publish or Perish," 753.

institutions to better align with their goals. The content could include information explaining why one attended the conference, which panels one attended, how conference attendance will affect one's teaching or research, and, if applicable, how attending the conference aligned with the institution's goals.

Third, at schools with a teaching focus, far greater emphasis could be placed on evaluating one's teaching. Using a combination of student evaluations, peer teaching reviews, reviews of teaching materials (e.g., handouts, PowerPoint slides), and multi-page, in-depth narratives, one's path to tenure could be through teaching. In the narratives, one could be expected to carefully and critically evaluate one's performance, providing in-depth explanations of what one did well and what needs improvement. By removing the publication requirement, we could expect these instructors to spend more time reflecting on their roles. These narratives could be required for every section one teaches. Furthermore, they could build on previous narratives. In subsequent years, they would indicate the steps taken to address areas needing improvement.

My next suggestion is also likely controversial, but I believe it would provide the most help in alleviating some of the issues in academic publishing. One way to help control some of the major issues we have considered—e.g., paper mills, salami publications, fraudulent papers, superfluous or false citations, unreproducible results, and unnecessary publications—is to implement stronger oversight and improved review practices across academic fields. But scholars are given little motivation to be a reviewer, let alone a careful reviewer. Thus, some institutions could give the same credit for serving as a paper reviewer as they do for paper publishing. To be effective, this would require both institutional mechanisms for evaluation and changes to the journal review process, but those changes would hopefully address some of the problems with publications. First, because this would count as a publication, the review process could take a much more rigorous form. Journals could require reviewers to provide a detailed summary of the article, check for other similar research in the field (noting how they checked for similar research), check the veracity of references, check figures, note possible errors, and provide suggestions for improvement. As free plagiarism-detection software becomes more sophisticated, it may be necessary to run the paper through such software. They could be expected to thoroughly detail both what the paper does well and areas that need improvement. In the best case, journal editors could provide a comprehensive template that reviewers would need to complete to be credited with completing the review. Editors could then send email documentation to the reviewer that the review meets the journal's standards. Tenure and promotion committees could then credit faculty for providing verified peer reviews.[4] To help ensure that individuals do not take advantage of the system, universities could perhaps require that anonymized copies of reviews be included in the promotion or tenure dossier so that committees can see the work that went into the review and whether or not it meets their institution's standards.

[4] I would like to thank an anonymous peer reviewer for this suggestion.

While some journals may already expect reviewers to perform this kind of work, incentivizing reviewing by linking it to job security would enable editors to ask more of reviewers. Reviewing papers would not be a "best practice" or something one is expected to do on the side, but instead another path toward tenure and promotion. And while this process could be exploited just as publications are (e.g., by using LLMs to perform reviews or by charging individuals to serve as reviewers), it could also generate a much more comprehensive review system while giving individuals a path to tenure that does not require the production of unnecessary articles. This would also likely help editors recruit reviewers; if people know that careful reviewing could lead to tenure and promotion, editors will likely get many more volunteers. Journals could establish a system for scholars to apply to serve as reviewers and to hold reviewers accountable. If individuals submit incomplete reviews, reviews lacking appropriate rigor, or fail to complete the review within the allotted time, editors could inform them that the review did not meet their standards.

While these suggested changes might reduce the number of publications produced each year, I do not believe that publications would cease at institutions that provide alternative avenues for promotion and tenure. However, I believe that the goals of many scholars publishing at these institutions would be more closely aligned with advancing the field. They would publish because they believe they have something important to contribute to the academic discourse, not to check boxes for tenure and promotion. Again, this strategy might not work for every institution, but if some were able to adopt it, there would likely be a decrease in unnecessary publications from those universities and less pressure on academics to use shortcuts such as LLMs to obtain job security.

The biggest practical problem with this solution is that institutions would need to accept the changes to the current process. Administrators may see presenting papers, attending conferences, teaching, and reviewing papers as an easy path to tenure and promotion; but, again, more rigor could be added to these activities and how they are reported. The aim would be to align tenure and promotion more closely with each university's goals than with the general goal of simply publishing. While some details of the processes may need to be discovered and corrected through trial and error, providing researchers with a pathway to tenure and promotion that does not require publication could significantly mitigate some of the issues with current academic publications. At the very least, individuals could publish because they believe they have important contributions to make to the field, rather than because their job depends on it. Furthermore, it would incentivize people to be careful reviewers in their field, thereby reducing poor research.

6. Conclusion

I end by clarifying something I mentioned at the start: that rather than incentivizing authors to publish scores of papers yearly, we should treat this occurrence with greater skepticism. I want to be clear that the goal here is not to champion academic witch-hunts. It is instead to get authors to think carefully about their contributions to their fields as well

as (a) whether a particular publication is worthwhile and (b) whether they deserve to be listed as an author. When quantity is sought, we not only incentivize the bad actions noted above but also incentivize individuals to force their way into co-authorship.[5] This is particularly problematic when there is a power disparity between the paper's author (who may be a graduate student or untenured faculty member) and an established figure in the field who seeks to be included on the paper.

The skepticism I describe is also meant to raise the question: how plausible is it that hyper-prolific authors could *carefully review* the significant number of publications attributed to them (either as first author or co-author)? We need to seriously consider this question, as LLMs will significantly increase the number of publications one can produce. While some authors may be able to carefully vet the numerous publications they produce each year, we have evidence (e.g., academic fraud, undisclosed use of LLMs, unreproducible research) that many do not. Treating cases with greater skepticism would, hopefully, encourage prolific authors, especially those whose publications do little to advance their fields, to reconsider their publishing strategies.

Funding: This research received no external funding.

Acknowledgments: I would like to thank Krista Capps, Ricardo Friaz, Audra King, Tom Story, Tara Weese, and an anonymous reviewer for their time and numerous helpful suggestions.

Conflicts of Interest: The author declares no conflict of interest.

References

1. Baker, M. (2016). 1,500 Scientists lift the lid on reproducibility. *Nature* 533, 452–454, https://doi.org/10.1038/533452a
2. Cabanac, G., Labbé, C., & Magazinov, A. (2021). Tortured phrases: A dubious writing style emerging in science. Evidence of critical issues affecting established journals. *arXiv preprint arXiv:2107.06751*.
3. Chan, W. (2018) What Is the Value of Publishing? *ACS Nano12* (7), 6345-6346 DOI: 10.1021/acsnano.8b05296
4. Clark J. (2023). How covid-19 bolstered an already perverse publishing system. *BMJ* 380, 689. doi:10.1136/bmj.p689
5. Dattaro, L. (April 19, 2023). Imaging journal editors resign over 'extreme' open-access fees. *The Transmitter*. https://www.thetransmitter.org/publishing/imaging-journal-editors-resign-over-extreme-open-access-fees/?swcfpc=1
6. Davies, B. & Felappi. G. (2017). Publish or perish. *Metaphilosophy*, 48(5), 745-761. https://www.jstor.org/stable/26602102.
7. Freijedo-Farinas, F., Ruano-Ravina, A., Pérez-Ríos, M., Ross, J. & Candal-Pedreira, C. Biomedical retractions due to misconduct in Europe: characterization and trends in the last 20 years. *Scientometrics* 129, 2867–2882 (2024). https://doi.org/10.1007/s11192-024-04992-7.
8. Grey, A., Avenell, A., & Bolland, M. J. (2025). Ten years later: Assessments of the integrity of publications from one research group with multiple retractions. *Accountability in research*, *32*(4), 488–508. https://doi.org/10.1080/08989621.2023.2295996

[5] For example, see: Britta S. von Ungern-Sternberg, Adrian Regli, Bojana Stepanovic, and Karin Becke-Jakob, "Authorship misconduct: professional misconduct in editorial handling of authorship" *British Journal of Anaesthesia*, 133 (6): 1134-1136 (2024).

9. Hamilton D. P. (1990). Publishing by--and for?--the numbers. *Science*, 250(4986), 1331–1332. https://doi.org/10.1126/science.2255902
10. Hanson, M. A., Barreiro, P. G., Crosetto, P., & Brockington, D. (2024). The strain on scientific publishing. *Quantitative Science Studies*, 5(4), 823–843. doi: https://doi.org/10.1162/qss_a_00327
11. Kincaid, E. (July 5, 2023). Editors of public health journal resign over differences with publisher. *Retraction Watch.* https://retractionwatch.com/2023/07/05/editors-of-public-health-journal-resign-over-differences-with-publisher/
12. Kwon, D. (December 15, 2021). *Nature's* 10: Ten people who helped shape science in 2021. *Nature*. https://www.nature.com/immersive/d41586-021-03621-0/index.html?utm_medium=Social&utm_campaign=nature&utm_source=Twitter#section-gM9iO4XBRl
13. LitCovid: A literature hub for tracking up-to-date scientific information about the 2019 novel Coronavirus. Retrieved July 6, 2025, from https://www.ncbi.nlm.nih.gov/research/coronavirus/
14. Mann, S. P,, Earp, B. D., Møller, N., Vynn, S., & Savulescu, J. (2023). AUTOGEN: A personalized large language model for academic enhancement—ethics and proof of principle. The American Journal of Bioethics, 23(10), 28–41. https://doi.org/10.1080/15265161.2023.2233356
15. Marcus, A. (July 12, 2021). Elsevier says, 'integrity and rigor' of peer review for 400 papers fell "beneath the high standards expected. *Retraction Watch*. https://retractionwatch.com/2021/07/12/elsevier-says-integrity-and-rigor-of-peer-review-for-400-papers-fell-beneath-the-high-standards-expected/
16. O'Grady, C. (May 31, 2024). Software that detects 'tortured acronyms' in research papers could help root out misconduct. *Science*. https://www.science.org/content/article/software-detects-tortured-acronyms-in-research-papers
17. Oransky, I. (December 27, 2022). Nearing 5,000 retractions: A review of 2022. *Retraction Watch.* https://retractionwatch.com/2022/12/27/nearing-5000-retractions-a-review-of-2022/
18. Papers and peer reviews with evidence of ChatGPT writing. *Retraction Watch.* Retrieved July 6, 2025, from https://retractionwatch.com/papers-and-peer-reviews-with-evidence-of-chatgpt-writing/
19. Peterson, C. J., Alexander, R., & Nugent, K. (2022). COVID-19 Article retractions in journals indexed in PubMed. *The American journal of the medical sciences*, *364*(1), 127–128. https://doi.org/10.1016/j.amjms.2022.01.014
20. Publishing futures: Working together to deliver radical change in academic publishing. Cambridge University Press. https://www.cambridge.org/core/services/aop-file-manager/file/68ef9df9a6c3702e57eae12a/Cambridge-University-Press-Publishing-futures.pdf
21. Rawat, S. and Meena, S. (2014) Publish or Perish: Where Are We Heading? *Journal of Research in Medical Sciences*, 19, 87-89.
22. Retracted Coronavirus (COVID-19) Papers. *Retraction Watch*. Retrieved July 6, 2025, from https://retractionwatch.com/retracted-coronavirus-covid-19-papers/
23. Retraction Watch Leaderboard. *Retraction Watch.* Retrieved July 6, 2025, from https://retractionwatch.com/the-retraction-watch-leaderboard/
24. Retraction Watch Mass Resignations List. *Retraction Watch,* Retrieved July 6, 2025, from https://retractionwatch.com/the-retraction-watch-mass-resignations-list/
25. Supak Smolcić V. (2013). Salami publication: definitions and examples. *Biochemia medica*, *23*(3), 237–241. https://doi.org/10.11613/bm.2013.030
26. van Dalen, H.P., & Henkens, K. (2012). Intended and unintended consequences of a publish-or-perish culture: A worldwide survey. *Journal of the American Society for Information Science and Technology*, 63(7), 1282-1293. https://doi.org/10.1002/asi.22636
27. Van Noorden, R. (2013). Brazilian citation scheme outed." *Nature* 500, 510–511, https://doi.org/10.1038/500510a

28. Van Noorden, R. (2017). The science that's never been cited. *Nature* 552, 162-164. *doi:* https://doi.org/10.1038/d41586-017-08404-0
29. Van Noorden, R. (2023). More than 10,000 research papers were retracted in 2023 — A new record. *Nature.* 624, 479-481. *doi:* https://doi.org/10.1038/d41586-023-03974-8
30. Van Noorden, R. & Chawla, D. S. (2019). Policing self citations. *Nature,* 572, 578-579, *doi: https://doi.org/10.1038/d41586-019-02479-7*
31. von Ungern-Sternberg, B. S., Regli, A., Stepanovic, B., & Becke-Jakob, K. (2024). Authorship misconduct: professional misconduct in editorial handling of authorship. *British Journal of Anaesthesia*, *133*(6), 1134–1136. https://doi.org/10.1016/j.bja.2024.08.015
32. Weinberg, J. (April, 27, 2023). Wiley removes Goodin as editor of the *Journal of Political Philosophy* (Updated). *Daily Nous*. https://dailynous.com/2023/04/27/wiley-removes-goodin-as-editor-of-the-journal-of-political-philosophy/
33. Weinberg, J. (May 22, 2024). Editors at *Philosophy & Public Affairs* resign; will launch new OA journal. *Daily Nous*. https://dailynous.com/2024/05/22/editors-at-philosophy-will-launch-new-oa-journal/
34. Weintraub, P. (April 8, 2010). The doctor who drank infectious broth, gave himself an ulcer, and solved a medical mystery. *Discover Magazine.* https://www.discovermagazine.com/health/the-doctor-who-drank-infectious-broth-gave-himself-an-ulcer-and-solved-a-medical-mystery
35. Zhang, M, Wu, L., Yang, T., Zhu B., & Liu, Y., (2024). RETRACTED: The three-dimensional porous mesh structure of Cu-based metal-organic-framework - Aramid cellulose separator enhances the electrochemical performance of lithium metal anode batteries. *Surfaces and Interfaces*, 46, 1-7, https://doi.org/10.1016/j.surfin.2024.104081.