Podcast Interview: Kyle Siler

PNAS: Welcome to Science Sessions. I’m Paul Gabrielsen. Editors, especially those at prestigious scientific journals, often face high volumes of submissions and are limited by constraints of time and publication space. Many submitted manuscripts, of necessity, are “desk-rejected,” meaning they are rejected by the editor without being sent for peer review. Does this process ensure that journals select and publish the best work? Kyle Siler, a postdoctoral scholar at the University of Toronto’s Rotman School of Management, asked that question in a recent PNAS study. Siler and his colleagues examined the citation count of papers accepted by three major medical journals, as well as the citation count of papers initially rejected by those journals but published elsewhere. I spoke with Siler over the phone.

Siler: On the whole we found that peer-review was effective, but a bit surprisingly to us, the top 14 cited articles in our dataset, and this is roughly the top 2%, they were all rejected by our three major journals, and most were desk-rejected. And since citations in science are almost always exponentially distributed, these 14 top-cited articles were far and away the most cited.

I’d kind of guess that there’s three main explanations for why this sort of thing happens. And I guess first I’d say, brilliant work often threatens the status quo, and gatekeepers tend to be rather embedded in the status quo, and brilliant, groundbreaking, paradigm-shifting work can only be evaluated by existing knowledge, which then makes it more difficult to evaluate. And, I’d also add, that in general, most new ideas are bad ideas. . . So it makes sense that people are kind of resistant to things that are new, things that are exciting, but once again, these are kinds of things that become revolutionary science.

Secondly, I’d also add that diffusion and contagion, the processes by which an innovation goes viral, it’s often based on luck or arbitrary factors, often influenced by randomness or social elements. People follow herds, and highly cited articles tend to be cited often vaguely or incorrectly as broad exemplars of big ideas, as opposed to the actual quality of the research.

And thirdly, I have to really emphasize that popularity does not always imply quality in science. I mean, I’m sure many people can think of an article, or for that matter a musical band, a TV show, where you just scratch your head and wonder, how on Earth did this ever become popular? Why do people pay attention to this? I might add that lowest-common-denominator work may have broader diffusion potential than complex, esoteric contributions.

PNAS: What brought about this study?
Siler: Well, I’d say it was serendipity, like is often the case in science. I was presenting other research at a conference, and mused that studying peer review is pretty difficult due to the sensitivity and confidentiality of the subject matter. So after expressing these limitations publicly, a very generous colleague gave me a tip that there were these pharmacologists at UC San Francisco that had extensive archives of peer-review decisions at three elite medical journals. Now this seemed too good to be true to me, because nobody has ever had this kind of data, and I’ve always been fascinated about gatekeeping in risky and creative professions. So I was pretty determined to get access to the data set, so I flew to San Francisco, I pitched the idea of dusting off these archives to see if editors really do pick the most popular articles to publish.

PNAS: How do gatekeepers make decisions regarding manuscripts?

Siler: Right, well I’ll preface my answer here by acknowledging that these editors have a very difficult job. They’re fielding over 1,000 papers annually, and they have very finite resources in terms of their own time, as well as the time and labor of scientists that they can tap to be peer-reviewers. I’d argue that these desk rejections are a very necessary evil. With over 1,000 articles annually, you just can’t devote the time and resources to give a full review to everybody expeditiously. And I’ll state really clearly, that despite the fact that a number of very highly cited articles were desk-rejected, on the whole desk-rejections were effective on the whole. Editors making snap decisions on the whole were able to kind of predict which articles will become more popular down the road. I guess to explain how desk-rejections work, I’ll borrow the heuristic thinking notion from Daniel Kahneman, who’s a famous Nobel-winning psychologist, and suggest that when we have to make decisions quickly, we base them on existing frames. And in science this means judging work kind of vis-à-vis obvious and established paradigms and conventions in the field. This may be efficient and effective on the whole, but it’s also likely to overlook ideas and results that lie outside the status quo. And so to quote former editor of the American Economic Review, Preston McAfee, a journal that does not occasionally reject a highly-cited paper will publish lower-quality articles on the whole.

We can observe errors of commission that journals make, when a journal publishes a dud, and no journal is immune to this, including PNAS. But it’s much harder to observe errors of omission - the brilliant idea that never gets published, or gets consigned to an obscure outlet and nobody notices it. And so if you want to be a leading journal with cutting-edge, counterintuitive, provocative findings, in theory this will increase the risk of errors of commission because counterintuitive, provocative findings are much more likely to be proven wrong.

The middle of the road is often the most popular, and despite the positive efforts of gatekeepers, the cream does not always necessarily rise to the top. In science this is true
in the sense that elite journals may not identify the most brilliant work. On the other hand, scientists may not always elevate the best work with their attention and citation choices. . .The cream just does not always necessarily rise to the top.

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