

Einstein's papers were never peer reviewed (except for once when it failed)

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Will present the evidence that it seems Einstein's papers were never peer reviewed (except once). His papers were treated as speculation thus not needing peer review; where policy was not to be too critical of speculation for fear that if were too critical then would be stifling of new ideas. Einstein laughed and thought it was amusing that his papers were full of mistakes.

Einstein was used to German journals just publishing his work without peer review, and when he encountered peer review in America of one his papers that was critical of his paper, he was very angry about it. That seems to be the only time he was peer reviewed and all his published papers were published without peer review.

The evidence that Einstein seems to have experienced peer review only once (in which he failed) and that all his works were published without peer review comes from article in *Physics Today* 2005. [1]

I will now go through the relevant parts: During Einstein's "first period he generally published in German physics journals, most famously the *Annalen der Physik*, where all five of his celebrated papers of 1905 appeared."

Then he went to America, and: "Einstein began to publish frequently in North American journals."

He hit a problem with *Physical Review*, where he: "first published there in 1931 on the first of three winter visits to Caltech. With Nathan Rosen, his first American assistant, Einstein published two more papers in the *Physical Review*: the famous 1935 paper by Einstein, Boris Podolsky, and Rosen (EPR) and a 1936 paper that introduced the concept of the Einstein–Rosen bridge, nowadays better known as a wormhole. But except for a letter to the journal's editor he wrote in 1952—in response to a paper critical of his unified field theory work—that 1936 paper was the last Einstein would ever publish there."

The reason he stopped publishing with *Physical Review* was because he had a bad review from them:

"Einstein stopped submitting work to the *Physical Review* after receiving a negative critique from the journal in response to a paper he had written with Rosen on gravitational waves later in 1936."

Now in 2005:

"Last March, the journal's current editor-in-chief, Martin Blume, and his colleagues uncovered the journal's logbook records from the era, a find that has confirmed the suspicions about that referee's identity. Moreover, the story raises the possibility that Einstein's gravitational-wave paper with Rosen may have been his only genuine encounter with anonymous peer review. Einstein, who reacted angrily to the referee report, would have been well advised to pay more attention to its criticisms, which proved to be valid."

The referee seems to be Robertson, and it turns out:

"The *Physical Review* received Einstein's submission on 1 June 1936, according to the journal's logbook. Tate returned the manuscript to Einstein on 23 July

with a critical review and the mild request that he “would be glad to have [Einstein’s] reaction to the various comments and criticisms the referee has made.” Einstein wrote back on 27 July in high dudgeon, withdrawing the paper and dismissing out of hand the referee’s comments:

Dear Sir,

We (Mr. Rosen and I) had sent you our manuscript for publication and had not authorized you to show it to specialists before it is printed. I see no reason to address the—in any case erroneous—comments of your anonymous expert. On the basis of this incident I prefer to publish the paper elsewhere.

Respectfully,

P.S. Mr. Rosen, who has left for the Soviet Union, has authorized me to represent him in this matter.”

In reply, the editor Tate gave:

“On 30 July, Tate replied that he regretted Einstein’s decision to withdraw the paper but stated that he would not set aside the journal’s review procedure. In particular, he wrote, “I could not accept for publication in THE PHYSICAL REVIEW a paper which the author was unwilling I should show to our Editorial Board before publication.””

It was noted that Einstein published his paper elsewhere but heavily revised:

“The paper was, however, subsequently accepted for publication by the *Journal of the Franklin Institute* in Philadelphia, a periodical in which Einstein had already published. The paper appeared with radically altered conclusions in early 1937. A letter dated 13 November 1936, from Einstein to the journal’s editor, indicates that the institute had accepted the paper in its original form: Einstein simply explained why “fundamental” changes in the paper were required because the “consequences” of the equations derived in the paper had previously been incorrectly inferred.”

There was a difference in how physics journals were dealt with in America from how dealt with in Germany:

“German journals in the early part of the 20th century were considerably less fastidious than the *Physical Review* about what they published. Infeld claimed that the German attitude, in contrast to that prevailing in Britain and America, was “better a wrong paper than no paper at all.” In a March 1936 letter to Einstein, the relativist and fellow European exile Cornelius Lanczos, who had

himself been on the receiving end of one of Robertson's reports, remarked on "the rigorous criticism common for American journals" such as the *Physical Review*.

Historians Christa Jungnickel and Russel McCormmach have studied in some detail the editorial policies of *Annalen der Physik*, the leading German journal of the early 1900s, and note that "the rejection rate of the journal was remarkably low, no higher than five or ten percent." They describe the editors' reluctance to reject papers from established physicists, even relatively junior ones. As they put it, "Now and then the journal published bad papers by good physicists." In one specific example, editor Paul Drude annoyed Max Planck by printing what Planck considered a worthless paper, whose author had "appealed to [Drude] personally, and Drude lacked the heart to refuse him."

"Planck's own editorial philosophy was to "shun much more the reproach of having suppressed strange opinions than that of having been too gentle in evaluating them." In America things were different, although Robertson and Tate surely treated Einstein more gently than they would have many others. Indeed, Robertson, in his very next report to Tate, commented that the author "is a man of good scientific standing, and it would seem to me that if he insists, he has more right to be heard than any single referee has to throttle!" That dispute turned more on matters of interpretation, though, and when it came to a paper that might actually be wrong, even an Einstein had to be queried, however gently."

"Doubtless the rigorous criticism may have come as something of a shock to Einstein, who was accustomed to gentler treatment early in his career. However, Einstein could be very frank and direct in his criticism of others' work. From 1914 on, as a member of the Prussian Academy of Sciences, he was regularly called on to review articles submitted to the academy's proceedings. The German word for worthless frequently occurs in those brief reviews. As a member of the academy, Einstein had his papers published without question or revision. Anything less must have seemed to him a tremendous slight."

"In his letter to Einstein, Tate had carefully avoided stating that anonymous review by the editorial board or others was a necessary step in the acceptance of a paper by the journal. In fact, the *Physical Review* logbook suggests that neither of the two previous papers by Einstein and Rosen, including the one with Podolsky, had been sent to a referee: In both cases the field for the referee's name was left blank, and the EPR paper was sent for publication the day after its receipt at the journal. Therefore it is likely that the gravitational

wave paper was Einstein's first encounter with the anonymous peer-review system practiced in American journals at the time."

"That Tate chose to have the 1936 paper refereed is interesting. After all, Einstein's two previous submissions were certainly controversial. EPR is arguably the most controversial paper Einstein ever published, and the Einstein–Rosen bridge paper was part of an ongoing controversy with Ludwig Silberstein. Einstein and Rosen's letter to the *Physical Review* in 1935 was part of this same debate. Tate published both of those papers without outside advice."

"A paper purporting to prove that gravitational waves did not exist, though, apparently sounded alarms with him. Nowadays one imagines that most physicists of the time knew little and cared even less about general relativity. But apparently gravitational waves were already such a well-accepted prediction of the theory, despite the absence of experimental support, that such a surprising result warranted some scrutiny. More than a month elapsed between receipt of the paper and its referral to Robertson. The delay certainly suggests hesitation on Tate's part, and may even be evidence of an initial round of editorial discussion."

While Infeld tried to be careful in writing a book, Einstein inquired what Infeld was doing and when he found out, *Physics Today* says: "Einstein laughed his loud laugh and replied: 'You don't need to be so careful about this. There are incorrect papers under my name too.'"

So, presumably Einstein knew that his papers were not be subjected to being demanded high standards of accuracy.

In 1919 Einstein became famous, and suddenly his papers (with all their uncorrected mistakes) were deemed to be a revolution in physics. The headlights being:

LIGHTS ALL ASKEW IN THE HEAVENS

Men of Science More or Less
Agog Over Results of Eclipse
Observations.

EINSTEIN THEORY TRIUMPHS

Stars Not Where They Seemed
or Were Calculated to be,
but Nobody Need Worry.

A BOOK FOR 12 WISE MEN

No More in All the World Could
Comprehend It, Said Einstein When
His Daring Publishers Accepted It.

Given that Einstein's work was just speculation without being properly checked pre-1919, one might expect that when it was deemed important work that it should then been checked over in fine detail, but that appears not to have been done. Instead there was arguing over it being wrong, and the people saying it was wrong tended to be ignored.

Reference

[1] Einstein Versus the *Physical Review*, Daniel Kennefick,
Physics Today 58, 9, 43 (2005); <https://doi.org/10.1063/1.2117822>

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