Sir,

In a recent issue of Nature (*Nature* **434**, 956) Steve Donovan expressed his opposition to an all-online submission and peer-review system, based on the very sensible argument that the multiplicity of file formats sometimes causes much trouble to visualize the e-mailed documents. Although it seemed that this could be easily corrected by the generalized use of a free and universal file format such as PDF, another noticeable issue with online publishing and referring is that it could leave a number of poorly equipped nations out of the scientific community. But I want to comment on a single sentence in Donovan's correspondence: "it took me less than an hour to review the associated paper". I am not sure if this short amount of time includes the typing (or hand-writing?) of the corresponding referee's report, but this seems to me a very short time for an act that is usually hailed as the cornerstone of the scientific work and credibility. True, I am no expert in Paleontology, and maybe the paper Donovan is referring to was indeed assessable in such a brief period. But I often have the impression when receiving referee's report (either favorable or negative) on work I have contributed to, that they were also written and thought over in "less than an hour", when I would prefer a salutary criticism of oversights and imprecision. I am personally spending too long a time referring each individual manuscript I accept to review (often time a full week-end or more) and sometimes end up writing overly long reports. The reason is, that this is the time needed for me to check the cited literature, perform calculations or try to interpret the data in a (generally) less fashionable way than the authors did. If I fail to find any potential flaw in the paper, I consider the manuscript as acceptable, and limit my comments to remarks aiming at clarifying possible ambiguities. If I find some weakness or erroneous result, I feel obliged to state them clearly, and suggest to the authors to either tone done their claims or substantiate them more convincingly. Admittedly, such referee's reports are not necessarily pleasant to read, and mistakes can be made by the referee on these occasions, but often time the revised version of the article turns out to benefit from such an exchange. In a famous story, Einstein wrote to the Physical Review editor: "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." As the story goes, Einstein eventually discovered a mistake in his calculation (precisely the one pointed out by the referee), and never published the original version. In brief, since the refereeing process is in practice the only way to prevent false results or erroneous claims to be published, and thus delay our construction of a clear understanding of Nature, we should be both cautious not to make it unnecessarily difficult (as reminded by Donovan), but at the same time, not trivialize it into a mere social mannerism. And yes, this activity is not recognized in career advancement committees. But is the time spent reading scientific journals such as Nature?

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