Are we making progress?

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Almost every day, large numbers of articles come to our desk or computers. It is impossible to read everything that is interesting, important, or relevant. It is even impossible to select what is important or relevant. We rely on the editors of journals, writers of reviews, or presenters at congresses to select what is important and not. And we realize (hopefully) that some work in important journals is not so relevant and work from unknown persons in less prestigious journals can be groundbreaking. At the fortunate age of almost 60, there is lot to look back on, and a nice example of that notion comes from own experience. When we discovered in the late 1980s of the previous millennium that healthy individuals can have cancer-associated translocations, we had a great difficulty in publishing our article. After rejection by several high-impact journals, we finally had it in the public domain and our data were rapidly confirmed. When I presented the work with some more details and further discoveries a year later, the notion that a genetic alteration does not mean there is cancer was so common knowledge that my presentation was not felt as something new. Just a year before, the article had been rejected with a note from the editor that he did not know what we did wrong but that it was impossible that a translocation could be present in a person without cancer. If you find something that does not fit into the concept of a reviewer or editor, it is not so easy to get your data published.

Our work, however, had been based on a sound hypothesis that was actually not out of sync with the existing paradigm: cancer develops through a series of steps, so cancer-associated alterations, hallmarks of cancer, may be present, can even be expected to be present, in individuals without cancer. Nowadays no one is surprised by this, but in the late 1980s, it was apparently a problem. At that time, we wanted to see whether the t(14;18) translocation is an early event, so early that it might be present in normal individuals.

In the ideal scientific world, one dreams up an interesting idea, formulates a hypothesis, and designs experiments that confirm or reject the hypothesis. In pathology, this is often not the approach. One may see something unusual through the microscope and tries to find more of it. Alternatively, a large series of cases is collected and all kinds of features are noted. Yet another approach is to use a new technique, antibody, or method on an existing material. All this work is generally done using a rigorous scientific approach and brought to the public domain, but is it also all worthwhile reading?

In the Journal of Hematopathology, we try to publish articles that can help the hematopathologist in daily work. In the literature review, this approach is also taken: I try to select from the huge amount of articles those that I find interesting. These are sometimes published in high-impact journals, by well-known authors. But others come from obscure journals from not so well-known colleagues (at least by me). Going over the articles, I notice each time that there is a lot that makes me feel on the one hand overwhelmed, on the other hand also surprised by the many articles that I feel are not interesting or relevant at all. Of course, this is only my opinion and I may miss very groundbreaking work because I fail to see the real meaning. Nevertheless, would it not be good when we are a bit more reflective and critical when we start to do research? I believe that we make progress, but we could do more with more critical thinking before doing.

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