Editorial

When and Where to Publish Important Findings: A Casualty of Biogerontology’s Rise to Respectability

Publish and be damned!

—Arthur Wellesley, Duke of Wellington

This issue of Rejuvenation Research marks a considerable, if ostensibly minor, milestone in our progress towards the goal of becoming the forum of choice for publishing results relevant to postponing aging. Zullo et al. report1 (and see also the Perspective by Khrapko2) the successful restoration of growth of cells in culture whose mitochondrial copy of a gene essential for oxidative phosphorylation has been inactivated. This rescue was achieved using a suitably modified version of the gene that had been incorporated into the nuclear DNA. Similar results have been reported in the past, but this is the first example of phenotypic rescue of an inactive mitochondrial gene by a stably integrated transgene; moreover, the assay for rescue is also considerably more unambiguous than in previously published work. I regard this as the most significant research yet published in RR, as it paves the way for the corresponding manipulation of all 13 mitochondrial protein-coding genes in human cells, something which can in principle also be achieved, in the future, by somatic gene therapy or by SCNT.3 Such “total allotopic expression” would make the mitochondrial DNA superfluous, thereby obviating the mitochondrial mutations that accumulate throughout life and have for over 30 years been suspected as contributing substantially to aging.4

But that’s not what this editorial is about. What I want to draw readers’ attention to is the fact that this work was actually completed nearly five years ago. It was submitted to Science and reviewed favourably overall, but the reviewers asked for some additional experiments (as reviewers for Science rather often do). These experiments turned out to be extremely challenging and were never completed. But they were not by any means essential to the reliability of the main result: on the contrary, they were relatively peripheral experiments that journals less prominent than Science but still eminently respectable would not have required. Nonetheless, it has taken until now for the work to be resubmitted to such a journal—and published with alacrity after being very positively reviewed by top experts in the field. I feel sure that the mitochondriology community’s enthusiasm for pursuing allotopic expression will be greatly enhanced by this publication, but also that it would have been similarly enhanced if the work had been published four years ago. In other words, a line of research possibly essential for the substantial postponement of human aging has been greatly delayed.

It would be bad enough if this were a one-off failure of the scientific publication system. Sadly, however, it is not. I can immediately think of three highly remarkable results in biogerontology that have gone unpublished for nearly as long, and thus remain known only to the “in crowd” who attend many scientific meetings or talk a lot to those who do. The maximum lifespan of already long-lived mice has been extended substantially by the ectopic expression of an antioxidant enzyme: this is the most unambiguous life-extension result ever achieved by a method not related to caloric restriction. A part of the eye has been identified that accumulates a much higher abundance of...
mitochondrial DNA deletions during aging than has been reported in any other tissue and may thus prove to be a superlative tissue in which to assay the effect of interventions on mitochondrial mutation accumulation. Finally, long-lived mice kept for only a couple of generations in the laboratory since being captured in the wild have been found to derive no benefit from caloric restriction, profoundly challenging one of the most robustly supported tenets in the whole of biogerontology. In all these cases, publication has been delayed because the researchers considered (or were advised) that the results would merit publication in top-flight journals only when accompanied by additional data, and obtaining that data has proven either very laborious or of lower priority than other (unrelated) work in the researcher’s laboratory.

I cannot stress too strongly that the above is in no way the fault of the researchers involved. Rather, it is a consequence of the “impact factor tyranny” under which the majority of biologists currently toil. To publish an important paper in a journal with an impact factor of, say, 5 when it might be published later in one with an impact factor of 30 is to deny its lead authors the recognition that having the latter on one’s CV always confers in the eyes of those too busy to study the scientific merits of a job applicant’s past work in detail—and, unfortunately, such people are often the ultimate decision-makers these days. For this reason among many others, there is a desperate need for the scientific community (especially in the biological sciences, where the problem seems most severe) to identify and press for alternative evaluation strategies, which are acceptably efficient but also much more reliable than the impact factors of an academic’s publications. (I’m afraid I don’t have any suggestions in this regard, however.)

Finally I should explain the title of this editorial. I noted above that impact factor tyranny haunts most scientists: whom does it not haunt (or less so)? The answer is: those who work in disciplines so esoteric or low-profile that almost no papers get into top-flight journals, and thus for which impact factor cannot claim to discriminate between competing candidates. Gerontology was, until a decade or so ago, a relative backwater, held in rather low esteem by many biologists, and thus was somewhat immune to the impact factor effect (which was anyway less acute then for everyone than it is now). With the advent of genetically tractable methods for modulating aging in model organisms, and more recently the application of more universally fashionable techniques such as microarray analysis, it has become rather common to see biogerontology papers published in the most competitive journals. It is of course an excellent thing, on balance, that biogerontology has both advanced so far and been generally recognised to have done so—but one major downside to this is that impact factor tyranny has arrived in biogerontology and seems to be here to stay. I urge researchers to recognize that, nevertheless, it must occasionally be rejected in the interests of prompt disclosure of information vital to the progress of our field.

REFERENCES

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