A yeast researcher's perspective on basic research

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Yeast supports our daily lives as a fermentation and brewing agent and is said to be the oldest organism that humans have tamed (without knowing it). Yeast is also the model organism (or at least one of them) that has made the most significant contribution to our understanding of life phenomena at the cellular level in basic biological research. The Nobel Prizes awarded to Hartwell and Nurse et al. in 2001, Schekman et al. in 2013, and Professor Ohsumi in 2016 demonstrate it. I have spent most of my career as a yeast researcher since I chose a yeast lab as a graduate student, fascinated by yeast's great potential as a model organism. Although my research has been oriented to the basics, I have also often been exposed to research aimed at or inspired by the application. And now, I realize that this enlightening environment, where the basic and the applied naturally interact, is unique to yeast.

I have consistently studied the mechanisms of intracellular signal transductions. The results obtained in yeast have been referred to, guided studies using plants, filamentous fungi, and mammals, and provided useful experimental tools. Furthermore, the number of citations from publications and requests for samples is never inferior to those of so-called "exit-oriented" research. Which is good evidence that basic research on yeast is "useful." However, the fact that my above arguments are also, in a sense, stuck by "usefulness" seems to indicate the depth of the issues surrounding basic research these days.

When appealing for support for basic research, we often say that research guided by the researcher's curiosity is essential. As a party working in the field of research on-site, this is absolutely true. But I have always felt that this expression could be misleading. To those with no contact with research, it sounds like "give me research money so that I can continue doing what I like to do as a hobby" or something like that. I'm afraid that's not right. Instead, we should say that nothing significant enough to be considered innovative can, in principle, be foreseen, and yet specialists are doing their utmost to explore the frontier, whose very ceaseless activity is the fundamental nature of basic science. This point is what I would like to be more widely understood. To this end, I believe that basic researchers should take every opportunity to promote this to the outside world. And on top, they should also not waver in their pursuit of genuinely innovative research.

Nevertheless, research requires upfront funding. And therefore, I sincerely admire the activities of the Ohsumi Frontier Science Foundation, which is trying to create a breakthrough

in the cycle in which the allocation of funds is biased toward "exit-oriented research," introducing even more bias in recent years.