Twelve Rules for Science (Yates Lab Version)

Cite This: J. Proteome Res. 2020, 19, 991−992

n a recent walk with my dog, Annie, I was listening to a Malcolm Gladwell podcast about the psychologist Jordan Peterson, who wrote the book 12 Rules for Life: An Antidote to Chaos (https://en.m.wikipedia.org/wiki/12_Rules_for_Life). Gladwell said the success of this book triggered an avalanche of lists enumerating "the 12 rules" for almost everything. This got me thinking: Do I have 12 rules for science? If you ask any former or current member of my group, they will tell you that I have many adages that I share during group meetings in an attempt to motivate, but in a funny, " cliched " way. After listening to Gladwell, I was inspired to organize these aphorisms and offer my " 12 rules for science". I realize many will not agree with my " rules", so I encourage you to share your own.

So, here they are.

1) Publish, publish, publish − This rule is fairly specific for academic science, but since we are in the business of knowledge generation, I believe it is not knowledge if it is not shared, e.g., published. While knowledge can also be shared by speaking at conferences, etc., it does not become part of the permanent scientific record unless it is published. So...write it up.

2) Do the experiment − Great ideas are just ideas until you do the experiment. The secondary part of this rule is "it's better to be first than best", or "the perfect is the enemy of the good". Get your idea, concept, or discovery out there, and refinement can always come later. If you doubt the importance of this rule, just try to think, who was the second person to fly solo across the Atlantic Ocean? He did it faster and used less fuel (he did it better!), but his name is lost to history because he was not first.

3) Write, write, write − Perhaps this is a corollary to rule #1, but writing is a skill that requires regular practice and refinement. I encourage people in my laboratory to always be writing something. It is the mandate of our jobs to share, and sharing occurs through publication.

4) Seize the idea − I remind my lab that if you have a great idea, it is likely that someone else has had it, too. So, you need to seize the day and move to experiments quickly. Intel and Microsoft have thrived on institutional paranoia. The fear that someone else is working on the same thing that could disrupt your business can be motivational, and this works in science, too.

5) Be careful of advice − Occasionally, advice can discourage you from doing an experiment because someone else thinks "it won't work" (see #2). Several times during my career I have been told my ideas will not work. Sometimes we pursued the idea anyway, but sometimes we did not, and a few of these ideas turned out to be important. For example, in 1993, I was told by a corporate scientist/engineer that data-dependent acquisition would not work, and thus we did not pursue it. We now know it works, and it was an important development in mass spectrometry and proteomics.

6) Be tenacious − Tenacity is important given the low funding rates of grants and low percentages of papers accepted at high-impact journals. Even if a study section or reviewer did not like a grant or paper, that does not mean it was not good. It is possible to be too far ahead of the curve and reviewers just do not see the significance, or maybe something was not clear. Consider the feedback, and try again; remember that the reviewers on papers and on study sections change, so a skeptical reviewer who may have voted against your paper or proposal may have been replaced by someone who takes a liking to the grant and becomes your advocate. NIH loves to track grants they funded that led to important discoveries or technologies, but they do not track those proposals that were rejected and eventually led to great discoveries or technologies. The proposal for my most highly cited paper was rejected twice from NIH study sections.

7) Interact with colleagues − I love brainstorming with colleagues, but it is important to respect others' turf. One of the fun things to do at conferences is to get together at dinner with friends and colleagues to talk science. A great question to get the discussion going is, "What problem would you like to solve in the next five years?" But this does not work—and you will lose friends—if you run off and try someone else's idea. An older scientist once told me that he did not pursue an area of science because his friend worked in that area, and he was a "gentleman" and therefore would not compete with his friend. If you are really interested in working on a friend's research project, collaborate, do not appropriate.

8) Understand that experience is never wasted − I am a firm believer that learning something new is never worthless, even if it is not useful at the time. While

Published: February 7, 2020
writing computer code was not central to my projects in graduate school and postdoctoral training, I spent a lot of time programming in my “spare time”, and this skill was key for success when I started my own laboratory. Which leads to rule #9...

9) **Learn to write computer code** — Being able to write computer code is powerful. Even knowing a simple language like Perl can be useful to process data. But having the ability to write complex algorithms is even better as it allows you to develop technology or to test other ideas. If you cannot write code, then it makes you increasingly dependent on people who can as data sets get bigger and more complicated.

10) **Collaborate** — Collaborations are very powerful, especially when they are with people passionate about their research. My research involves the development and use of a platform technology that has also been useful for others, so my laboratory has collaborated widely. Typically, these collaborators are passionate about their research and are experts in their fields, so the collaboration is fruitful and can progress in unexpected directions. Many times, collaborations have helped find weaknesses in our technology/methods and driven us in new directions.

11) **Be brave, be bold** — This is easier said than done, but life is short, and we should try to do meaningful and gratifying work. Go after known unknowns and unknown unknowns. One of my postdocs summed it up nicely on one project by asking, “Do I go after the protein everyone else is studying and get an okay paper for sure, or do I go after the unknown protein and take the risk I don’t get anything or I get lucky with a blockbuster paper?” (FYI, he took the risk and got the blockbuster!)

12) **Never say never** — I have come to appreciate that people are clever and creative, and saying something will never work is not a good idea. I try to refrain from saying something will not work, but I will offer how I would approach the problem. The thing about disruption is you usually do not see it coming.

We at the *Journal of Proteome Research* strongly believe in rule #1. So, write up the manuscript and send it to us.

Signed,

John R. Yates, III, Editor-in-Chief

orcid.org/0000-0001-5267-1672

■ **AUTHOR INFORMATION**

Complete contact information is available at: https://pubs.acs.org/10.1021/acs.jproteome.0c00040

**Notes**

Views expressed in this editorial are those of the author and not necessarily the views of the ACS.