Everyone knows the value of resilience and persistence when trying to publish, but our story is an outlier even in that context. We thought we would share our perhaps quixotic and in hindsight somewhat humorous journey in hope that it encourages others to persevere.

Our paper, “The Effect of Simultaneous Learning and Performance Goals on Performance: An Inductive Exploration” (Masuda, Locke, & Williams, 2014), published in the Journal of Cognitive Psychology, was unique (at least in our view) in two important respects: (a) It was the first study (actually two studies) to look at the effects of specific learning and performance goals when given at the same time; (b) it found a variety of curvilinear relationships never seen before in the goal setting literature.

Over a period of 6 years the paper was rejected by six A journals, a B journal, and a C journal. Getting rejected is now a normal experience for everyone but what is more interesting were the types of reasons given.

1. The most frequent (and virtually universal) criticism was that we submitted the manuscript as inductive rather than deductive. We did this because we had no basis for making deductive predictions about our major question: What would happen if the two types of goals were given together? This seemed to infuriate reviewers. This is ironic because goal setting theory was developed inductively (Locke & Latham, 1990, 2013) and could never have been developed at all if the journals in the 1960s and 1970s had been as theory manic as they are now. We could have made up phony deductions for this paper but chose not to. Our work was inductive, and it would not have been right to present it as deductive (although we were certainly pushed to do so).

2. The first study, originally submitted alone, used self-reported goals because the assigned goal manipulations failed. The first journal we submitted to told us to replicate with successfully assigned goals before they would accept it. We did that; then they rejected it anyway.

3. Despite the fact that this was a goal setting study, many reviewers did not seem to know the goal setting literature. A frequent problem was that many reviewers only knew the goal orientation (GO) literature. This literature is based on traits,
whereas GST is based on states. The two literatures barely overlap, and no relevant predictions could be made from the trait literature. GO can be manipulated as a state (though the goals are still not specific), but no state findings bear on our study. One reviewer claimed GST was developed deductively from the start, the exact opposite of the truth. Others did not understand what had been previously discovered about the best way to measure or assign a goal (i.e., the lowest score you would be satisfied with, which is not the same as saying the goal itself is low). Another claimed that you need incentives, other than built-in achievement motivation, to do lab studies. Others did not know the proximal-distance goal literature, and so on.

4. More bizarre, we were accused of not knowing the GS literature ourselves; this was obviously connected to #3 above.

5. We realize the bind that editors may have felt in selecting reviewers for our manuscript. In the traditional goal setting literature, there are two experts on learning goals, Gary Latham and Gerard Seijts. Latham could not be used as a reviewer because of a long time association with Locke, but Locke had never worked with Seijts and only knew him slightly. All the rejection journals refused to use Seijts despite his name and qualifications being offered. Many of the journals assumed expertise in GO was sufficient.

6. We were told by a number of reviewers that we should have used a different design (that is, done a different study). Each one had their own opinion. But all the suggested designs were incompatible with our core purpose, which was to look at the effects of assigned performance and learning goals together. When breaking new ground, there are scores of legitimate options, including those suggested by reviewers. But at the beginning stage of research on a topic, it is simply arbitrary to claim that only one design should be used. Inductive theory building involves doing a variety of studies and then integrating them into a coherent whole. You need to start somewhere.

One might ask why we kept trying after so many failures. After all, sometimes it is just better to give up, and this is especially true if the work is, in fact, not very good. Perhaps we were just gluttons for punishment, but we were convinced that our studies would move the field forward and encourage new studies; here was high author efficacy and strong goal commitment. Of course, persistence is a mediator of goal effects.

We hope our experience might inspire other researchers, if they are convinced that their work is good. Of course, doing good work is hard. Locke may post on this at a later date.

References