



## When to End It

Dear Labby,

I am a new assistant professor and had great hopes for my initial career but every experiment done by me and my able research assistant these first nine months has been a total bust. This project evolved from my postdoc, with permission from my lab head to take it away. The recent failures of the experiments in my own lab don't seem to be technical as I am at the bench, using the same techniques that were working during my postdoc. We have replicated results I got in my postdoc, but the new experiments we are doing just aren't working. We are getting either results that are noninterpretable or no results at all. I could go into detail but wonder if Labby might be able to

view this in broader perspective.

—Not Advancing

Dear Not Advancing,

When to terminate a line of research is one of the most vexing problems a scientist ever faces. There is always an emotional bond, e.g., "I have worked on this for so long, so the answer can't be far off." Clinging to the project for that reason is understandable but, let's face it, it is irrational. A second factor that often looms is that a student's PhD project may be doomed, and that is far more troubling.

Variables in reagents or other conditions seem unlikely from your account. And you have replicated the foundational results you obtained during your postdoc. So the answer is likely in the experiments themselves, probably not in their technical execution but in their dialectical design. In other words, something is going on in the cell that neither your postdoc work nor your recent experiments have embraced, because you don't know what it is (a valid excuse). This is the whole point of doing science with an open mind.

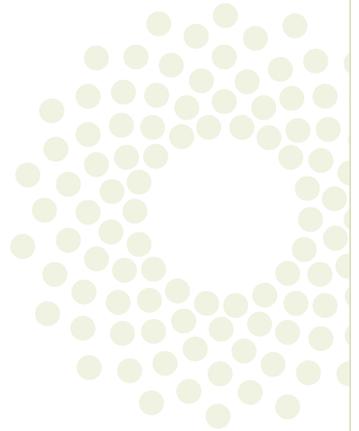
Your first step should be to engage your former postdoc mentor. Be prepared that she/he might adhere to a long-held idea; but, if you are lucky, your mentor may have an open mind. A second step should be to talk to someone in your field known for thinking outside the box. This is just a hunch, but sometimes talking with seasoned scientists at the edges of one's work can be enormously catalytic. The key in your situation is to be sure you haven't failed to look at the problem from every possible angle. If these inquiries don't provide new perspectives, whether at the moment or after you reflect further, then giving up the project might be best. Labby suspects, however, you will not give it up, but will charge ahead with a refined idea. The history of science has few examples of the linear and "no surprises" track you were on, and has very many cases in which a course redirection was the key. Visualize that you are on this latter path. ■

—Labby

Direct your questions to [labby@ascb.org](mailto:labby@ascb.org). Authors of questions chosen for publication may indicate whether or not they wish to be identified. Submissions may be edited for space and style.

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