When to Abandon an Unresolved Project

Successful breakthroughs are always elusive, so don't get too attached to any one prospect.

Academics and corporate innovators both spend their workdays pursuing breakthroughs that may never materialise. Venturing into unknown territory carries fairly high potential rewards, but also a fairly high risk of failure.

When working on a research project, it can be difficult to decide when to cut your losses. Optimism tells us that just beyond our grasp hovers the solution that will make it all work; pessimism, on the other hand, tells us that the end we have in sight may well be a dead end. We are always aware that every day spent chasing a mirage wastes valuable time and resources. Absent a glaring signpost of failure, how does one know when it makes strategic sense to abandon an idea?

Our new paper in *Operations Research* helps answer this question systematically. (See an **earlier version** of this research.) While uncertainty is a given in any speculative project, the good news is that we can still base our decisions on something more solid than guesswork and intuition.

Search and development

Our paper presents a prescriptive analytical model designed to mirror a researcher's real-life situation. There is a risky project being worked on, with no reliable information regarding how close to completion the project actually is. The option to drop the project and resume searching for a more worthwhile endeavour is always available. We model the trade-off between the cost of giving up too early (thereby forfeiting potentially valuable rewards) and the cost of letting more promising opportunities pass by. How should the researcher determine the right time to stop?

Across several trials, we varied the most relevant parameters, including the number of potential rewards (single vs. multiple), the ability to revisit previously abandoned ideas and the risk tolerance of decision makers.

A consistent finding was that one primary cause of uncertainty in these cases—the probability of success in a given project—was not as important as one might think. The optimal stopping time is insensitive to the probability of success, as long as this probability is not too high (e.g. below 50 percent). This is a broad enough range to include most creative endeavours. Certainly, it would be foolishly optimistic to think that exploration in, say, the pharmaceutical industry has a greater than 30 percent chance of producing the next wonder drug.

To obtain a fairly accurate estimate of when you should move on, you need to know only two things: the arrival rate of new projects (i.e. the speed at which your search process generates results) and the arrival rate of success (i.e. how quickly you are

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usually able to bring a project to completion). It boils down to knowing your own speed. As you pick up the pace in your search and exploration activities, you can reduce the time you're willing to devote to an unresolved project.

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