Selecting Issues and Hypotheses for a Research Proposal

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Whenever you write a grant proposal, several questions usually cross your mind: Have I chosen the right topic? Is this a fundable issue? Is this important research? Those questions come up frequently, and they have served as the genesis for the following commentary on the process of identifying and capitalizing on the recognition of a critical research issue.

There is a natural history, a cycle if you will, of ideas and issues that is important to keep in mind when trying to answer these questions. There are seven major forces that I feel shape this cycle, which are the focus of this chapter.

An important part of the process of identifying a critical issue is to conduct an ethnographic study of the funding culture. In that regard, I am going to discuss my perception of NIMH as a culture and of charitable and private foundations as cultures. Then, I will review the tools that you should use to identify these critical issues, as well as some of the mechanisms by which you can maintain this effort on an ongoing basis. This is a constant effort, but it can be interesting, even exciting.

Catching the Wave

Ideas and issues, I believe, have a point of initial introduction, reach a threshold of recognition, and then wane. The degree of interest in a particular issue may be due to the field of study, or it may be a function of the agenda of a funding agency. We will talk about it in both senses.

If you were to plot this wave of interest in an issue, you would find that there is a specific time frame during which you can access this interest. You can mark this time in months, fiscal quarters, fiscal years, even in terms of careers, but the wave is actually continuous. There comes a point, the threshold of recognition, when a critical mass of resources and energy builds behind an issue and captures the popular imagination. The amplitude of the wave may be different from one type of issue to another, but it remains ascended for a period of time, then gradually loses force over time. The same issue may reappear a decade or two later.
The challenge in selecting a critical issue is figuring out how to catch a wave on its ascent. I think a surfing analogy is appropriate. If you try to catch a wave too early, it will not carry you to shore but will go right past you. On the other hand, if you catch the wave too late, it may crash on top of you. Therefore, you want to catch the wave not at its exact peak, but at its greatest momentum. The key, then, is to determine the size of the wave, how quickly it will be moving through time, and when it might end.

There have been a number of waves during my career. With respect to AIDS and mental health-related disorders, we are at the point of greatest momentum. Therefore, your timing with respect to this particular set of issues is good, and thus, potentially fruitful.

Ten years ago, even five years ago, the wave was just beginning to build. At that time, you could have formulated the best ideas in terms of AIDS and its mental health implications, but because it was early in the developmental history of that wave, your ideas would not have experienced the same reception.

However, during the next three to five years, much of the major work in AIDS research already may well be under way. Many of the major efforts will have already caught the wave; it may even begin declining with respect to some of these areas. It is important for you to think about this, not just in terms of your work related to HIV infection and mental health disorders, but as it applies to other interests.

You also should remember that these waves may move through different institutes at different times. Therefore, if you drew the wave of interest in HIV research in various fields, you might not see them as overlapping.

The question, then, is how do I describe and how do I catch a wave? This can be answered by reviewing the seven forces that can make things happen for you at the peak of that wave: (1) need, (2) feasibility, (3) generalizability (not in the scientific sense, but in a programmatic sense), (4) continuity, (5) applicability, (6) scientific merit, and (7) fundability.

Need

The questions that pertain to need are: To whom is this issue important? Who has a stake in it? What is the nature of their stake in the field? What do they perceive as the benefits from the systematic investigation of this particular issue?

Need is mercurial, because it changes depending on the person to whom you are talking. The degree of perceived need is not the same for one constituency as it is for another. The definition of need for the person in the street, who may be infected with HIV or know someone who is, may be quite different than that of a politician, scientist, or staff person at a funding agency. It is important to recognize that the definition or perception of need varies depending upon the person and the setting.

In my experience, one of the most common mistakes that many of us make when we work in this field is in thinking that granting agencies perceive need in the same way as those with whom we work. We act based on our assumptions of need, and we do not take the time and effort to check the concordance of perceptions of need.

When people visiting my program say, "We need to research this problem because we need answers to these questions," I ask, "Well, who else do you think needs this? Are there other people like yourselves, as well as other agencies or organizations, that perceive this need as equally important as you do? In what way do they perceive it as a need?" Answers to each of these questions help us to describe a wave.

Feasibility

Another consideration in catching the wave at its optimal point has to do with feasibility. There may be consensus that an issue is critical, but we must assess feasibility: Are there available methods to pursue