Advice for HFG Applicants

HFG, like every organization that supports research, will expect you to address three questions in an application for funding:

- What do you intend to find out?
- Why is it worth finding out?
- How will you go about finding it out?

What?

What question or questions do you want to answer? These questions may be phrased in the form of formal hypotheses, with statements of alternative potential findings, as is common in the natural sciences and some social sciences. We do not require this, nor do we require you to pose your research goals as questions. But we do expect your proposal to convey clearly what it is you hope to learn. There is an exception to this expectation, though: we consider proposals from applicants who believe they have already learned what they set out to learn in previous research and are seeking support to write up their findings for publication. Many applicants for our Emerging Scholars award will be in this situation, as the award is for graduate students who have completed their doctoral research and would like support during the dissertation-writing year. (Students who will complete both their research and their dissertation during the award year are also eligible to apply.)

If you do pose research questions, it is not necessary that you appear to be completely noncommittal about the answers. In fact, many proposals are extended arguments for the applicant's answers to their questions. If you are presenting such an argument in your proposal, it is advantageous to include enough evidence for your account that the proposal has "texture"; specific findings from your own research or from the relevant research literature will make a proposal more compelling to our evaluators.

In short, if you have already completed all or part of your research, give us an idea of what you've *found out*. In the case of applications for studies that have yet to take place, it is fine to take a position regarding the answer you expect to find or which hypotheses you expect to still be standing at the completion of your analysis. However, in these cases, we look for an indication that the applicant is open to changing their mind, to modifying or abandoning their initial beliefs—in other words, to proving themself wrong.

(We believe that, like scientists, historians too should embrace some version of this openness to alternative explanations in commencing a research project.) In a proposal to evaluate a policy or program intended to reduce violence, we expect the application to indicate that, no matter how widely endorsed the policy or program has been, the investigator is as prepared to find no effect as they are to find it effective. Negative findings, too, are valuable.

Why?

Your argument for the importance of your research project—that is, why it should be funded—will vary by the granting agency you apply to, of course. HFG is not a general funder of research but rather has a specific remit: to support research on the causes and nature of violence so that it might be prevented or at least reduced.

Relevance to Violence

Accordingly, the first standard we employ in evaluating an application is the relevance of the proposed research to violence. As many as a third of the proposals we receive in each round of applications are eliminated from further consideration because they don't meet this relevance criterion.

By violence, we mean physical violence—behavior intended to inflict physical harm on people. We do not fund research on projects that are about metaphorical extensions of the concept of violence, such as "symbolic violence" or "structural violence." The problems that the notion of structural violence refers to, such as discrimination and poverty, are serious ones and unquestionably deserving of research. However, our resources are devoted to the problem of physical violence both because this was the evil that Harry Frank Guggenheim was most concerned about and because we feel that focusing on this problem rather than a wider array of ills is the most effective use of the Foundation's resources.

Originality

We do *not* require an application to propose research on a problem that has not been studied before; in the area of violence, as in other domains of human behavior, there are few entirely new topics under the sun. A proposal that makes a case for a new *approach* to investigating a well-studied problem is appropriate for our consideration, especially if the applicant includes a compelling argument that previous work on the topic fell short in a way that their own study will avoid. An adequate proposal will include a review of the scholarly literature that is relevant to the research problem to demonstrate that the applicant is equipped with the theoretical and factual knowledge required to tackle the problem. This would be the place to relate any shortcomings you believe limited the insights of prior work in your area. Your discussion of the pertinent literature may either take the form of its own section of the proposal or occur throughout. (It is best to avoid the invocation of the most-discussed social theorists of the day if the purpose is mainly to demonstrate your erudition rather than to help your evaluators understand how your study connects to previous work on your topic.)

One justification for funding we often read in applications is that "surprisingly little research" has been conducted on the topic proposed. If you are confident this is true, then don't hesitate to include it as part of the case for your topic. Bear in mind, though, that the evaluators at a foundation dedicated to research on violence are probably going to know if there is, to the contrary, a significant amount of research on the applicant's topic. In addition, it may be true that little research has been conducted on the topic proposed but that this dearth is *not* surprising. We receive many proposals in which the basic topic has already been the subject of voluminous study—as in the case of, say, conflict between pastoralists and herders, children's exposure to community violence, or intimate-partner violence—but not in the place or group the applicant wants to investigate. That lacuna alone doesn't make a project worthy of funding. Unless you can make a convincing case that studying this well-researched problem in yet another population promises to shed light on the problem *in general*, your proposal might well be assessed as the scholar Moses Hadas is said to have concluded about a work he was reviewing: "This book fills a much-needed gap."

Scale and Utility

Alongside relevance to violence and originality, we accord some weight to the scale of the violence problem to be investigated. If two excellent proposals are comparable on every other measure but one is about a problem of violence that entails many victims and the other relatively few, we will generally favor the former.

We will give serious consideration to research proposals in the social sciences, history, law, and biology that promise to illuminate the causes of violence. In this commitment to elucidating the mechanisms of violence—economic, ideological, psychological, biological—we are akin to biomedical funding agencies such as the National Institutes of Health and the National Science Foundation, which support both research with a clear potential to remedy an ill and investigations intended to uncover fundamental biological mechanisms without such a known applicability. However, given two proposals on the same problem that are both solid in research design, we will favor one if its research plan includes a cogent discussion of the potential implications of the applicant's findings for improving existing amelioration policies or crafting new ones.

Similarly, history proposals will be assessed with an eye toward contemporary relevance. For example, 19th-century colonial practices that economically favored one ethnic group over another, accorded one group political dominance, or construed cultural differences as reflecting natural differences in capabilities could well have engendered group animosities and intermittent violence persisting into the present day. Projects exploring such practices always have been and will continue to be seen as appropriate for our consideration.

On the other hand, proposals about the evolution of warfare during the Neolithic era, slave trading in the Middle Ages, or even the logistics of the Nazi death camps—though all unambiguously about violence—should contain a compelling argument for their promise to elucidate current situations of violence.

In short, the foundation is interested in where violence comes from and what works to reduce it. We prioritize studies of the causes and dynamics of violence over research on the *effects* of violence. We are interested in the effects of violence primarily to the extent that these outcomes might themselves conceivably serve, in turn, as *causes* of future violence. Thus we would certainly consider a study investigating whether victims of child abuse are later at elevated risk of perpetrating their own violence. We would *not*, on the other hand, give equal consideration to a study looking for elevated rates of depression in such victims. There are two reasons for this weighting. One is that the deleterious effects—psychological, physical, financial, social—of this and of other forms of violence are already well known: violence is bad news. Second, if such a study determined that rates of depression were *not*, in fact, higher in victims than in nonvictims, would the proper lesson be that we should be less concerned about child abuse? (No.) Similarly, a project looking at the consequences of government repression for, say, subsequent voting patterns or political activism of targeted groups would be seriously considered only insofar as a plausible case were made that these effects had implications for future violence.

How?

This question covers the issue of research design—your plan for getting the answers to your research questions. The foundation is broad-minded about methods, open to both qualitative and quantitative approaches. And, as mentioned above, our conception of research includes the analysis of data already in hand or preparation of written accounts of findings. We support archival work, ethnography, biological and psychological laboratory studies, program evaluations, statistical analyses, and, occasionally, purely theoretical projects. We fund time off from teaching, supplements to sabbatical salaries, laboratory and fieldwork costs, and research assistance.

Our evaluators will want to see that the methods you're planning to use are appropriate for your research questions. For example, if you want to find out what attitudes and beliefs fuel group conflict, it makes sense to conduct interviews, surveys, or focus groups. If you are interested in the role of conflict entrepreneurs in violence, you should talk to them, read their writing, or listen to their broadcasts. And, of course, you'll want to include a way to determine the *influence* of what they say on those who read or hear them, as the extent of that influence cannot simply be assumed. If you intend to elicit the perspectives of people directly involved in a form of violence, it's important to indicate your awareness of the limitations of this approach. If finding the causes of violence in the Niger Delta or Israel/Palestine required only collecting the views of local residents, there would be no need for social scientists, as journalists regularly carry out such work very capably. It should be an article of faith of the social scientist that people don't necessarily understand the social phenomena of which they're a part or the forces that influence their behavior and beliefs. They may have valuable insights, but they can also be quite wrong. This is where you, the analyst, come in.

If your research is going to consist mainly of conducting interviews or focus groups, it is best to avoid calling this "ethnography," a terminological mistake that has become common in recent years. These are valuable methods, but just because you're talking to people doesn't mean you're doing ethnography, which would involve living with or at least spending long hours with the group you're studying, for months or years, in order to learn their lifeways. If the evaluator of your proposal is an anthropologist or sociologist, they might well take umbrage at this mischaracterization of what you're planning to do.

Our reviewers will want to know that you are certain to have access to the location, people, data sets, archives, or other things necessary for the research you're planning.

In the case of fieldwork with an ethnographic component, will the time in the field site(s) be substantial enough to acquire real familiarity with a place—say, several months in one or two places as opposed to two weeks in each of six?

All scientific and historical research entails claims about what causes or caused what. Scholars within these disciplines should thus be familiar with the essential logic of causal reasoning. If even simple quantitative comparisons between groups are to be made, are the samples large enough to reveal differences between the groups that are unlikely to be due to chance? Are the people you'll be interviewing or surveying truly representative of the group you think they represent? Have you given thought to problems of selection bias, confounding variables, and other sources of error? These considerations may seem too elementary to mention here, but it is not rare for a proposal to founder by failing to indicate the applicant's awareness of these potential threats to valid causal conclusions.

If you will be employing statistical methods or computational techniques that are unlikely to be familiar to a scholar outside of your discipline—or even to some of those within it—it is a good idea not just to mention those methods but to give at least a one-sentence explanation of what they entail and why they'll be useful in your analysis; the augmented synthetic-control method and support vector machines do not explain themselves. Do *not* devote pages to a tutorial on each of your methods, but do strive to convey to the evaluators the merits of your choices.

Other Advice

Proposal length

Please pay attention to the application guidelines provided on our website, www.hfg.org, for each of our award programs, especially about proposal length. The main part of HFG proposals, the research plan, averages about fifteen double-spaced pages for our Emerging Scholars and Distinguished Scholars awards, and between ten and fifteen for our African Fellows award. Anything substantially shorter will look thin to our reviewers, and anything substantially longer will not endear you to them, as they must read many proposals each application round. Also, a proposal that is much longer suggests either that the applicant has submitted to us a proposal prepared for another funding agency—one that doesn't have our length guidelines—or that the applicant's writing suffers from prolixity. Or perhaps both. (And please do not use a font smaller than twelve points.)

Proposal language

Our panel of reviewers is diverse in disciplines. Your proposal will first be read by the member whose expertise is most appropriate for your topic. If that reviewer sees significant merit in the proposal, it will then be read by all members of the panel. It is thus advantageous if, even if your proposal contains a subtle theoretical discussion or technical methods, your writing is clear enough that what you're up to can be readily grasped by a scholar outside of your field. You should thus avoid technical jargon as well as the windy verbosity that blights much social-science writing. When a reader understands your proposal, they feel smart; that can only help your chances of success.