APPENDIX B
TWO ESSAYS ON WRITING FELLOWSHIP PROPOSALS: SSRC, NEH

On the Art of Writing Proposals

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Some Candid Suggestions for Applicants to Social Science Research Council Competitions

Writing proposals for research funding is a peculiar facet of North American academic culture, and as with all things cultural, its attributes rise only partly into public consciousness. A proposal’s overt function is to persuade a committee of scholars that the project shines with the three kinds of merit all disciplines value, namely, conceptual innovation, methodological rigor, and rich, substantive content. But to make these points stick, a proposal writer needs a feel for the unspoken customs, norms and needs that govern the selection process itself. These are not really as arcane or ritualistic as one might suspect. For the most part, these customs arise from the committee’s efforts to deal in good faith with its own problems: incomprehension among disciplines, work overload, and the problem of equitably judging proposals that reflect unlike social and academic circumstances.

Writing for committee competition is an art quite different from research work itself. After long deliberation, a committee usually has to choose among proposals that all possess the three virtues mentioned above. Other things being equal, the proposal that is awarded funding is the one that gets its merits across more forcefully because it addresses these unspoken needs and norms as well as the overt rules. The purpose of these pages is to give competitors for Council fellowships and funding a more even start by making explicit some of those normally unspoken customs and needs.

Capture the Reviewer’s Attention

While the form and the organization of a proposal are matters of taste, you should choose your form bearing in mind that every proposal reader constantly scans for clear answers to three questions:

What are we going to learn as the result of the proposed project that we do not know now?

Why is it worth knowing?

How will we know that the conclusions are valid?

Working through a tall stack of proposals on voluntarily-donated time, a committee member rarely has time to comb proposals for hidden answers. So, say what you have to say immediately, crisply, and forcefully. The opening paragraph, or the first page at most, is your opportunity to grab the reviewer’s attention. Use it. This is the moment to overstate, rather than understate, your point or question. You can add the conditions and caveats later.

Questions that are clearly posed are an excellent way to begin a proposal. Are strong party systems conducive to democratic stability? Was the decline of population growth in Brazil the result of government policies? These should not be rhetorical questions; they have effect precisely because the answer is far from obvious. Stating your central point, hypothesis, or interpretation is also a good way to begin: Workers do not organize unions; unions organize workers. The success and failure, of Corazon
Aquino’s revolution stems from its middle class origins. Population growth coupled with loss of arable land poses a threat to North African food security in the 1990s.

Obviously some projects are too complex and some conceptualizations too subtle for such telegraphic messages to capture. Sometimes only step-by-step argumentation can define the central problem. But even if you adopt this strategy, do not fail to leave the reviewer with something to remember: some message that will remain after reading many other proposals and discussing them for hours and hours. “She’s the one who claims Argentina never had a liberal democratic tradition” is how you want to be referred to during the committee’s discussion, not “Oh yes, she’s the one from Chicago.”

Aim for Clarity

Remember that most proposals are reviewed by multidisciplinary committees. A reviewer studying a proposal from another field expects the proposer to meet her halfway. After all, the reader probably accepted the committee appointment because of the excitement of surveying other people’s ideas. Her only reward is the chance that proposals will provide a lucidly-guided tour of various disciplines’ research frontiers. Don’t cheat the reviewer of this by inflicting a tiresome trek through the duller idiosyncrasies of your discipline. Many disciplines have parochial traditions of writing in pretentious jargon. You should avoid jargon as much as you can, and when technical language is needed, restrict yourself to those new words and technical terms that truly lack equivalents in common language. Also, keep the spotlight on ideas. An archaeologist should argue the concepts latent in the ceramic typology more than the typology itself, a historian the tendency latent in the mass of events, and so forth. When additional technical material is needed, or when the argument refers to complex ancillary material, putting it into appendices decongests the main text.

Establish the Context

Your proposal should tell the committee not only what will be learned as a result of your project, but what will be learned that somebody else does not already know. It is essential that the proposal summarizes the current state of knowledge and provides an up-to-date, comprehensive bibliography. Both should be precise and succinct. They need not constitute a review of “the literature” but a sharply focused view of the specific body or bodies of knowledge to which you will add. Committees often treat bibliographies as a sign of seriousness on the part of the applicant, and some members will put considerable effort into evaluating them. A good bibliography testifies that the author did enough preparatory work to make sure the project will complement and not duplicate other people’s efforts. Many proposals fail because the references are incomplete or outdated. Missing even a single reference can be very costly if it shows failure to connect with research directly related to one’s own. Proposal writers with limited library resources are urged to correspond with colleagues and libraries elsewhere in the early stages of research planning. Resource guides such as Dissertation Abstracts International and Social Science Periodical Index are highly recommended.

For any disciplines, Annual Reviews, (e.g. Annual Review of Anthropology) offer state of the art discussions and rich bibliographies. Some disciplines have bibliographically-oriented journals, for example Review of Economic Literature and Contemporary Sociology. There are also valuable area studies-oriented guides: Handbook of Latin American Studies, International African Bibliography, etc. Familiarizing yourself with them can save days of research.

What’s the Payoff?

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Disciplinary norms and personal tastes in justifying research activities differ greatly. Some scholars are swayed by the statement “it has not been studied” (e.g. a historian may argue that no book has been written about a particular event, and therefore one is needed), while other scholars sometimes reflect that there may be a good reason why not. Nevertheless, the fact that less is known about one’s own chosen case, period, or country than about similar ones may work in the proposer’s favor. Between two identical projects, save that one concerns Egypt and the other the Sudan, reviewers are likely to prefer the latter. Citing the importance of the events that provide the subject matter is another and perhaps less dubious appeal. “Turning points,” “crucial breakthroughs,” “central personages,” “fundamental institutions,” and similar appeals to the significance of the object of research are sometimes effective if argued rather than merely asserted. Appealing to current importance may also work: e.g. democratic consolidation in South America, the aging population in industrialized countries, the relative decline of the hegemony of the United States. It’s crucial to convince readers that such topics are not merely timely, but that their current urgency provides a window into some more abiding problem.

Among many social scientists, explicit theoretical interest counts heavily as a point of merit. Theoretical exposition need not go back to the axiomatic bases of the discipline—proposal readers will have a reasonable interdisciplinary breadth—but it should situate the local problem in terms of its relevance to live, sometimes controversial, theoretical currents. Help your reader understand where the problem intersects the main theoretical debates in your field and show how this inquiry puts established ideas to the test or offers new ones. Good proposals demonstrate awareness of alternative viewpoints and argue the author’s position in such a way as to address the field broadly, rather than developing a single sectarian tendency indifferent to alternatives.

Use a Fresh Approach

Surprises, puzzles, and apparent contradictions can powerfully persuade the reviewer whose disciplinary superego enforces a commitment to systematic model building or formal theorizing: “Given its long-standing democratic traditions, Chile was expected to return to democracy before other countries in the Southern Cone and yet.... It is because the assumption on which this prediction was based is false?” “Everybody expected that the ‘One Big Union’—the slogan of the movement—would strike and win wage increases for workers. Yet statistical evidence shows just the contrary: strong unions do not strike but instead restrain workers’ wage demands.

It is often worthwhile to help readers understand how the research task grows from the intellectual history or current intellectual life of the country or region that generated it. Council committees strive to build linkages among an immense diversity of national and international intellectual traditions, and members come from the various countries and schools of thought. Many committee members are interested in the interplay of diverse traditions. In fact, the chance to see intellectual history in the making is another reason people accept committee membership. It is a motive to which proposals can legitimately appeal.

It pays to remember that topics of current salience, both theoretical and in the so-called real world, are likely to be a crowded field. The competitors will be more numerous and competition less interesting than in truly unfamiliar terrain. Unless you have something truly original to say about them, you may be well advised to avoid topics typically styled “of central interest to the discipline.” Usually these are the topics about which everyone is writing, and the reason is that somebody else has already made the decisive and exciting contribution. By the time you write your proposal, obtain funding, and write it up, you may wish you were working on something else. Or if your instinct leads you to a problem far form the course that the pack is running, follow it—not the pack: nothing is more valuable than a really fresh beginning.

Describe Your Methodology

Methodological canons are largely discipline-specific and vary widely even within some disciplines. But
two things can safely be said about methodological appeal. First, the proposal must specify the research operations you will undertake and the way you will interpret the results of these operations in terms of your central problem. Do not just tell what you mean to achieve, tell how you will spend your time while doing it. Second, a methodology is not just a list of research tasks but an argument as to why these tasks add up to the best attack on the problem. An agenda by itself will normally not suffice because the mere listing of tasks to perform does not prove that they add up to the best feasible approach.

Some popularly used phrases fall short of identifying recognizable research operations. For example, “I will look at the relation between x and y” is not informative. We know what is meant when an ornithologist proposes to “look at” a bird, but “looking at” a relation between variables is something one only does indirectly, by operations like digging through dusty archive boxes, interviewing, observing and taking standardized notes, collecting and testing statistical patterns, etc. How will you tease the relationship of underlying forces from the mess of experience? The process of gathering data and moving from data to interpretation tends to follow disciplinary customs, more standard in some fields than in others; help readers from other fields recognize what parts of your methodology are standard, what innovative.

Be as specific as you possibly can be about the activities you plan to undertake to collect information, about the techniques you will use to analyze it, and about the tests of validity to which you commit yourself. Most proposals fail because they leave reviewers wondering what the applicant will actually do. Tell them! Specify the archives, the sources, the respondents, and the proposed techniques of analysis.

A research design proposing comparison between cases often has a special appeal. In a certain sense all research is comparative because it must use, implicitly or explicitly, some point of reference. Making the comparison explicit raises its value as a scientific inquiry. In evaluating a comparative proposal, readers ask whether the cases are chosen in such a way that their similarities and differences illuminate the central question. And is the proposer in a position to execute both legs of a comparison? When both answers are positive, the proposal may fare particularly well.

The proposal should prove that the researcher either possesses or cooperates with people who possess, mastery of all the technical matters the project entails. For example, if a predominantly literary project includes an inquiry into the influence of the Tulpan language on rural Brazilian Portuguese, the proposal will be checked for the author’s background in linguistics and/or Indian languages, or the author’s arrangements to collaborate with appropriate experts.

**Specify your Objectives**

A well-composed proposal, like a sonata, usually ends by alluding to the original theme. How will research procedures and their products finally connect with the central question? How will you know if your idea was wrong or right? In some disciplines this imperative traditionally means holding to the strict canon of the falsifiable hypothesis. While respecting this canon, committee members are also open to less formal approaches. What matters is to convince readers that something is genuinely at stake in the inquiry—that it is not tendentiously moving toward a preconceived end—and that this leaven of the unknown will yield interesting orderly propositions.

Proposals should normally describe the final product of the project: an article, book, chapter, dissertation, etc. If you have specific plans, it often helps to spell them out, because specifying the kind of journal in which you hope to publish, or the kind of people you hope to address, will help readers understand what might otherwise look like merely odd features of the proposal.

While planning and drafting your proposal, you should keep in mind the program guidelines and
application procedures outlined in the brochure specific to the program to which you are applying. If you have specific questions about the program, you may wish to consult with a staff member. Your final proposal should include all requested enclosures and appendices.

**Final Note**

To write a good proposal takes a long time. Start early. Begin thinking about your topic well in advance and make it a habit to collect references while you work on other tasks. Write a first draft at least three months in advance, revise it, and show it to colleagues. Let it gather a little dust, collect colleagues’ comments, revise it again. If you have a chance, share it with a seminar or similar group; the debate should help you anticipate what reviewers will eventually think. Revise the text again for substance. Go over the language, style, and form. Re-sharpen your opening paragraph or first page so that it drives home exactly what you mean as effectively as possible.

Good luck!
The Art of the Fellowship Proposal

John Lippincott

With appreciation to the editor of Humanities, Judith Chayes Neiman, for her kind permission to let us recopy this. Mr. Lippincott wrote this as a member of the endowment staff.

Each year the NEH receives thousands of individual fellowship applications from good scholars for good projects. A few hundred are recommended by review panels for funding, but only a few score elicit a unanimous recommendation of “Absolutely Yes!” As budgetary constraints on the endowment increase, the importance of a strong panel endorsement to the success of an application also increases.

Writing a fellowship proposal that receives enthusiastic endorsement from panelists is both an art and a science. The science is in carefully following the guidelines for the format of the application and in presenting a proposal that clearly reflects knowledge of the subject being studied and the methodology appropriate to it. The art is more difficult to describe and is the subject of this article.

The art of writing a successful proposal is not a matter of knowing arcane secrets of grantsmanship, a presumed hidden agenda at NEH, or that influential someone in the Fellowships Division. Nor is it achieved by mimicking proposals that received NEH grants in the past. (Examples given in this article are intended to demonstrate levels of quality, not to serve as models.)

The art of writing a successful proposal is largely a matter of understanding how individual fellowship applications are selected for funding.

There are three fellowship programs that award grants for individual study and research in the humanities: Summer Stipends; Fellowships for College Teachers; and Fellowships for Independent Study and Research. They are all highly competitive because of their limited budgets and the large number of good proposals submitted each year. The ratio of grants to applications varies among the programs and from year to year, ranging from a low of one-to-five in the College Teachers program to a high of one-to-nine in Independent Study.

All three programs use ad hoc review panels—composed of scholars representing the disciplines of the applications under consideration—to evaluate the proposals. Panel ratings serve as the basis for the National Council on the Humanities’ funding recommendations to the NEH chairman, who gives final approval on all endowment grants.

In making their assessments of an application, panelists consider the evidence provided by the applicant—the description of the project, the letters of reference, the curriculum vitae, and the bibliography of works relevant to the study. (Directions for proper completion of application materials cannot be recapitulated here; they are given in the guidelines for each program and should be followed carefully.)

In evaluating this evidence the panelists adhere to the four selection criteria stated in the program guidelines. A review and discussion of these criteria (which vary only slightly among the three individual fellowships programs) will help reveal what makes for an “artful,” i.e., competitive, application.

1. The quality or promise of quality of the applicant’s work as a teacher, scholar, or interpreter of the humanities.

This criterion focuses more on the applicant than on the project. The panel looks for evidence that the individual has the knowledge and ability to carry out the project and a commitment to excellence in
scholarship. In making this determination, the panel considers more than just the curriculum vitae and record of previous publications. Reference letters provide critical information as well, and the project description itself, in its conception and presentation, is an important indicator of the quality of the individual’s thought.

The phase “the promise of quality” in this criterion indicates that panelists are concerned not simply with past accomplishments of the applicant. All three programs make grants to scholars early in their careers, as well as to senior scholars. Panelists try to judge the quality of applicants’ work by standards appropriate to their career stages. There are no quotas set for awards to junior or senior scholars, nor is there any prejudice against either group. Among the Independent fellowships awarded last November, forty-eight percent went to junior scholars. (Forty-nine percent of the applications were from junior scholars.)

One of these junior scholars is studying the origin of the economic decline in New England from 1840 to 1925. The applicant was awarded a doctorate in history in 1979 and is currently an assistant professor at a major university. Her record of publications includes two journal articles and three conference presentations.

In evaluating her application, panelists took note of her status as a younger scholar. The sophisticated knowledge of the subject revealed in the proposal itself and strong letters of reference were instrumental in convincing the panel that there was “promise of quality” from this applicant. “Extremely impressive proposal,” commented one panelist. “Well-reasoned, clear and attractive.”

When panelists evaluate the “quality of work” of senior scholars, they may place greater emphasis on some aspects of the application.

One of the 1982-83 Independent Study awards to an established scholar (doctorate awarded in 1968, college professor since 1966, currently an associate dean at a major university) was for a biography of Anne Sexton. Certainly the proposal description was a principal element in panelists’ consideration of the quality of work of the applicant, as were the letters of reference. But panelists also took careful note of the applicant’s record of achievement—nine academic honors; three books and numerous articles of high quality; and poems published in a variety of journals.

Without this level of accomplishment it is unlikely a panelist would have concluded, “Seldom have I found an applicant I could bet on with more certainty—an absolutely first-rate proposal and person to do it.” Another remarked, “Publications are quite good, references are excellent, and the candidate obviously has access and can do the biography.”

It should be noted that the “work” whose quality is being judged under this criterion need not have been conducted in an academic setting. Two of the three programs entertain applications from scholars unaffiliated with colleges or universities; they also include unaffiliated scholars on their panels.

2. The importance of the proposal to the specific field and the humanities in general.

The best evidence of the importance of the project is given in the applicant’s project description, though certainly letters of reference provide necessary corroboration. An applicant cannot assume that panelists will appreciate the importance of a project or have a predisposition toward the subject matter. It is incumbent upon the applicant to make the case for the importance of the study to be undertaken.

Because applications are competitive and reviewed in groups, panelists look for those projects likely to make the greatest contribution to the humanities. The contribution an applicant expects to make may be
through teaching, through the production of materials that will serve other scholars, or through development of new perspectives on the discipline that will encourage further discussion and understanding of the subject among all interested audiences.

A project that will serve only the applicant (such as remedial work by the applicant to “catch up” in a field) will not be competitive with projects that offer to add to the knowledge of students, colleagues, or a wider public.

A summer stipend was recently awarded for a project to write an archaeological commentary on the *Wasps* of Aristophanes, applying vase paintings and other monumental evidence to a study of the play’s terms, puns, metaphors, objects, actions and the *mise-en-scene* of the Athenian law courts.

In his proposal, the applicant argued the importance of the project by citing other scholars who have affirmed the value of applying archaeological evidence to interpretation of Aristophanes’ comedies. He then offered his own view of the significance of providing a “material and historical context” for understanding literature in general and the *Wasps* in particular. He suggested the study would serve classicists as well as a wider group of readers and would provide a basis for more authentic and effective productions of the play.

He persuaded the panel that a new understanding and appreciation of Aristophanes was needed and could be achieved through this project. One panelist commented, “This kind of study is something we should see more of and that is an approach to a classical text which attempts to conceptualize a drama as it was originally conceived and produced as, among other benefits, a stimulus to the production of ancient comedy.” Another noted that “it is the sort of work that combines ‘scholarly’ and ‘practical’ use; it may well help directors and actors present more visually meaningful performances of the play.”

In addition to the importance of the subject matter, the proposal may argue for the value of its methodology, as in this excerpt from a 1982-83 Independent Study proposal:

> Political history is currently out of fashion, largely because it tends to be biographical and narrative in orientation and, except for vote counting, does not lend itself to social-scientific techniques and analysis. Political history, however, deserves attention, partly because it contains the central questions of history—how are decisions actually made—and partly because political, old-fashioned elitist history needs redoing. I propose to take a fresh look at the political history of Tudor England and study the political environment in which individuals translated their culturally conditioned aspirations and assumptions into the realities of political success and failure. It is customary to approach politics from the perspective of those who succeeded because the documentation is skewed in that direction and successful ideas live on in terms of their historic consequences. Unfortunately, successful people also tend to be well-adjusted and to know how to make the system work for them; as a result, they do not usually have much to say about the functional and psychological strains under which they operate. It is the unsuccessful who flounder and cry out and thereby reveal in their lives and writings the pressures and emotional strains under which all the natural leaders of society must work. As Scott Fitzgerald said: ‘It is from the failures of life and not its successes that we learn the most.’ The ultimate tour de force is to relate theory to practice and to offer an explanation of Tudor politics in terms of a multitude of failure stories, thereby rewriting and reinterpreting the sixteenth-century political scene... Irrationality in politics, political failure and paranoia are, alas, sufficiently relevant themes to need no special pleading. That they are being studied within a sixteenth-century context should not distract from their importance to the scholar, from their interest for the general reading public, or from their impact upon our knowledge about mankind.
Panelists were convinced. “It appears that the realization of this project would shed new light on the political dynamic of a crucial period... I think his approach will serve as an important scholarly model in terms of developing understanding of the political process in any era.” “The book would likely reach not only specialists but intelligent readers generally and make a significant and original contribution to both. This is one among two or three proposals that I rank as the very best -the reflection of a mature and brilliant scholar on a field in which he has long worked, that is at the same time an act of imagination—an asking of fresh questions of material long familiar that will influence all our thinking.”

Importance of the project is not a function of the discipline or scope of the project. There are no favored fields, time periods, or cultures. It is rather what the applicant makes of the subject that determines its importance—a point to be taken up under the third criterion.

3. The conception, definition, and organization of the proposal.

This and the preceding criterion are mutually supportive. The importance of a project is dependent on the way it is conceived, and its conception cannot be judged without regard for its importance.

Good conception, definition, and organization of the project obviously result from the applicant’s command of the subject and thus fall within the realm of the science of proposal writing. There is, however, also an art to conceiving, defining, and organizing the project. Put simply, the most successful applications seem to be those in which applicants let their ideas and enthusiasm for the subject “shine through.”

A potential applicant once contacted an NEH program officer and said she had two projects for a summer stipend in mind. After describing the projects, she asked the staff member which she should submit. The program officer counseled her to submit the one that interested her most.

Conception of the project involves asking the right questions about the subject to be studied, drawing the right comparisons with other works and subjects, and setting the right scope for the project. The operative term here is “right.” The right questions, right comparisons, and right scope—in addition to being appropriate to the field—are those that capture the interest of the panel. And since a panel is made up of scholars in the discipline, their interests will be similar to those of an applicant’s colleagues.

Competitive proposals are those that go beyond a naive or redundant treatment to explore the subject’s real potential, to yield new perspectives (including interdisciplinary views), or break new ground.

Among the applications for 1981-82 Fellowships for College Teachers were two projects treating ethical issues related to science. Both studies were intended to improve classroom instruction and serve as the basis for new courses. Of these two projects in essentially the same discipline and with the same purpose, only one was funded. The quality of the conception and definition of the project made the difference.

The successful proposal focused the study on ethical issues relating to medicine and explained clearly the value of the project to the institution and students it would benefit. It then discussed the nature of and reasons for recent moral problems associated with medicine and appropriate ways for approaching these problems. The proposal concluded with the specific questions to be explored and the methodology that would be applied.

The project received a strong recommendation from panelists. Typical of their comments was, “This is an excellent proposal both in terms of care with which it is worked out and the probable significance for teaching.”

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The unsuccessful application proposed a two-part study on 1) “the history of the biological sciences and of philosophical issues peculiar to them” and 2) “contemporary work in the area of ethical issues in science and technology.” The proposal discussed the applicant’s teaching responsibilities, academic background, and current approach to and problems with teaching ethical perspectives on science, and offered as a plan of study only a brief paragraph noting resources and faculties to be consulted.

Panelists expressed concern at the application’s lack of a clear focus for the study, or specific issues to be tackled of the approach to be taken. “In comparison with the other proposal which takes biology as background for considering ethical issues, this one is not as well developed,” one panelist remarked.

Another called the proposal “too broad, too vague.” A third said, “not clear that this really takes her enough beyond what she already does and knows to constitute a ‘project.’”

As these examples illustrate, it is important that applicants state clearly what they intend to do, what questions they intend to ask and why. It cannot be left to the panel to infer or the references to imply what the plan of study will be. Panels must know how the grant period is going to be used.

4. The likelihood that the applicant will see the project through to completion.

This criterion simply means that panelists will consider whether or not what is proposed can be and is likely to be achieved.

The criterion does not mean that the entire project must be completed during the grant period, only that it should eventually be completed and that the portion slated for the period of the fellowship can be handled in that time.

A 1981-82 College Teachers fellowship was awarded for a study of gambling in eighteenth- and nineteenth-century England, focusing on how this leisure activity reflects changes in social and private values resulting from industrialization.

Following an intensive discussion of the significance and approach of his study, the applicant stated:

As ambitious as the project is, I believe that it is not an unrealistic one, and my previous work suggests that I can undertake it successfully. I am already familiar with much of the literature, both primary and secondary, on “sporting” topics, and I have had some success in using this material in a constructive way.

In addition to favorable reactions to the applicant’s abilities and the potential value of the study, panelists were convinced of the likelihood the research and a monograph would be completed. “Proposer offers convincing argument and has evidently pursued work to point where it can be completed,” said one. Another said, “[He] has background to indicate likelihood of completion.”

Finally, there are a few additional factors a panel may consider in making decisions on a group of applications. Geographical and institutional diversity are sought among fellowship awards, though no quotas are set. Panelists often take this into consideration as a tiebreaker among highly rated proposals.

The individual fellowship programs give preference to applicants who have not had major grants or postdoctoral fellowships in the last six years. Panels are also sympathetic to able applicants in situations or institutions that offer few research opportunities.

There is also a je ne sais quoi a “sparkle,” an appeal that distinguishes successful proposals from the
nearly successful proposals. This special quality is synergistic, combining and transcending all the previously mentioned qualities, as the following excerpt from a highly rated summer stipend proposal demonstrates:

An extensive study of Russian twentieth-century literature for children is long needed. It would provide us with an observation point from which the very formation of the ‘Soviet mind’ could be observed, because children’s literature in the USSR reflects that process in its complexity; from ideological indoctrination by the state to inoculation with critical attitudes and ways of independent thinking by dissenting writers. For this author the study of Russian children’s literature is a lifelong commitment. I was born and raised in a family of children’s writers: my father was the author of more than sixty books of prose and poetry for children and about two dozen plays for the same audience. And my mother has published several books of poems for children as well. I had the privilege of knowing almost every contemporary significant children’s writer personally. For fifteen years I worked as a writer and, from 1962 to 1975, as an editor for the children’s magazine *Kostyor* in Leningrad. I published a few books of my own and translated poetry for children. Nine of my plays for children were staged and published. At the same time, I was studying and collecting materials related to the history of Russian children’s literature, beginning with the 1920s, when the Russian literary avant-garde became involved in children’s literature.

At this point I am entering the conclusive stage of my project: to complete my manuscript on the “History of Modern Russian Literature for Children” I need to carry on some additional research in earlier Soviet periodicals and rare books and to double-check the materials that I copied in Soviet libraries some years ago. The NEH stipend would enable me to complete my work during the summer of 1982 by working in the libraries of Harvard and Yale, and, primarily, in the Library of Congress.

“Absolutely yes!” was the funding recommendation from one member of the review panel. The other members agreed.