# The Vannevar Bush Faculty Fellowship Proposer Guide

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### 1. Program Philosophy

This document is intended to provide help and guidelines in applying for the Vannevar Bush Faculty Fellowship (VBFF) program. Whetehr you have applied before or this is your first attempt, this guide is specifically addressed to *you*, the Principal Investigator (PI), while *we*, the Basic Research Office at the Office of the Under-Secretary for Research and Engineering (USD(R&E)), are hoping that it will help in crafting a better submission package, both white paper and proposal. In order to achieve that goal, we must consider the mechanics of the submission, but also the philosophy of the VBFF program. This is more important that it seems, because the VBFF program is rather unique, and noticeably different from many research opportunities you may be familiar with. Many attempts at the VBFF fail simply by not recognizing this difference. When reading the Federal Opportunity Announcement (FOA), you will see the following, bold-highlighted sentence:

**“VBFF is oriented towards bold and ambitious “blue sky” research that may lead to extraordinary outcomes, such as revolutionizing entire disciplines, creating entirely new fields, or disrupting accepted theories and perspectives.”**

Some explanation is in order. The most important conclusion to draw from that sentence is that we are not looking for “safe”, incremental advances. This is not to say that there is no value is steady progression, since scientific advances are mostly the result of such work. In fact, the big leaps rely on this vast body of steady and incremental (and more often than not, uncoordinated) efforts, which sets the fertile ground on which revolutionary ideas can flourish. However, there are many opportunities for that type of research. Here, we are looking for big jumps, and ambitious exploration. This also means higher risk, but this is where the program differs from many others. Risk is much more acceptable here – as long as the returns are commensurate. The latter are briefly listed above, for example: “revolutionizing entire disciplines”, “creating new fields”… Note that these beneficial returns are rather vague, described simply as “extraordinary outcomes”. Although this research program is sponsored by the Department of Defense (DoD), it is not necessary to specify how the research can address a specific application, technology or problem the DoD is facing. It does not hurt to know it – except that you may be tempted to concentrate excessively on that particular application, erroneously assuming that this is want this program is looking for. In fact, being narrowly focused will hurt your chances at success, so you need to have a broader and longer vision.

The ideal VBFF proposal is an exploration, and once a discovery takes place, things have a tendency to take a life of their own. If you truly discover the scientific equivalent of a new continent, it does not matter what the DoD may have thought it needed in the first place; the landscape, the rules of the game, have suddenly changed. There are also additional benefits along the way: you have set a new path and trained a new generation of scientists, who can further contribute to the R&D eco-system and expand leadership in this new field. Thus, your approach, your methods, and the skills that you and the next generation of scientists will develop, are also important.

The conviction that there IS something out there should however not come without a foundation; there must be preliminary hints, those obtained from the collection of tentative, incremental steps made (yours and others), and you must make convincing arguments that the possibility of such discovery exists. It also means that you are well aware of the landscape produced by this preliminary body of work, i.e., you are an expert, and you know your field inside-out.

This may seem overwhelming, since you cannot guarantee a discovery of that magnitude. Of course not. We accept the fact that it is a risky proposition. Nevertheless, you must have the conviction and the passion, and be able to articulate it.

Now that the program philosophy is explained – or so we hope – we can now review all phases, evaluation criteria, strategies and potential pitfalls. Note that this is a *guid*e only; it is not a *requirement* that you follow the guidelines and advices described here. You may totally ignore them and follow your own path; after all, it will be better if you can write with a firm inner conviction than try to follow an approach that you are not comfortable with or have not embraced – ultimately, it would show in your submission. You should reflect upon what you read in this document, but don’t have to agree with it, or blindly obey rules that you believe we are offering (in fact, we want to avoid strict guidelines and constraints). In the end, you should do what *you* think is best.

## 2. Topic Selection

The FOA comes with descriptions of several scientific areas of interest. These vary from year to year, either as a result of minor edits, broadening of the themes, or significant restructuring. One topic area may even disappear, or be combined with another. If you applied previously to one topic area and do not find it the next time around, you may be puzzled and concerned. However, it is highly unlikely that your research direction has disappeared entirely from the topics described in the announcement. Re-structuring of the topics has a lot more to do with obtaining the right balance of technical expertise required of the review panels, than a lack of interest from the DoD. However, the descriptions inside the topics can change, in response to evolving priorities that the DoD places on the various subjects. These topic descriptions are aimed at describing the range of scientific questions the DOD is interested in, but not defining specific research directions that would answer these questions, or solve the challenges facing the Department. Above all, your proposed solution, approach, and/or idea must be innovative, whether it is in a category of research directions already mentioned in the topic description, or not.

If you are unsure or do not believe that the research you are proposing fits into one of the main topics, you can always choose the “Other” category. This is not a topic with reduced interest; there have been several VBFF awards made under it, and the submissions are treated with the same level of attention as for the other topics. The main difference is that there is no single panel dedicated to it, since the range of scientific disciplines that may fall under it is so wide, that it would be impossible to assemble a panel a-priori, which could cover the entire range. Instead, review panels are constructed on the spot, depending on the white papers and proposals being submitted. There is also the possibility that your research lies at the boundary between topics, or is a combination. For example, you may look at the genetic component of memory formation and how to improve brain function at the cellular scale. Would this fit under bio-engineering or neuroscience? Starting with the 2021 announcement, you may explicitly choose two (primary and secondary). This does not mean that you double your chances; to the contrary, your idea would be evaluated by *two* panels in concert. If it peaks the interest of one panel, but the other one finds it flawed, it would be very unlikely that you would proceed to the full proposal phase or receive an award. There is, however, nothing unusual in the circumstances. Any multi-disciplinary project must face the inquisitive eyes of expert reviewers in the appropriate and relevant fields. A Multi-University Research Initiative (MURI) proposal, for example, would be subject to similar scrutiny.

This also does not mean that multi-disciplinary research is not encouraged in the VBFF program. To the contrary, it is believed that radical advances can be found at the intersection of disciplines, and these can be truly transformative. This does, however, place an additional burden – and the only one – on the PI because the VBFF is a *single-investigator* award. The PI cannot simply assemble a team of experts, or include a co-PI. Thus, there is in this case the burden of proof that the PI has the required skills and expertise to operate at the junction of two disciplines; this is an evaluation criterion. Nevertheless, the PI can call on collaborators for additional help, to a limited degree (see also the FOA for specific budgetary limitations); this issue is important and is being addressed in another section later in this document.

## 3. THE WHITE PAPER

As explained in the FOA, the process starts with a white paper, which is reviewed by a panel of experts, and after which you *may* be invited to submit a full proposal. The white paper phase is the most selective, since the fraction of proposal invitations/white papers is less than awards/proposals. It is therefore very important to construct a very good white paper. This is a difficult task – more difficult than a 15-page proposal, because you must be able to condense your arguments, the rationale, the approach, the technical merit, into a few paragraphs. Writing a good white paper can be considered an art form, and there is no unique recipe for it. We can only provide suggestions in the following. First of all, you should already have a good idea of what you want to say *in the proposal itself*. This does not mean that you must write the proposal first and just cut and paste in order to fit the page limit of the white paper. Rather, you must have pre-emptively thought of what arguments to present in support of your idea, and how to counter skepticism and objections; what your approach will look like; whether the scope is reasonable, and whether you can confidently claim that you have all the necessary skills. Clearly, you cannot expand on all these points in the limited space; so what is important?

Let us proceed in reverse order. First, you don't need to discuss a budget. It is expected that you will be asking for the maximum allowed, and there is no benefit in asking for less; if your scope of work is not commensurate with that budget, this is not the right program to apply to. You also do not need to liberally expand upon your experience and skills. You can provide a CV with the white paper, so use it, and mention only what is critically important and relevant to that specific project. If you are, for example, expanding your horizon and/or suggesting a multi-disciplinary project, it is worth explaining (in a brief but impactful way) how you have prepared yourself to conduct this research.

Next is the relevance and impact to the DoD. Of course, your project should be relevant, but this is worth discussing further, since it can have various interpretations. One could consider, for example, that research on climate change is relevant to the DoD. One could argue so, since the global climate affects the frequency and intensity of weather events, in turn affecting the execution of military operations. But you should ask yourself the following: a) is it a primary mission of the DoD to understand, predict or negate the effects of climate change? b) is there another government agency that would be much better suited to sponsor that research? The answer to the first question may seem to require knowledge about the range of activities that the DoD is tasked with. However, it is not difficult to imagine what the DoD cares about, without having to go into details and specifics of types of weapons and platforms or strategies being considered. In fact, as mentioned earlier, it would be rather detrimental to be too narrow and specific. The second question is more subtle; there are certainly areas for which other agencies take precedence; for example, the National Institute of Health (NIH), or the Department of Energy (DOE). The greatest overlap is with the National Science Foundation (NSF), which is the principal agency for conducting basic research. Many of the interests of the NSF are shared with the DoD: quantum science, artificial intelligence, materials, bio-engineering, etc. Most of you will have had multiple research projects funded by the NSF, including concurrent ones. This is not an issue, as long as the one submitted for the VBFF is sufficiently different and satisfies the objectives of the program (see Section I). In fact, these NSF-funded projects may have prepared you for applying to the VBFF.

Generally speaking, the panels reviewing your white paper will easily identify the potential benefits to the DoD. The white paper therefore does not require a lot of details on this subject matter, but some brief explanation is still recommended. Furthermore, if the proposed research leads to a totally new way of doing things, this should be emphasized. Be aware also that the DoD cares about operating in certain conditions that are not often found elsewhere. For example, if you propose a new way to manufacture some materials, but they are stable only in a narrow temperature range, this may be good for some commercial applications, but of more limited interest to the DoD.

We are left, finally, with the most important part of the white paper: the technical description. Here, you must be able to provide the right information; this is a qualitative and quantitative optimization problem. The most important questions to answer, and which the panel will be looking for, are:

1. Is this research highly innovative and even paradigm-shifting, or is it an incremental continuation of prior work?
2. Will this research open up new areas and disciplines if successful?
3. Is this fundamental research, as opposed to application-focused or data collection?
4. Does the approach make sense and appears feasible?

As explained in Section I, steady and incremental progress is valuable and recognized – but it is not the objective of this program. Here, we are looking for a significant change: a big leap forward, or a new direction. The timing must be right, i.e. there is sufficient preliminary indications that support the idea, and the methods and skills have developed sufficiently to make the approach feasible, and the idea has been thought-through carefully, re-examined, and re-examined again. Details can be put in references, but you must choose them well, and still describe the core scientific principles. Pictures can be helpful, but they must be readable, impactful, and critically relevant to the arguments. In the end, there is only so much help this guide can provide, since ultimately this calls for both the most creative instincts on the part of the PI, as well as a clear, logical and skillful exposition of the arguments.

## 4. THE PROPOSAL

### 5.1. <u>The Feedback</u>

As explained in the FOA, the process starts with a white paper, which is reviewed by a panel of experts, after which you *may* be invited to submit a full proposal. Let us then assume that you have successfully passed the first test. Congratulations! Making it to this part of the journey already indicates that your project and your personal expertise were considered of very high quality.

After the white paper is selected, the most important step a PI should take is thinking about how to properly address the comments made by the reviewers, and provided in the feedback to the white paper. It is disappointing to receive a proposal that fails to do so. In most cases, the same panelists will be reviewing the white papers and proposals, so it would be strategically unwise to ignore the feedback, especially since these are written with care and provided with the specific intent to make the proposal better. This does not mean you have to agree with the comments; it may very well be that the latter arose from a lack of clarity or insufficient explanation. This is not surprising, since the white paper format is very constraining and the lack of space forces you to skip a lot of details. In that case, this is just an indication that you must provide sufficient explanation in the full proposal, whether this was part of your initial plan or not. The comments also do not need to be rebutted in a systematic (and obvious) fashion, e.g., a bulleted list. You are not writing a response to the review of a submission to a journal; make it flow.

### 5.2. The Science

There is little guidance that can be provided on the scientific aspects, since these are the core of your proposal. The FOA provides some description of a broad range of interests, but we do not want in any way to guide or constrain your creativity. We can only emphasize, once again, the philosophy behind this program and repeat the most important criteria that you should pay attention to:

1. Is this research highly innovative and even paradigm-shifting, or is it an incremental continuation of prior work?
2. Will this research open up new areas and disciplines if successful?
3. Is this fundamental research, as opposed to application-focused or data collection?
4. Does the approach make sense and appears feasible?

We should point out, however, that this program is calling for a significant investment from you; in ideas, in focus, and in time. The fellowship is designed to allow a deep dive, and facilitates research in an important topic, for a long period of time. This should bring out your best effort, a significant time commitment, and your best research skills to the problem, without distractions of proposal-writing and time consuming reporting requirements. There are no milestones to adhere to. You are allowed to deviate from an original plan if the results suggest it. You have a lot of freedom in conducting this research, but in exchange, we ask that you think deeply about the science and explore to the best of your abilities. This fellowship should not be viewed as yet another award in a long list of income-generating projects.

### 5.3. Relevance to the DoD

The proposal evaluation by the panel includes considerations of relevance to the DoD. As explained earlier, the program does not necessarily ask you to identify a specific mission or problem that the DoD is facing *today* and needs an urgent solution to, but the research should be applicable to a general class of problems and missions. Because your proposal is about basic research, and transformative at that, this is generally not a problem. Nevertheless, you must think in broad terms and use your imagination. What would the soldier, sailor or airman of the future really need? What would warfare look like one or two decades from now? Again, you don't have to be specific and provide excruciating details of that vision, but you should provide some initial thoughts, in order to guide the panelists in the right direction.

Another important aspect of DoD relevance is whether DoD is the right sponsor for the type of research you are proposing. If your description is a good match with one of the topic areas described in the FOA, you should have no problem. Still, you should be careful; bio-medical research, for example, is better suited to the NIH than the DoD. For research related to energy production or efficiency, the DOE would be the better sponsor. Research in climate, ecology are the realm of NSF. As mentioned earlier, this program has the greatest overlap with NSF, but there are still many areas where the DoD relevance is non-existent. For example, if you are interested in the gravitational waves produced by black hole mergers, or the equation of state of quark matter at the core of some neutron stars, you may be tempted to propose it under the "Other" research category, but you will still have a difficult time justifying a DoD interest. If you can imagine future applications that can help national defense, as explained above, you should be on the right track. If you are unsure, you may want to do some research on your own, e.g., searching the web for ideas and commentaries on the future of warfare. The Program

Officers in Service-funding organizations such as the Army Research Office (ARO)[1], Air Force Office of Scientific Research (AFOSR)[2], and Office of Naval Research (ONR)[3] can also be an excellent resource that can help you place your research into the context of how it can help support DoD needs and operational missions. You may reach out to them or read their Broad Agency Announcements (BAAs) published in grants.gov. However, you can look to them for information about the general interests of the DoD, but these POs will not be able to comment on the merits of your idea. Some of you may already have established a relationship with the DoD, i.e. been funded by the DoD, and/or have interacted with the laboratories, i.e. Air Force Research Laboratory (AFRL), Naval Research Laboratory (NRL), or Army Research Laboratory (ARL). In this case, you may even include letters of support from them. However, none of that is a requirement. You should ideally show a clear enthusiasm for interacting with the DoD Research Enterprise, but there are many ways you can demonstrate it.

It is a common perception that the selected fellows have previously been funded and have maintained deep relationships with Program Officers throughout the years. Many fellows selected by this program have not received any prior awards, or even had any prior experience with the DoD. To be clear, this program does not intend to only select members from academia who already have established relationships with the DoD. This would greatly limit the breadth of knowledge and ideas that could be gained from the academic community, and since this program seeks deeply innovative and far-reaching ideas, it would be self-defeating to do so.

### 5.4. The Question of Collaborators

This program is the DoD's largest single-investigator research award. Co-Principal Investigators are not allowed under the VBFF. That being said, collaborations that are deemed necessary for the success of your project can be allowed, but your proposal should clearly indicate that the collaborative work is under your direction. **It is expected that the PI will be the main driver of the research effort, both intellectually and in the execution of the research work, within her/his laboratory.** Proposals that suggest that the success of the research is highly dependent on substantial intellectual contributions of specific collaborators will tend to be rated poorly. For research projects that are highly dependent on the intellectual input from multiple PIs, there are several other funding mechanisms in DoD that can support such efforts, namely the Multi-University Research Initiatives (MURI), DARPA, and the Tri-Service core grants, for example.

This issue of collaborators can be fraught with uncertainty. You may want to have a collaborator to supplement your skills and increase the chances of success, but should you be worried about being penalized? It depends. Does this collaborator receive a significant fraction of the award? Must it be that particular person? Is he/she participating throughout the entire duration of the project? This question makes multi-disciplinary projects particularly difficult, since the need for complementary expertise may be more acute. Nevertheless, we still expect you to be the intellectual driving force behind it, and this is one of the criteria you will evaluated against, i.e.: do you have what it takes to be a Vannevar Bush fellow? In those multi-disciplinary cases, the opportunity for you to initially learn from the collaboration and strike on your own, would appear more acceptable. Other situations are more straightforward. For

[1] https://www.arl.army.mil/who-we-are/aro/
[2] https://www.wpafb.af.mil/afrl/afosr/
[3] https://www.onr.navy.mil/

example, you may be a theorist but are collaborating with experimentalists who can verify and test your hypotheses and results. You may have more than one, as long as their overall budget remains a relatively small fraction. Unpaid collaborators, in this case, are probably better, as long as they are not a critical component and are somewhat inter-changeable (so that their unavailability does not suggest that the entire project would come to a screeching halt). Foreign collaborators fall into that category (they cannot be paid under this program). The white paper can be a good way to test the level of collaboration, if you are unsure. If you are invited to submit a full proposal, look for any comments regarding the suggested collaborators.

### 5.5. Previous Experience and Qualifications

This program requires fellows to already be leaders, at the forefront of their technical fields. Generally speaking, this is not an entry-way for young investigators (although age has nothing to do with the selection) who are in the early phase of their career. This also does not mean that applicants with decades of experience and lengthy lists of publications will be favored. Quality trumps quantity always, and what matters in the end is what you are proposing, and whether you are the right person to pull it off. The DoD looks for Fellows to start as leaders, and to expand their leadership status. The return on investment goes beyond the research itself. As a Fellow you are, to the DoD, a gift that keeps on giving, a resource to be…exploited is maybe too harsh a word, but a resource nonetheless. Fellows are expected to be partners with the DoD in discovering new areas of research in which the Department should be investing. Fellows also have the opportunity to serve in an advisory capacity to help direct the course of research interests of the Department, by serving on research boards, attending program reviews, and contributing to DoD workshops in this capacity.

The VBFF program is also interested in developing the next-generation DoD scientific workforce. The selection and mentoring of talented graduate students and post-doctoral fellows is therefore an important aspect of the Fellowship. These may later expand your work and develop their own brand of innovative research, and may eventually contribute to the DoD Research Enterprise in other and unexpected ways. Therefore, the proposal should point out any proven track record of mentoring students and junior faculty, and the CV is a good place to provide that list. This should not just be considered a simple box to check; the narrative should also contain a description of how you plan to provide this mentoring. This brings into consideration the time commitment made by the PI (and reflected on the budget), and the availability of the PI. Too many on-going projects may also translate into little availability, which affects both the research itself (see Section 4.2), but also the mentoring.

### 5.6. Personnel

This section is about who will be working on the project. Collaborators are discussed in Section 4.4 already, but only in the context of the importance given to them (not co-PIs!). Here, the question is more about the qualifications and experience of the personnel. Are they senior research personnel? Other faculty? Post-docs? Graduate students? The answer is simple: you decide. There is no strong guidance in choosing one versus the other. It is the proposed *research* that dictates what makes sense. The skills and experience of the personnel must match the scope, requirements and difficulty of the project. We mentioned strong guidance, meaning that we do not establish rules, but is there a weaker guidance? Something we wish for? To some degree, yes. We are very much interested in having a new generation of scientists grown and nurtured by the project. As described in the previous section, we expect that the type of explorative, transformative research that you would be conducting, calls for new scientists who can be exposed to the new ideas at very early stages of their career, and are able to carry these ideas forward and expand upon them. Therefore, we do wish to see graduate students funded in the proposal. However, this is not a requirement, and if only post-docs seem a better fit to the particular project, this is perfectly fine – some awards did not have any graduate students, just post-docs. Usually, there will be a balance, but this all depends on the nature of the research, and it is up to you to explain the particular personnel choice being made.

### 5.7. Budget

A proper budget may not seem very significant but it could be very detrimental if it contains extraordinary items which are not well justified. High-quality proposals can be sharply criticized for having such peculiar budget items. A frequent issue is questionable travel, for example, $300,000 for travel to Australia in one year. It is expected that travel to conferences, or visiting collaborators – if applicable – are reasonable requests. However, the PI should exercise good judgment. We expect that most of the budget goes into the research itself, and paying for salaries and equipment. Other expenses can also raise objections, such as excessive fees for conducting the work, whether for computing time or use of laboratory facilities, or consumables. Finally, there is the question of large capital expenses. Some research projects may require the acquisition of very expensive equipment, whether off-the-shelf or built according to some design specifications. This can be problematic, since the fellowship program is focused on performing the research, i.e. the activity that is enabled by the equipment. If the equipment cost is a significant fraction of the overall budget, this will be closely scrutinized. You could leverage other funds, which would be welcome, but in that case, the panels need to see a firm and undeniable commitment by the entities responsible for providing leveraged or matching funds. In any case, the more the budget items are out of the ordinary, the higher the level of justification and detail must be provided. It should be clear that all aspects of the budget are important and vital to the project, and the associated costs are reasonable expectations for the given requirements of the research.

### 5.8. Recommendation Letters

The application process comes with the submission of up to three letters of recommendation. These are submitted separately by the references, and are not shared with the applicant. The choice of references is entirely up to you, the PI. It is worthwhile, however, to explain briefly what we are looking for in these letters. As repeatedly mentioned earlier in this document, an important part of your selection as fellow is the determination of your scientific qualifications. Of course, the panels will look at your CV, but the recommendation letters provide more information, more explanations, and something that is more personal. This means that the references must know you well, or have known you well at some time in your career, and the letters should provide the storyline behind their impression of you and your work. What makes you special? The choice of the references also matters, to some degree. Are they well known in the scientific community? Are they experienced leaders in their fields? How deep was their interaction with you? These letters of recommendation should be chosen for supporting the evaluation of your scientific qualities. They should not be construed as a requirement to show ties to the DoD or its laboratories. If you believe that a DoD scientist can provide the best letter, that is entirely acceptable, but the scientific credentials of the reference, and the level of interaction will matter. The fact that the letterhead shows a DoD affiliation would play no role at all.

As a final word, we should also point out that it is the PI's responsibility to make sure that the letters are submitted on time. If received past the deadline, they will not be included in the submission packages to be examined by the review panels, and this should be communicated to the references. It is therefore recommended that the letters be sent a few days before, allowing sufficient time to verify that they have been properly received, as email communication can sometimes be subject to breakdown.

## 5. Re-Applicants

Researchers frequently apply for a VBFF award numerous times before they are selected for the fellowship. There is no sure path to success, but the ones who take the time to address all of the comments of the panels stand a better chance. The panels may also take note if the PIs have made progress in the proposed research since the last proposal. We keep track of previous submissions so that the panels can look at the changes that are made. However, this is not a guarantee. Each year, the competition starts anew, and although your proposal may have improved, others still may be found superior. If your proposal fails multiple times, it may very well be that the idea is not quite a good match with the objectives and criteria of the program. There is no penalty in changing a research topic, and a new proposal will be treated on its own. It should be pointed out that failure to obtain the fellowship does not imply that your work and ideas are less valuable. As stated at the beginning of this document, there are several types of research, and this program is rather unique in its objectives and requirements. The fellowship award may also depend on extraneous factors which are outside anyone's control, such as the competition field, or the timing of events (e.g., scientific discoveries or your career choices).

Another frequent question is whether previous fellows can apply again, and if there is a limit to the number of awards. Currently, there is no limitation or constraint, except that a currently active fellow, i.e. one with an active grant under this program, cannot obtain a second one. A prior fellow can apply again, but the proposal will be treated like anyone else's, and will be subject to the same criteria of innovation and scientific quality. Thus, a continuation of a previous work, or a submission that is highly related to the previous award, will be highly unlikely to succeed and even move past the first phase of white paper review. Nevertheless, if the idea is new, different, paradigm-shifting and with extraordinary potential, it will be of interest, irrespective of who submits it.

## 6. Concluding Remarks

To conclude this guide, we provide a checklist in the next page, which summarizes the points made earlier. One should be very much aware that this document is only an attempt to clarify the intent of this program and help you to better understand what the program is essentially looking for, and what the essence of a good proposal would be. This should not be construed as *rules*; you may very well be following all the advice given and be unsuccessful, and someone else may never read this document and be awarded a fellowship. Ultimately, you have full responsibility in choosing your research direction and convince the panel of its merit and your qualifications. Nevertheless, we hope this document is useful.

## 7. CHECKLIST

| Strength | Weakness |
|---|---|
| Transformative research, requiring deep thinking | Evolutionary research |
| Fundamental and broad scientific work | Geared mostly towards application |
| Potentially revolutionary scientific impact | Incremental knowledge gains |
| Well-written proposal, logically laid-out, free of grammatical and technical errors | Confusing write-up, unexplained technical jargon, uninformative figures. |
| Budget is well justified, with reasonable estimates for students, instrumentation, lab consumables, equipment user fees, travel and publication costs. | Insufficient or excessive travel budget for program activities; no proper justification for large capital equipment; or budget is generally not commensurate with the scope of the project |
| Collaborators will bring knowledge and complementary skills to the research, but it is clear that the PI has intellectual ownership. | The project is very dependent on the named collaborator(s) in order for the research to be successful. A co-PI is mentioned. |
| A relevance to the DoD is clearly identified, for a range of future applications. | Project has little relevance, or proposed work is much better suited for sponsorship by other Federal agencies (e.g. DOE, NIH, or NSF). |
| PI is considered a leader in her/his field, demonstrated by career growth, quality of work, and letters of recommendation. | PI has little publication record, or has not made any impact in the field of research he/she is proposing. |
| The proposal has a good balance of personnel and describes how mentoring a new generation of researchers will be accomplished. | The proposed research provides little opportunities for training graduate students or developing new skills and knowledge. |
| The PI provides a significant time commitment to the project, to lead the research. | The PI seems over-committed and there is little confidence that sufficient focus will be given to the project. |
| The PI describes a plan for interactions with DoD or DoD Service lab researchers. | The PI makes no mention of attempting to reach-out to the DoD research laboratories. |
| The proposal addresses the scientific challenges, and approaches to their solutions are well chosen and adapted to the tasks. | The proposed methodologies are frequently used in the field and have not shown great promise for future breakthroughs, or are unlikely to be sufficient to meet the challenges. |
| The proposal shows that significant thought into risk mitigation strategies in a high-risk proposal. | The methodology is linear, each task is critically dependent on the success of earlier ones. |