

Example summary comments from the Graduate Committee reviewing Biological Sciences student RGGs proposals (note, comments were cropped in some places to preserve anonymity):

Reviewers all agree that the proposal is well written with a clear hypotheses, strong experimental design, sufficiently detailed protocol and anticipated results. The research seems innovative and new with potentially important implications for dental care. The weakness of the proposal is that the proposal may be overambitious and the

Very interesting idea and the merit of the study is high. However, the author failed to demonstrate he had a clear handle on the science and background literature/information. The hypothesis and suggested potential conclusions have some incongruities with regard to the time frames involved. There were grammatical errors throughout and the writing was not the best. The reviewers recommend funding this proposal if funds are available.

The narrative of the proposal is very well written and the proposed experiment is justified well. The procedures are clearly laid out and the aims of the proposal have a high probability of being achieved. The project has potential to identify new and exciting information that will contribute to the understanding of gravity perception in plants. The reviewers agreed that the high level of scientific merit of this proposal outweighed grammatical errors and word choice issues through out the proposal and we strongly encourage funding.

The proposal is very well written, with a clear hypothesis, interesting study idea, and a novel approach to answering the question. The proposal includes a clear description of the scientific merit and makes a strong connection to the bigger picture. Some preliminary data are given for the technical procedure and the remaining experiments are outlined well. Study should be easily completed in the timeframe. The reviewers recommend funding this proposal.

The proposal offers a very interesting study idea, with the potential to be significant piece of research. However, as presented the research isn't clearly rooted in background literature or fundamentals of this field (i.e. why this study is the next clear and logical step was not evident). The methodology is extremely vague and writing errors are problematic. The hypothesis is too broadly stated for the actual experiments proposed. For example, the hypothesis

White tailed deer are a problem species so this study is significant. The project has clear aims, experimental design, and experimental questions. The data should provide evidence about the amount and type of damage, if any, is being sustained by the trees having various treatments. The project should produce results that can actually be used by wildlife managers. The reviewers recommend funding this proposal.

The proposal is incomplete. The methodology is extremely vague and writing errors are problematic. There is no explanation of how any sequence differences that might be found would be evaluated to determine if they are causative for the phenotype or just chance variation. There is no explanation of what the genes to be sequenced code for or how they relate to the hypothesis.