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Brain Research Needs New Ideas

One can only sympathize with the argument put forward by Stanley B. Prusiner and George P. Shultz in "[Brain Research and the Challenge of Aging](#)" (op-ed, July 5). Neurodegenerative diseases exact a terrible toll on millions of lives around the world. However, it only compounds the tragedy if we accept without question the authors' fundamental premise that insufficient funding has stymied the development of effective therapies.

The time is now for the biomedical research communities and disease advocates to be willing to ask questions that do not focus on "How much?," but rather "For what?" Fifty years of pursuing targeted research on a limited number of hypotheses and laboratory disease models that fail to fully represent the full complexity of human disease have mostly led to failed attempts to translate the knowledge acquired into clinical therapies. Through a complex chain of circumstances, what happens is that the focus of research shifts to the models and away from reality. If money were the only issue, we would have solved problems ranging from education to cancer.

Too often lack of progress is the fault of inadequate knowledge. Throwing more resources (research dollars, drug development funds, clinical trial investments, and thousands of highly trained personnel) at the challenges of pathological brain aging without challenging some deeply entrenched assumptions about these diseases, and how it is that they ravage brain function, are doomed to repeat the past. Changing the future requires new thinking. Rather than react emotionally and do more of the same, we will acknowledge the lack of progress and create opportunities for the innovation desperately needed.

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