Edited by **Jennifer Sills**

Basic science: Bedrock of progress

ALMOST 4 YEARS ago, one of us (F.S.C.) wrote an Editorial (*1*) affirming the continued importance of basic research to the National Institutes of Health (NIH) mission. The Editorial emphasized that basic scientific discovery is the engine that powers the biomedical enterprise, and NIH continues to spend more than half its budget supporting basic research projects. This is critical, because the private sector generally funds projects that yield a more rapid return on investment.

Despite these assurances, some members

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of the community believe that NIH's interest in basic science is flagging. For example, investigators have told us that the requirement for a "Public Health Relevance" statement in every NIH research grant application suggests that every project must relate directly to a public health concern—that NIH places less value on projects that cannot be expected to yield an immediate public health benefit. This is simply not true. As we wrote in our Strategic Plan (*2*), we recognize that many of the most important medical advances trace back to basic research that had no explicit disease link. To address this concern, we have revised our application instructions (*3*) so that the Public Health Relevance statement reflects the NIH mission and our commitment to supporting a robust, diverse research portfolio, including the pursuit of basic biological knowledge.

We are particularly concerned that misperceptions about NIH's priorities and interests may be causing investigators to submit fewer basic research applications. For example, the National Institute of Neurological Disorders and Stroke (NINDS) noticed a gradual and significant decline in the number of basic grants awarded between 1997 and 2012 (*4*). This decrease in awards was not a consequence of peer review given that basic grant applications actually did substantially better in review than applied research proposals. Instead, the major driver of this decline was a decrease in the number of fundamental basic research applications submitted.

The taxpayer investment in NIH has yielded spectacular returns from basic science over the long term. These range from the discoveries of the low-density lipoprotein receptor (*5*) and the development of CRISPR-associated protein-9 nuclease (*6*, *7*) to recent substantial advances in structural biology through cryo-electron microscopy (*8*). For this track record of success to continue, we must continue our vigorous support of the pursuit of fundamental knowledge. All of NIH's senior leaders believe strongly that progress toward these goals occurs most rapidly when investigators pursue their passions, whether they lie in basic research or in applied, disease-focused studies. By supporting a broad portfolio of basic, translational, population, and clinical research, NIH will continue to lead the way toward a healthier future.

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A research symbiont

M. MCNUTT ("#IAMARESEARCHPARASITE," Editorial, 4 March, p. 1005) can be proud to be a "research parasite." The creators of this term, Longo and Drazen (*1*), miss the very point of scientific research when they write that researchers may "even use the [open] data to try to disprove what the original investigators had posited." It is at the core of the scientific paradigm that researchers take nothing as final truth. In fact, using research data to try to disprove a result is

good scientific practice, especially in light of the replication crisis (*2*–*4*).

However, Longo and Drazen are right that scientific data sharing deserves recognition. They suggest that credit for data sharing should take the form of co-authorship, but co-authorship as the sole instrument of credit will unnecessarily restrict the potential of data sharing and could be a detriment to the original researcher (for instance, if the resulting publications lack quality) (*5*). In the case of replication studies and meta-analyses, co-authorship makes no scientific sense.

A more suitable instrument would be a much higher appraisal of data sharing by research communities through citations of data sets, awards, and the consideration of data "production" in career prospects, funding applications, and evaluations. With this end in mind, research parasites are beneficial for the organism as a whole.

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Pseudonymous fame

J. BOHANNON'S In Depth story "Fight over author pseudonyms could flare again" (26 February, p. 902) described problems stemming from authors using the pseudonymous screen names under which they had done their research instead of their real names. The most famous case of pseudonymous authorship occurred over a century ago in the form of William Sealy Gosset's famous 1908 paper "The probable error of a mean," for which he used the simple pseudonym Student (*1*). This work set the stage for what is now known as Student's *t* test, a hypothesis-testing tool familiar to practically every analyst and statistician. Gosset was employed by Arthur Guinness and Sons brewery in Dublin, and legend holds that his use of a pen name was prompted by the company's concern for secrecy in their use of statistical methods for quality control.

Gosset's case notwithstanding, pseudonyms will hopefully not become a more regular occurrence. Ephemeral screen names may be acceptable for Internet message boards, but their use in research papers may ultimately lower the public's perception of the transparency, integrity, and timelessness of the permanent scientific record of human knowledge.

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TECHNICAL COMMENT ABSTRACTS

Comment on "Single-trial spike trains in parietal cortex reveal discrete steps during decision-making"

Michael N. Shadlen, Roozbeh Kiani, William T. Newsome, Joshua I. Gold, Daniel M. Wolpert, Ariel Zylberberg, Jochen Ditterich, Victor de Lafuente, Tianming Yang, Jamie Roitman

Latimer *et al.* (Reports, 10 July 2015, p. 184) claim that during perceptual decision formation, parietal neurons undergo one-time, discrete steps in firing rate instead of gradual changes that represent the accumulation of evidence. However, that conclusion rests on unsubstantiated assumptions about the time window of evidence accumulation, and their stepping model cannot explain existing data as effectively as evidence-accumulation models.

Full text at http://dx.doi.org/10.1126/science.aad3242

Response to Comment on "Single-trial spike trains in parietal cortex reveal discrete steps during decision-making"

Kenneth W. Latimer, Jacob L. Yates, Miriam L. R. Meister, Alexander C. Huk, Jonathan W. Pillow

Shadlen *et al.*'s Comment focuses on extrapolations of our results that were not implied or asserted in our Report. They discuss alternate analyses of average firing rates in other tasks, the relationship between neural activity and behavior, and possible extensions of the standard models we examined. Although interesting to contemplate, these points are not germane to the findings of our Report: that stepping dynamics provided a better statistical description of lateral intraparietal area spike trains than diffusion-to-bound dynamics for a majority of neurons.

Full text at http://dx.doi.org/10.1126/science.aad3596