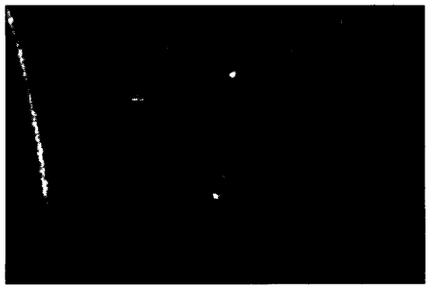
**In response to an earlier letter, a physiologist says that the name "Henneman's Size Principle" is "rightfully referred to." On the topic of "UFO research," a scholar says that "science is legitimized by its methodology, not the subject matter it investigates." And whether a "racial divide on the Internet" exists is debated.**

## UFOs and the Scientific Method

The article "Panel says some UFO reports worthy of study" by David Kestenbaum (News of the Week, 3 July, p. 21) describes one critic who "worries that the report will unjustly legitimize UFO research" and notes that some scientists "have a record of enthusiasm for these exotic topics." Is it the topic under investigation that determines whether or not research is legitimate? Does a researcher's passion for his subject makes its legitimacy suspect?



JOURNAL OF SCIENTIFIC EXPLORATION

**Could this be your thesis?**

At a time when media attention and public interest in anomalous phenomena seem to be at a peak, should we not demonstrate that science is legitimized by its methodology, not the subject matter it investigates? And do we really expect to attract potential scientists to the calling with the message that they must be dispassionate? If lack of passion is the criterion by which a researcher's work is to be validated, how much research qualifies?

It is, of course, appropriate to make personal judgments about how fruitful UFO research is likely to be, or to decide (as a physicist cited in the article concludes) that UFO research may be "just a total waste of time." But such conclusions should be based on an examination of the evidence itself.

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## Henneman's Size Principle: The Right Name

In their letter "Renaming the 'Henneman Size Principle'" (26 June, p. 2031), J. A. Vilensky and S. Gilman correctly point out that Denny-Brown and Pennybacker (*1*) made a landmark observation in the 1930s when they showed that mammalian motor units tend to be activated in a fixed sequence from the weakest to the strongest units. This finding, however, does not represent the "size principle" as Vilensky and Gilman suggest; rather, it describes the phenomenon of orderly recruitment. It was not until the seminal work of Elwood Henneman in *Science* (Reports, 27 Dec. 1957, p. 1345) that the neural mechanisms underlying orderly recruitment began to be revealed.

On the basis of innovative experiments and biophysical reasoning, Henneman and his colleagues proposed that the amount of excitatory input required to activate a motoneuron is directly related to its size (surface area of soma and dendrites). It was argued, therefore, that activation of motoneurons should proceed from smallest to largest as the broadly distributed excitatory input to a pool of motoneurons (*2*) increases. Moreover, smaller (and more excitable) motor neurons have thinner axons that give rise to proportionately fewer terminal branches, which in turn innervate smaller numbers of muscle fibers. Consequently, recruitment progresses "automatically" from weak to strong muscle units.

These ideas, rightfully referred to as "Henneman's size principle," not only explain the phenomenon of orderly recruitment originally observed by Denny-Brown and Pennybacker, but also represent one of the few parsimonious and testable hypotheses describing the functional organization of any population of neurons.

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1. D. Denny-Brown and J. B. Pennybacker, *Brain* **61**, 311 (1938).
2. L. M. Mendell and E. Henneman, *J. Neurophysiol.* **34**, 171 (1971).

The seminal importance of the Denny-Brown and Pennybacker paper (*1*) was discussed in several reviews of the historical development of the "size principle" (*2*). Moreover, Henneman and his co-workers were aware of the Denny-Brown and Pennebacker observations as evidenced by this paragraph from a 1968 paper (*3*):

> Denny-Brown and Pennebacker [*4*] and Norris and Gasteiger [*5*] also observed orderly recruitment, although they did not carry out a statistical analysis of their data. They attributed this recruitment to properties of the motor neurons and suggested that the larger motor neurons may have higher thresholds and innervate larger motor units. Although the evidence to support these suggestions was not available, their observations are in harmony with ours and their suggested interpretation, in view of later developments, is remarkably prescient.

Henneman, of course, did not give the "size principle" its commemorative moniker. Nonetheless, he was reproved more than once for not giving Denny-Brown more credit in the development of his ideas. I know from conversations with Henneman that he regretted having done so.

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## Division on the Internet?

In their Policy Forum "Bridging the racial divide on the Internet" (17 Apr., p. 390), Donna L. Hoffman and Thomas P. Novak present data on computer access and use as well as purchase intentions among white and black Americans. They find that overall, blacks are less likely to own computers or to have used them recently for Internet access, and that they are more likely to want to buy one. Hoffman and Novak call for improvements of "educational opportunities for African Americans." This suggestion is good, but unrelated to the data. As they are presented and analyzed, the data cast more shadow than light on this important topic.

The study apparently has no hypotheses. blacks and whites are grouped eight different ways and their responses to 13 items (for example, "own home computer") are compared by tests of statistical significance. Of the 104 tests, 42 are significant, and 33 of those indicate greater "digitality" among whites. It is difficult to say whether this is a large number of significant results because few of the comparisons are independent. If one looks only at the primary variables for ownership, purchase intentions, and Web use, for example, only 6 of the 11 significant differences favor whites.

Some "surprising" results have been picked up by the popular press. For example, among low-income respondents, "whites were almost six times more likely than their African American counterparts to have used the Web in the past week." This ratio is based on 5.9% and 1.1% for whites and blacks usage, respectively. Such ratios are deceptive because they increase as the overall base rates of the response decreases. It seems more newsworthy that 23% of low-income blacks plan to buy a computer as compared with 14% of such whites, although this ratio is less extreme.

The median split of the sample by household income obscures important information. If it is correct that "increasing levels of income correspond to an increased likelihood of owning a home computer, regardless of race," this relationship may also hold among low-income respondents. If the average income of low-income blacks is lower than the average income of low-income whites, the "digital divide" result is confounded by other variables, money and race.

Hoffman and Novak state that whites are more likely than blacks to own computers regardless of education. Education is related to racial differences in computer access at work, but among the respondents with a college education, blacks are more likely to report access (63.9%) than whites (55%). The face validity of the conclusion notwithstanding, it is unclear how these data suggest that "increasing levels of education are needed to promote computer access and Web use."

What are the means and ends in the computer age? The traditional idea is that computers facilitate learning. To call for more education to boost computer sales is to put the cart before the horse. And what proportion of computer use is beneficial? Chat rooms, pornographic sites, and on-line advertisement are popular, but their contributions to public education are likely negative.

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Hoffman and Novak present some interesting and potentially revealing data concerning the use of the Internet by different sectors of U.S. society. Their presentation is marred, however, by their use and description and use of the term "race."

Biologically, it is generally agreed that no "races" can be meaningfully defined in the human species. Yet the use of the term in scholarly literature persists, particularly in disciplines like sociology (where race and ethnicity are often interchangeable). The confusion is even greater in the general public.

Scientists, at least, should at least be consistent in their use of these terms. Hoffman and Novak are not. Why are "whites," a reputed physical description, compared with "African Americans," a partial description of ancestry? Who are the "whites" in this study, anyone who is not of African American ancestry? Are Arab Americans included in the "white" pool? Do "African Americans" only include those whose ancestors came to the United States as slaves, or also recently immigrated Africans (Nigerians, for example, who are amongst some of the most educated people in the world) and Caribbean émigrés? How were students with one Euro-American or Asian-American parent classified? And will we soon see research about the "Internet avoidance gene" and its distribution among the "races?"

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Although partisans like to refer to computers as fostering democracy, the technology seems more likely to exacerbate social stratification. The real inequities are probably worse than the data in the study indicate because the information was collected by a telephone survey. Yet 18% of black households lack phones (the same for Hispanic homes; 80% on some Native American reservations), while over 95% of white homes have them. So the survey contains a sampling bias.

"Universal access" to the Internet is unlikely to be realized. Over 60 years of federal public policy devoted to achieving universal phone service has not prevented the maldistribution of this older technology. In a country that now appears to have a declining interest in equity considerations, it would take a political revolution to lead to a different result for computers and the Internet.

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Hoffman's and Novak's work was based on data that are over a year old. This is a fast-changing medium. Another study (*1*) based on 1998 data finds that the racial composition of Web users in the United States was "statistically indistinguishable from U.S. Census data for the general public." The major difference between Web users and others are in education and age: College graduates were online in a higher fraction (38%) than their presence in the general population (22%) would suggest. And the Net is still "skewing young."

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1. D. S. Birdsell, D. Muzzio, D. Krane, A. Cottreau, *Public Perspective* (April/May 1998). Available at: http://www.ropercenter.uconn.edu/pubper/pdf/pp93b.pdf

Hoffman and Novak presented some interesting data, but did not look at changes in Internet use over time (which are dramatic), and consequently come to incorrect conclusions. In the United States, one needs only motivation, a computer, and an Internet provider to access the Net. A used computer adequate for Internet access now costs a few hundred dollars, less than many TV sets. In the San Francisco Bay area, Internet access costs about $20 per month, less than cable TV. Thus, the economic barriers to Internet use are low and dropping fast.

Hoffman and Novak's data suggest that Americans of African descent have ample motivation. The size of this user group, however, is orders of magnitude greater than that of 3 or 4 years ago. Improved Internet access for those lacking it requires no policy changes. The economics of the computer revolution will inevitably—barring world war, major asteroid collision, or catastrophic global epidemic—rapidly lead to greatly expanded Internet use by any and all groups who want it.

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Hoffman and Novak discuss the "haves" and "have nots" and appear to assume that all "members of our society" desire the "rewards of this transformation." I have lots of things to see, hear, feel, measure, contemplate, and write about, and have never felt like a "have not." I observe, however, that many "haves" have not, as they have not time for anything but the Net.

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***Response***

Krueger mentions that our study has "no hypotheses." We take this to be a compliment. Preconceived hypotheses often reveal inherent biases of the researchers. We strove for complete impartiality in our analysis, believing the best approach was to simply "let the data speak."

Many people from across the United States have responded directly to us, drawing conclusions from their own perspectives. Thus, we have received many "hypotheses." Prejudices aside, these various reactions raise two main questions.

Is the difference between racial groups, as indicated by our data, real? Krueger states that 42 out of 104 is a "large number of significant tests." While it is difficult to factor in the effect of correlated dependent variables, if the variables were independent, one would only expect 5 of 104 tests to be statistically significant at $P = 0.05$. We would expect by chance to find far fewer than 42 significant differences.

Of more importance than statistical significance is the size of the differences themselves. For example, 17.8 percent of whites, as opposed to only 9.7 of African Americans, used the Web in the past month (during the study period); 33% of white students but only 13% of African American students had ever used the Web at home; and 27.2% of African Americans versus 16.7% of whites say that they would like to purchase a personal computer (PC) in the following 6 months.

As Bereano notes, because of the greater presence of telephones in white households, we likely underestimated the differences between whites and African Americans in computer access and Internet use. The differences we have uncovered are substantial, and should be a source of concern for all Americans.

Has the difference disappeared? Powell notes that our data from January 1997 were over a year old at time of publication. The more recent paper he cites (*1*), however, gives the same aggregate percentage of African Americans online (approximately 10%) as did our study. Also, it did not actually report on elements of a racial divide, as detailed segment-level data were not presented. Finally, we find no information as to whether African Americans or Hispanics in their sample were, in fact, statistically representative of the U.S. population on variables such as education, age, and student status. Thus, we do not know if the minority respondents in that study had, as we suspect, higher educational attainment (and thus greater Internet use) than those in the nation as a whole.

A paper (*2*) based on U.S. census data collected in October 1997 (approximately 10 months after the data in our study were collected) found that "the digital divide between racial groups in PC-ownership has actually *increased* since 1994" [italics sic].

We appreciated the opportunity to address these reactions to our Policy Forum and look forward to continued public debate about this important social issue.

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1. D. S. Birdsell, D. Muzzio, D. Krane, A. Cottreau, *Public Perspective* (April/May 1998), p. 33
2. J. W. McConnaughey and W. Lader, *Falling Through the Net II. New Data on the Digital Divide* (U.S. Department of Commerce, Washington, DC, 1998). http://www.ntia.doc.gov/ntiahome/net2/falling.html

## CORRECTIONS AND CLARIFICATIONS

The response "Dangerous mixture" by Y. Li (Letters 7 Aug., p. 783) should have included a second author, Yitai Qian.

In the article "Inflections: A cause of artery-clogging plaques?" by T. Gura (News Focus, 3 July, p. 35), the bacteria *Chlamydia pneumonia* was incorrectly cited as the cause of sexually transmitted diseases. *Chlamydia pneumonia* causes upper respiratory infections, while a related microbe, *Chlamydia trachomatis*, is the culprit in sexually transmitted diseases and several eye diseases, such as trachoma. Also, the name of University of Helsinki researcher Pekka Saiku was misspelled.

In the Table of Contents of 31 July (Books and New Media, p. 613), the name of K. Wailoo was misspelled.

In the article "How calcium enhances plant salt tolerance" by E. Epstein (*Science's* Compass, 19 June, p. 1907), reference 3 should have included "J. Li, Y.-R. J. Lee, S. M. Assmann, *Plant Physiol.* **116**, 785 (1998)."

In the article "Memory and Awareness" by D. L. Schacter (*Science's* Compass, 3 Apr., p. 59), in the table (p. 59), the row labeled "Trace conditioning" should have read, "Yes" in both columns. Also, the sentence beginning on line 9 of the first full paragraph on page 60 should have read, "The magnitude of this word-priming effect is similar in healthy participants who become aware during the test that they are producing words from the study list and those who do not (*6*)."

In the report "Skeletal muscle regeneration by bone marrow–derived myogenic progenitors" by G. Ferrari *et al.* (6 Mar., p. 1528), note 23 should have included acknowledgment of financial support from "Istituto Pasteur-Fondazione Cenci Bolognetti."