the data by the scientific community. I therefore think that there is an urgent need, when publishing scientific data, to distinguish clearly in references between perennial information (available on the Web but also safely preserved in multiple institutional libraries scattered around the world and accessible anytime to anybody) and information with an unpredictable lifetime (that is, available on the Web exclusively from one server). In order to protect our credibility, reference to possibly short-lived information in scientific publications should, I believe, be restricted to commentaries and, perhaps, letters and systematically banned from regular articles and reviews.

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Research on Auditory Cortex Plasticity

This year, Michael P. Kilgard and Michael M. Merzenich published a report, "Cortical map reorganization enabled by nucleus basalis activity" (13 Mar., p. 1714). In 1996, we published a study, "Induction of physiological memory in the cerebral cortex by stimulation of the nucleus basalis" (*1*). In both studies, pairing a tone with nucleus basalis stimulation produced tone-specific changes of neuronal responses in the primary auditory cortex of adult animals. We reported associative receptive field changes that involved selective increased response to a frequency paired with stimulation of the nucleus basalis, similar to the receptive field plasticity that is induced during tone-shock associative behavioral learning (*2*). In our study of the nucleus basalis, as in behavioral experiments, plasticity was induced in a single session of 30 pairings and only in paired versus unpaired groups. The results of both studies support a long-standing cholinergic model of learning-induced cortical receptive field plasticity (*3*).

We welcome the additional observations of map changes that devolve from receptive field changes and the statement of a cholinergic basis for the effects of nucleus basalis stimulation. Readers should be aware, however, that the 1998 report is not the first study to show that paired activation of the nucleus basalis is sufficient to induce specific receptive field changes in the auditory cortex. The subject of the two papers has been of sufficient general interest to warrant invited commentaries: a *Science*'s Compass research commentary, "Mapping the sensory mosaic" by Sharon L. Juliano (13 Mar., p. 1653), and a commentary by Charles D. Gilbert (*4*), which provide overviews of the field.

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Response: Weinberger was among the first to hypothesize that the central cholinergic system is involved in the synaptic plasticity underlying fear conditioning and other forms of learning. Members of his laboratory have probed these mechanisms with the use of both electrical stimulation of nucleus basalis and direct iontophoresis of cholinergic agonists. This rich body of work represents a significant portion of the foundation for our recent experiments. It was not our intention to overlook any of the relevant earlier studies by Weinberger and others, and we cited several of them (*1*) in our report. Furthermore, our report did not state that we were the first to use cholinergic modulation to generate receptive field plasticity.

Four main points made in our report are, to our knowledge, new. First, as Weinberger mentions, our study quantified plasticity guided by cholinergic modulation at the level of the cortical map by recording from up to 100 locations in a single animal. Second, we demonstrated that the plasticity we recorded was progressive over the course of several weeks and endured for at least 24 hours. Third, the observed map reorganizations were of a larger scale than would be expected from short-term studies of receptive field plasticity. Fourth, and most important, we demonstrated that the details of the stimulus paired with nucleus basalis activation determine whether receptive fields expand or contract.

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One Infant's Memory of *Oedipus*

In their report "Infants' memory for spoken words" (26 Sept. 1997, p. 1984), my former colleague Peter W. Jusczyk and Elizabeth A. Hohne give an elegant demonstration of language memory in infants who do not yet speak but who had, of course, been exposed to speech for many months.

In 1941, psychologist Harold E. Burtt published the last of three papers of a related experiment on language memory in the absence of "knowledge" of the language (*1*). When the child (Benjamin B. Burtt) was 15 months old, his father read 20-line passages from Sophocles's *Oedipus Tyrannus*, changing them every 3 months until the boy reached the age of 3, for a total of seven selections. Benjamin was subsequently tested for his memory of the passages by a prompting-learning method at the ages of 8.5 years, 14 years, and 18 years. The seven selections plus three additional ones chosen for comparability were learned through many prompting sessions in a rotating order so that every passage appeared approximately equally often in every position. The results were clear: at 8.5 years it took about 27% to 30% fewer repetitions to learn the previously heard material than the new material; the passages heard later in the 3-month learning period were learned most quickly; at the age of 14, the savings advantage was reduced to about 8%; the last test, at age 18, revealed no savings at all, although Benjamin reported that the material sounded familiar. It would be interesting if, 18 years hence, Jusczyk and Hohne were to locate some of the participants in their study and test them for response latencies to the Story-Word and Foil-Word lists.

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1. H. E. Burtt, *J. Genet. Psychol.* **40**, 287 (1932); *ibid.* **50**, 187 (1937); *ibid.* **58**, 435 (1941).

Jusczyk and Hohne demonstrate that 8-month-old infants can remember words spoken 2 weeks earlier. From their findings they conclude, "These results indicate that 8- to 9-month-olds engage in long-term encoding and retention of information about the sound pattern of words that occur frequently in fluent speech," and "[w]ord learning depends on associating underlying categories or concepts with the internalized representations of the sound patterns of words" (p. 1985). These conclusions are not supported because Jusczyk and Hohne did not run the proper control conditions, which should have included the following: presenting infrequent nonwords, in nonfluent speech, in a context where there are no underlying categories or concepts. If such a control experiment were to find significant retention of auditory material, relative to an appropriate control, the superstructure proposed by Jusczyk and Hohne would collapse, and one would have to conclude that repeated exposure to arbitrary sounds is all that is necessary in order to remember the sounds. Because such a conclusion contradicts current thinking and appears, on the face of it, untenable, it is unlikely that anyone would undertake such a study today. Fortunately, the experiment was done, and done well, 57 years ago by Burtt (*2–4*).

The report by Jusczyk and Hohne and the study by Burtt agree, however, in that they both show that young children (8-months-old and 15-months-old, respectively) can put spoken material into long-term memory under appropriate experimental conditions. However, Burtt used 20-line selections of Greek drama, which he characterized as nonsense syllables. These lines did not contain "words," as we commonly use that term; they were not part of fluent speech; they were most certainly not frequently occurring sounds; and the sounds were not representative of any underlying categories or concepts. Thus, Burtt's study supports the findings of Jusczyk and Hohne, but negates their conclusions. One factor is common to both studies: they show that infants will remember sounds to which they have been exposed repeatedly at an earlier time. From Burtt's findings, I conclude that Jusczyk and Hohne's language learning edifice does not have an empirical foundation (*2*).

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1. H. E. Burtt, *J. Genet. Psychol.* **40**, 287 (1932); *ibid.* **50**, 187 (1937); *ibid.* **58**, 435 (1941).
2. Readers might have a reasonable inclination to place less weight on single-subject studies. One might also view older studies with greater skepticism because of procedural differences and because theoretical ideas were then so different. However, Ebbinghaus's classical curve of forgetting [H. E. Gararett, *Great Experiments in Psychology* (Appleton-Century, New York, 1941)] was developed with the use of one subject—himself—and with nonsense syllables (which he invented to avoid the problem of differential word familiarity).

Response: We thank Littman and Denenberg for calling our attention to the interesting studies by Burtt (*1*). Although there are some significant differences between Burtt's study and our own, the overall pattern of results in the two investigations is similar. It would be interesting to follow up on Littman's suggestion to test these same children many years later to see whether they still respond differentially to the words that appeared in the stories they heard when they were 8 months old. Unfortunately, this may prove difficult to do.

Denenberg raises several criticisms of our report, and we would like to respond to each in turn. An assumption that is largely shared by researchers in the field is that learning a word requires linking a sound pattern with a meaning. Having stated that point in our introduction, we then indicated that the focus of our investigation was on a prerequisite for vocabulary acquisition, namely, infants beginning to store information about the sound patterns of particular words. Throughout the report, we were careful to indicate that we were talking about the long-term storage of the *sound patterns* of words and noted that there was little contextual support for the words from the surrounding environment (that is, no one was teaching the infants the meanings of the sound patterns). Thus, we viewed our test situation as one in which, as Denenberg puts it, "there are no underlying categories or concepts," particularly with respect to the types of sound patterns that appeared in our word lists.

There are several interesting issues that can be raised with respect to "arbitrary sounds." For example, to what extent did the infants segment sound patterns that correspond to whole words rather than part of a word or parts of adjacent words? Although we did not explicitly test this question, we have explored it in other studies. Infants familiarized with pairs of monosyllabic words responded to the whole sound patterns of those words, rather than to a particular portion of those words, such as the vowels or endings of the words (*2*). Sven Mattys and I (*3*) have examined whether infants who heard a passage in which a particular sound sequence occurred as parts of adjoining words in a passage (for example, "hard ice," "cold ice," "weird ice," and so on) would infer the occurrence of the sound pattern (the word "dice") in these contexts. In contrast to cases in which the sound pattern of a whole word appears in such a passage, the infants did not extract "dice" when the sequence occurred as parts of adjoining words ("hard ice"). This finding suggests that infants are sensitive to word boundaries in these types of passages.

Another issue related to arbitrariness concerns the extent to which 8-month-olds have to know anything about the sound structure of the language in order to segment words from fluent speech. Rochelle Newman and I investigated whether English-learning infants could extract words from speech in a foreign language, Mandarin Chinese. Even with a pretraining period involving 30 minutes of exposure to the language for 5 days before the test session, the infants did not give evidence of segmenting the familiarized pair of Mandarin Chinese words from fluent speech passages (*4*). It appears that infants require some extended period of experience with the sound organization of a language to discover how word boundaries are marked in that language.

Exposing a 15-month-old to 30 minutes of speech for 21 months may have provided sufficient experience to extract cues to word boundaries, although this is not what Burtt actually tested in his investigation. Denenberg seems to suggest that it is important to use a nonfluent speech, context, but it is not clear why this would be superior to a fluent speech context. Fluent speech is connected speech, as opposed to lists of isolated words. Extracting words from fluent speech is much more difficult than extracting words that are spoken in isolation, as can be attested by any researcher who has worked on automatic speech recognition devices. One of the unexpected aspects of our report was that infants did pick out the sound patterns of the words after only hearing them in fluent speech contexts. Also, contrary to Denenberg's assertion, Burtt's materials do qualify as fluent speech.

Finally, when we referred to the sound patterns of the words as frequent, we were referring to their occurrences within the stories, not in infants' typical home environments. Given that Burtt's materials were drawn from a drama by Sophocles, it is probable that some of the sound patterns of the words recurred often in the 20-line excerpts. As we noted in the report's introduction, little is known about infants' long-term memory for the sound patterns of words. Our study was an initial foray into this territory, not the last word on the matter. A more elaborated picture of what infants remember about sound patterns is critical for an understanding of how they apprehend the structure of the language they are acquiring.

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Corrections and Clarifications

■ The name of the deputy vice chancellor of the National University of Malaysia in the special report on Science in Southeast Asia was incorrect (6 Mar., p. 1467). He is A. H. Zakri, a plant geneticist.

■ The credit for the photo of the acorns on the cover of the 13 February issue (Caption, p. 955) should have read "M. Ahearn."

■ In the 13 February ScienceScope (p. 973), the color photo should have been credited to Marie Read.

■ The names of three coauthors of the report "Results from the Mars Pathfinder camera" by P. H. Smith *et al.* (5 Dec. 1997, p. 1758) were inadvertently omitted. They are F. Gliem and P. Rueffer at the Technical University of Braunschweig, 38106 Braunschweig, Germany, and S. Hviid at the Orsted Laboratory, Niels Bohr Institute for Astronomy, Physics, and Geophysics, Copenhagen University, 2100 Copenhagen Ø, Denmark.

Letters to the Editor

Letters may be submitted by e-mail (at science_letters@aaas.org), fax (202-789-4669), or regular mail (*Science*, 1200 New York Avenue, NW, Washington, DC 20005, USA). Letters are not routinely acknowledged. Full addresses, signatures, and daytime phone numbers should be included. Letters should be brief (300 words or less) and may be edited for reasons of clarity or space. They may appear in print and/or on the World Wide Web. Letter writers are not consulted before publication.