pression differed in the KCl group. When this was estimated from the records, it was found that the three subjects that did show retention (group B in Table 1) showed a shorter duration of maximum EEG depression (*8*) than those that did not show retention (group A in Table 1). This difference in depression was a reliable one ($P<.016$; two-tailed Mann Whitney U test); thus duration of EEG depression was inversely related to retention.

As a test for the possibility that the injections had produced hippocampal lesions, three of the group that had been injected with KCl and that showed a retention deficit were retrained in the apparatus. All three subsequently showed retention, indicating that neither performance nor the ability to retain was permanently impaired by the injection (*9*).

After being tested, the animals were perfused with formalin, and the brains were removed. After several days, the frozen brains were sectioned (50-μ sections) and stained to verify electrode placements. All electrodes were located in the ventral posterior hippocampus. Some of the animals that had been injected with KCl sustained small, uniformly unilateral lesions. In fact, one of the animals with the most extensive damage was a saline control. Furthermore, the animals injected with KCl could not be differentiated from those injected with saline on the basis of extent of lesion. Finally, the animal that had sustained the largest lesion was later retrainable. Thus, even the largest histologically estimated lesion was insufficient to interfere with retraining or retesting.

The results indicate that temporary interruption of hippocampal electrical activity as much as 24 hours after learning can produce retrograde amnesia. This finding is unusual in that it has been generally acknowledged that electrical activity need only occur for a few minutes after learning, after which permanent storage is assumed to have taken place. Our findings indicate that, at least in some areas of the brain, and with our particular experimental technique, electrical activity must continue for at least 24 hours. Somewhat similar results have also been found with bilateral hippocampal lesions and with deafferentation (*10*).

It has been reported that potassium chloride can inhibit protein synthesis (*11*), but neither the percentage nor duration of inhibition approaches that which puromycin must evidently produce in order for amnesia to occur (*12*). Although procedural differences may account for the differences in degree of inhibition, there is at least the suggestion that the deficit seen with KCl is not mediated by an inhibition of protein synthesis.

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7. There were no differences between the group injected with saline and that injected with KCl in the first 100 and the subsequent ten licks in the training session as evaluated by the U test. For the saline animals the medians were 53.8 for the first 100 and 4.6 seconds for the subsequent ten; the corresponding medians for the animals injected with KCl were 98.8 and 4.8. The times for the first 100 licks between training and test were, however, significantly different for the saline group but not the KCl group ($P < .05$; Wilcoxen matched-pairs test). Similarly, the KCl and saline groups differed in time to 100 licks during the test session ($P < .05$; U test). The medians were 64.6 seconds for KCl and 190.5 seconds for saline.
8. Maximum depression is defined as activity showing an amplitude of less than 10 μv.
9. As an added check on the possibility that KCl produces damage and thus a deficit in suppression, other animals were first given bilateral injections of KCl, were trained 3 days later, and then were tested 1 day after training. The mean duration of suppression in tone was 232.0 seconds (see the saline controls in Table 1). The water intake was also recorded in these rats; no effect on intake was noted.
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27 May 1968

Premature Citations of Zoological Nomina

While agreeing with Sohn (*1*) that premature citations of zoological nomina (technical names of animal taxa) may be undesirable, I believe that the argument should be modified in two respects.

Sohn gives as one example the publication by Egorov in 1953 of a previously unpublished specific nomen as *Mossolovella incognita* (Glebovskaja and Zaspelova in litt.) and states that the taxon (that is, its nomen) should be cited as *M. incognita* Egorov, 1953. In my opinion, that is probably incorrect under the present Code (*2*). Egorov's wording suggests that "some other person (or persons) is alone responsible both for the name and the conditions that make it available" (*2*, Article 50). If that is true, the name should be cited as *M. incognita* (Glebovskaja and Zaspelova in Egorov, 1953) [*2*, Article 51(c)]. The expression "the conditions that make it available" is not explained in the Code. In this particular example, it can be debated whether Glebovskaja and Zaspelova were in fact alone responsible for such conditions, although from Sohn's statement it would appear that they are. I am not particularly concerned with possible disagreement about the specific example but with the fact that the Code does provide for crediting nomina and their definitions to others than the authors of the paper in which they first appear.

Just this point is not covered by the inadequate and nonmandatory published Code of Ethics (*2*, Appendix A), but it is obviously simple good manners to seek permission from the authors, if possible, before publishing their nomen. I can see no cause for confusion in this practice, and nothing objectionable if done with permission.

A second point, not covered by Sohn except by implication, is that publication of *nomina nuda* may be justified and even desirable under special circumstances. If a nomen and its definition by author A are known by B to be in press or in a manuscript assured of publication and if the corresponding taxon is involved in publication by B, in my opinion it is desirable that B publish the nomen with or without quotation of A's definition and in either case with ascription to A, "in litt." or "in press." If published by B with definition quoted from A, under the Code the nomen dates from B's publication but is correctly ascribed to A, not to B. If published by B without definition, the nomen is a *nomen nudum* but will assuredly cease to be so when published by A and will take the authorship and date of the latter publication. In either case the convenience and accuracy of B's work are promoted, because the eventual nomen of a taxon discussed is given, without such ambiguity as, for example, writing about "a species later to be named by A," or just "an unnamed species" and without omitting relevant information. If in every instance B must wait for issue of A's publication before completing his own manuscript, work in that field will be appreciably retarded and the advancement of the science impeded. This situation is also not covered by the skimpy appendix on ethics in the Code, but again common courtesy indicates that B should communicate his intention to A and obtain agreement if possible.

The points made by Sohn and the additions suggested here bear not only on nomenclatural confusion but also on assignment of responsibility and on historical accuracy. Both those desirable ends are preserved in my suggestions. It should, however, be kept in mind that the primary aim of the Code and of acceptable nomenclature is the achievement of a clear, universal, and stable system of nomina. The Code is not basically concerned with responsibility or historical accuracy.

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9 February 1968

The fact that Professor Simpson and I arrived at diametrically opposed interpretations of authorship in the example given underscores the point that use of "in litt." results in nomenclatural snarls. Because application of the Code to this particular example is peripheral to the points we both made, I shall not discuss the reasons for my use of Articles 9(6) and 50 rather than Article 51(c).

Professor Simpson correctly interpreted my implication that under certain circumstances *nomina nuda* may be justified and desirable. I categorize *nomina nuda* in two classes: legitimate and illegitimate. Legitimate nude names are those used in circumstances described by Simpson. They clarify communication when deliberately introduced and documented by reference which will eventually validate them. Illegitimate *nomina nuda* are the inadvertent offspring of careless writing and poor editing. They confuse communication and should be aborted.

A point hinted at by Professor Simpson is the fact that the Code is in need of additional polishing. Recommendations referred to the International Commission on Zoological Nomenclature at the 16th International Congress of Zoology, Washington, D.C., 1963, were to be ratified or rejected at the next international zoological congress, then scheduled to meet in 1968. No announcement of a 1968 or later meeting of the congress or the commission has been circulated at this writing.

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Notes

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Ethyl Alcohol Consumption: Valid Measurement in Albino Rats

In his paper describing the breeding of rats with a phenotype for alcohol preference, Eriksson (*1*) assumes that the use of a single solution of ethyl alcohol provides an adequate method whereby alcohol preference in the Wistar strain can be evaluated. The procedure based on this assumption makes the validity of the individual measurements of fluid intake questionable and may perhaps undermine the significance of his findings.

Richter and Campbell (*2*) have shown that the amount of alcohol which rats drink in a self-selection situation is directly dependent upon the concentration of the solution offered. As a result of Richter's work, the concept of an alcohol preference threshold has gradually evolved over the years (*3*). By means of a three-bottle method to test an individual rat, a stable and reliable alcohol preference curve can be obtained when drinking bottles are rotated randomly on a daily basis (*4*). We have found that the alcohol preference threshold for several strains, including Wistar and Long-Evans, is usually somewhat lower than 10 percent. In fact, as solutions are increased in strength, total preference for ethanol will often shift to total aversion, usually between 5 and 7 percent (*5*). Most preference functions decline sharply as the selection of ethanol ceases, and no animal drinks the same amount (grams) of absolute ethanol when diluted to concentrations between 5 and 15 percent (*3*).

Eriksson reports that he has found that rats consume the same amount of absolute alcohol when offered the choice between water and solutions of alcohol ranging from 5 to 15 percent, and thus he dismisses the criticism of Fuller (*6*) pertaining to the use of a single concentration of ethanol for determining a phenotype. Furthermore, he writes that Rick and Wilson report the same observation (*7*), but these authors clearly state that "in selection experiments with rats, alcohol concentrations between 2 and 8 percent should be offered when maximum consumption is desired. Except in the case of animals specially bred to consume 10 percent alcohol, it appears inadvisable to offer rats 10 percent alcohol and to attempt to draw conclusions about their behavior, or their metabolism of alcohol, from their voluntary selection. This concentration of alcohol appears to be above the maximum preference level of the Wistar rats used in the present experiment."

To use a single concentration for describing the alcohol preference of an animal is analogous to the pharmacologist's attempt to describe the biological properties of a drug solely by administering one dose of that drug rather than by obtaining a dose-response curve. It is conceivable that if 4 percent alcohol is offered to rats instead of the arbitrary 10 percent solution, all animals would consume large amounts of the fluid; but if the choice were limited to 16 percent alcohol and water, probably none of the Wistar rats would drink the alcohol solution. In any case, measuring the intake of a single concentration of ethanol provides no information about the complex spectrum of factors governing the alcohol selection of individual rats under different experimental conditions.

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27 March 1968

Myers states (*1*) that I assume that a single alcohol solution can be used for determination of the alcohol preference of rats. This is not a mere as-