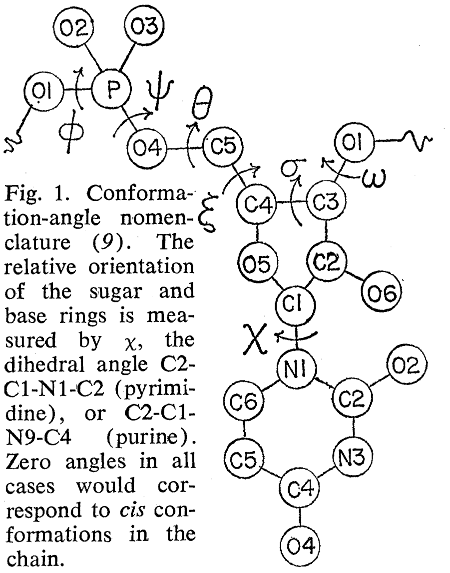

Fig. 1. Conformation-angle nomenclature (*9*). The relative orientation of the sugar and base rings is measured by χ, the dihedral angle C2-C1-N1-C2 (pyrimidine), or C2-C1-N9-C4 (purine). Zero angles in all cases would correspond to *cis* conformations in the chain.

polynucleotide result than is commonly observed. The three exceptions, while noteworthy, should not be allowed to obscure this interesting result.

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7. The polymer structure determinations considered appropriate all involve nucleotides with (C3′-*endo*, C2′-*exo*)furanose conformations as in UpA. Further, only structure determinations where linked-atom, least-squares refinement (*5*) has produced optimized conformation angle values are included.
8. The monomer survey presented here includes only values of χ corresponding to the *anti* conformation of J. Donohue and K. N. Trueblood [*J. Mol. Biol.* **2**, 363 (1960)] since this is the only type observed at the four bases of the two different UpA molecules.
9. S. Arnott and D. W. L. Hukins, *Nature* **224**, 886 (1969).
10. This research was supported by NIH grant GM17371 and an Eli Lilly grant.

7 February 1972

Arnott and Hukins' comment consists of essentially two parts. First, they point out that we have made some errors in reporting the torsion angles of A-RNA (RNA-11). Second, they claim that our emphasizing the conformational differences between UpA and A-RNA has obscured the similarities in their conformation. Regarding the first point, we acknowledge that errors were made in citing some of the torsion angles of A-RNA. Four of the values were in error by 2°, 4°, 5°, and 8°. It should be remarked that these errors are comparable to the estimated standard errors (E.S.D.'s in the torsion angles generally obtained in fiber diffraction studies of polynucleotides [It may be noted that Arnott and Hukins in their comment have made errors of 6°, 8°, and 11° in reporting the values of the torsion angles C2-C1-N1-C2 (pyrimidine) and C2-C1-N9-C4 (purine) of UpA1 and UpA2 which they deduced from our published values of the glycosyl tension angles χ_{CN}. These errors are highly significant in comparison to the E.S.D.'s in the torsion angles in the UpA structure.] The errors in the A-RNA torsion angles neither invalidate our conclusion that the major differences in the overall conformations of UpA1 and UpA2 and the corresponding unit in A-RNA occur in the conformation angles about the P-O3′ and P-O5′ ester bonds, nor do they obscure the similarities in the remaining conformation angles of the sugar-phosphate bonds (*1*, table 1). We naturally emphasized these striking conformational differences because the conformation about the P-O3′ bond in UpA1 differs by 113° from the corresponding value in A-RNA while the conformations about P-O3′ and P-O5′ in UpA2 differ by 167° and 162°, respectively, from the corresponding values in A-RNA. Furthermore, the emphasis of P-O bond rotations was made because it has important bearing on the folding of polynucleotide chains into hairpin loops.

With respect to their second point of having obscured the conformational similarities in UpA and A-RNA, we completely disagree. It is clear from our comparison of the torsion angles given in Table 1 that the conformational angles of the sugar-phosphate backbone other than those about the P-O ester bonds are similar. In addition, we did emphasize (*1*) that all four nucleoside moieties of the two UpA molecules exhibit the preferred *anti* conformation about the glycosyl bond and C3′-*endo*, C2′-*exo* sugar puckering similar to that of A-RNA.

We have given further details of the comparison between UpA, mononucleotides and nucleosides, and polynucleotides elsewhere (*2*).

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19 May 1972; revised 27 July 1972

Stability in Zoological Nomenclature

Mayr *et al.* (*1*) discussed the procedure of protecting well-established names of animals by means of Article 23(b) of the International Code of Zoological Nomenclature but they did not present the complete history of Declaration 43 (*2*) in their article. They quote Declaration 43, but omit the first two items of the Declaration, which read:

> 1. Article 23(b) is hereby repealed.
> 2. For the period from 6 November 1961 to the date of publication of this present Declaration [December 1970], Article 23(b) is to be read as follows:

The only reference to these statements by Mayr *et al.* is in their note 2:

> The so-called "Declaration 43" . . . purporting to repeal Art. 23(b), does not represent the vote of the Commission . . . Furthermore, the Commission has the authority to classify and interpret the Rules, but only the International Congress of Zoology can repeal any provision of the Rules.

Repeal of Art. 23(b) certainly does represent the vote of a majority of the Commission as shown in the 27-page history of the case that follows the Declaration. The members of the Commission voted on four proposals:

1) Accepting the draft Declaration as a satisfactory new text of Art. 23(b) —passed 16 to 7.

2) Requesting the XVII International Congress of Zoology to replace the text of Art. 23(b) with the present Declaration—passed 14 to 8.

3) Making the Declaration come into force as of January 1961—passed 16 to 6.

4) Requesting the XVII International Congress to delete Art. 23(b) from the Code—passed 13 to 10.

Item 4 seems to us clear and unambiguous, and there can be no question of the opinion of the majority of the Commission. This vote was taken only for the record (*2*), and was not for publication. Several of the Commissioners objected to the inclusion of this vote with the Declaration because they did not consider it official, and this presumably forms the basis for the statement in Mayr *et al.* (*1*) concerning the vote. However, the issue was submitted a second time to the Commission, asking if they approved the publication of the vote, and the ballot on this question was 17 to 5 in favor.

The comments of the Commissioners as published show a deep schism within the Commission on the validity of the actions taken on this Declaration, and we consider it unfortunate that Mayr and his colleagues have not made this schism clear in their statement because it will be read and acted upon by many who have no access to the Declaration itself. If the opinions of the majority of the Commissioners are to be accepted, one cannot accept the minority opinion expressed by Mayr *et al.*

Mayr *et al.* state, as if à fait accompli, that the Commission cannot repeal Art. 23(b). However, in the history following Declaration 43, it is clear that the legal adviser to the Commission, the Secretary, and some Commissioners do believe that the Commission has the authority to delete or suspend parts of the Code, such as Art. 23(b).

The important question for the working taxonomist is, if the Commission does have this authority, did they in fact repeal or suspend Art. 23(b). As defined by Art. 78 of the Code, a Declaration is a provisional amendment to the Code. It is to be issued by the Commission in a case that "involves a situation that is not properly or completely covered by the Code," and this Declaration remains in force until the next succeeding Congress ratifies or rejects it. We believe that the Commission did suspend Art. 23(b) until the next International Congress of Zoology this summer and that zoologists who are seeking to preserve well-established names must apply to the Commission to preserve them under the plenary powers.

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3 January 1972; revised 23 March 1972

The history of the Statute of Limitation is long and tortuous, and of no interest to the working zoologist. This is the reason why we restricted our note (*1*) to the undisputed facts. The publication of the "repeal" of Art. 23(b) (Declaration 43) was based on a misunderstanding by the Secretary of the Commission, and he was asked by the Acting President of the Commission to withdraw it. Pressure of work seems to have prevented him up to now from doing so by publication.

In the meantime the Council of the Commission together with an ad hoc Committee on the Constitution of the Commission met in London (13–15 June) and confirmed that the provisionally adopted wording of Art. 23(b) (*1*) be submitted to the 17th International Congress of Zoology at Monaco for ratification (that is, inclusion in the Code or rejection). None of those present at the meeting (including the Secretary) expressed the opinion that the article was repealed. Indeed it has been uninterruptedly in force, in one version or another, since the present code was published (1961). It is regrettable that Collette, Cohen, and Peters have insisted in publishing their confusing statement even though they were informed about the true facts of the case.

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20 June 1972

Use of Variations in Natural Nitrogen Isotope Abundance for Environmental Studies: A Questionable Approach

Kohl, Shearer, and Commoner (*1*) have published values for the alleged contribution of fertilizer nitrogen to nitrate levels of the Sangamon River and Lake Decatur, Illinois. Their data, which are being used to influence proposed legislation to regulate agricultural use of nitrogen fertilizers, were obtained by a method based on slight differences in the natural isotopic composition of soil nitrogen, atmospheric nitrogen, and fertilizer nitrogen. Our experience in the use of isotopes in soil research causes us to question the ability of their method to produce valid quantitative information concerning the sources of nitrate in surface waters.

Kohl *et al.* made three principal sets of measurements: (i) the ^{15}N concentrations of nitrogen fertilizers, (ii) the total amounts and ^{15}N concentrations of nitrate nitrogen in drain-tile waters, and (iii) the ^{15}N concentration of nitrate derived from soil incubated in the laboratory. They expressed their ^{15}N data in terms of δ^{15}N units, a calculated value where one unit is equivalent to 0.0004 atom percent ^{15}N. The maximum average difference in ^{15}N concentration among the samples they studied was 0.0040 ± 0.0004 atom percent, corresponding to 10.0 ± 1.0 δ^{15}N units. Using measurements (i) and (ii) to obtain reference values, Kohl *et al.* calculated the fractional contributions of soil and fertilizer nitrogen to nitrate in surface waters from the results of measurement (ii). Even if analytic measurements can be made with precision over this extremely narrow range of detection, we question the validity of the data and their interpretation.

First, we question whether the value +3, which they used for δ^{15}N, is representative of the ^{15}N concentration of fertilizer nitrogen after its addition to soil. Their use of this value presupposes that fertilizer nitrogen enters into and is released from the soil organic complex without a change in its isotopic identity, or else that all fertilizer-derived nitrate is formed from the fertilizer directly. Neither of these assumptions is valid.

An indeterminate amount of fertilizer nitrogen (primarily in the ammonium form) mixes with nitrogen in the soil organic complex before it is biologically oxidized to nitrate and loses its identity. Therefore, their value for fertilizer nitrogen had a probable average value in soil other than δ^{15}N = +3. Further, in using this value as a reference point, Kohl *et al.* assume that the ^{15}N concentration of nitrate derived from fertilizer is identical to that of