This formula, diluted with water, has been found particularly efficacious when employed as a pre-emergence *and* as a contact application on areas where planned treatment has inadvertently been postponed. We may assume that the postponement allows weed and grass growth to reach a height of about 1″. In such cases a somewhat heavier application in slightly increased volume generally results in a very satisfactory control. The duration of effect may, and usually does, exceed a continuous period of three months.

Where 2,4-DAC is to be applied without dilution, it is not necessary, of course, to include the emulsifying agent in the formula.

FRANCIS E. HANCE

Experiment Station,
Hawaiian Sugar Planters' Association, Honolulu

The American-Soviet Science Society

As officers and former officers of the American-Soviet Science Society, we should like to make a statement concerning the way in which the work of the Society was brought to an end by the failure of the U. S. Treasury to grant us a tax exemption certificate as a nonprofit educational organization.

The Society, which was formed in 1945 as the successor to the Science Committee of the National Council of American-Soviet Friendship, has notified its members that it is unable at present to carry on its normal activities. It had been engaged in facilitating relations between scientists in the U.S.A. and U.S.S.R. through the publication of scientific articles by Russian authors in American journals, the circulation in the United States of scientific books, journals, and reprints from the U.S.S.R., and particularly by acquainting American scientists, through its "Bulletin," with scientific work published or in progress in the Soviet Union.

When the Society became an independent organization in May 1946 by severance of relations with the National Council of American-Soviet Friendship, it was realized that the small dues and contributions it was able to obtain would not suffice to support the extensive administrative and editorial work which had increased greatly since 1943 or to convert the "Bulletin" into the full-fledged *American Review of Soviet Science* which had been planned. Consequently, a grant for the support of this work of international exchange and publication was obtained from the Rockefeller Foundation in June 1946. It was known at the time that this grant could be paid only when the Society should be in possession of a tax-exemption certification from the U. S. Treasury; and it was assumed that since the Science Committee had enjoyed such status, it would be granted to the Society as a matter of course, following its application in June 1946.

In making such an assumption the officers failed to reckon with the obstructive tactics by which for two years the Treasury Department has failed to act upon our application. The only reason given is contained in a letter from Mr. E. I. McLarney, Deputy Commissioner of Internal Revenue, dated April 28, 1947, who stated: "It appears from newspaper articles recently published that the Committee on Un-American Activities of the House of Representatives proposes to investigate the matter of whether your activities and those of certain of your leaders are detrimental to the interests of the United States. Under these circumstances a definite ruling on your status for Federal income tax purposes is being deferred pending further development of facts."

The "newspaper articles" were those concerned with the campaign waged by the Un-American Activities ("Thomas") Committee of the House against Dr. Edward U. Condon, director of the Bureau of Standards, who had been a member of the Executive Committee of the American-Soviet Science Society. The Society has never been on the so-called "subversive list" of the Attorney General, and no grounds whatever exist for any suspicion concerning the motives or actions of its officers in connection with American-Soviet scientific exchange. In common with other organizations striving to improve American-Soviet relations, its work has been hampered by the atmosphere of suspicion created by the Thomas Committee, which, in the absence of any inquiry, has by insinuation alone sufficiently influenced a department of the Government to prevent our receiving the material support needed for our work. This has taken place in spite of the approval given to the Society's scientific exchange service by American scientists, by libraries, by many government bureaus and departments which have used our service, by the Rockefeller Foundation, and by the State of New York, under whose laws the Society is incorporated as an educational organization. It is a sad commentary on the administration of a government department that it prefers unsubstantiated insinuations to these solid evidences of the scientific standing of the Society and its value to American science.

L. C. DUNN, *President,*
American-Soviet Science Society, until May 1946
HARLOW SHAPLEY, *Honorary President*
ALICE HAMILTON, *Honorary Vice-President*
LEO LOEB, *Honorary Vice-President*
DUNCAN A. MACINNES, *President,*
American-Soviet Science Society, Inc.

On the Number of Genes in Man

Reliable determination of the number of separate gene loci has not been made for any organism. For experimental forms, especially *Drosophila*, rough approximations are established (e.g. J. W. Gowen and E. H. Gay. *Genetics*, 1933, 18, 1–31; D. E. Lea. *J. Genetics*, 1940, 39, 181–188; H. J. Muller. *Proc. int. Congr. plant Sci.*, 1929, 1, 897–921). Although ideally controlled experimental results are not available, considerable theoretical interest attaches to the problem of gene number in man. For instance, the tempo of human evolution is, among other things, a function of the number of gene loci susceptible to mutation (S. Wright. *Bull. Amer. math. Soc.*, 1942, 48, 223–246). Another source of interest is that an estimate of gene number in man illustrates the

magnitude of the task for a "complete" human genetics. Even rough approximations are here relevant.

Estimates now available on the number of gene loci in man are based on argument by analogy from *Drosophila*. These arguments employ a single human datum, chromosome number. J. S. Huxley (*Evolution, the modern synthesis.* New York: Harper, 1943. P. 50), to cite a single case, suggests that man has 4–6 times the gene number of the fruit fly (where published estimates vary by a factor of about 6.5)—that is, a minimum of 8,000 to a maximum of 78,000. Sample sources of error in such arguments are the assumptions that human and *Drosophila* chromosomes contain the same mean number of genes and that they contain equal amounts of genetically inert material.

This note outlines two approaches to the problem of gene number using additional data specified on man. Individually, neither approach is fully satisfactory. Together they provide an interesting, but highly speculative, approximation.

(1) In *Drosophila* the total haploid chromosomal length with the X is about 6.85×10^{-4} cm (Gowen and Gay). Assume that the fruit fly has 5,000 genes (salivary chromosomal data; C. B. Bridges. *J. Heredity*, 1935, 26, 60–64); each gene would "occupy" an average of 13.7 units of this length. Evans and Swezy (*Mem. Univ. Calif.*, 1929, 9, 1–41) have measured the mean total length of all chromosomes in 10 late prophase nuclei from various tissues from 4 humans. Measurements by Andres and Navashin (*Proc. Maxim Gorky med.-genet. Res. Inst.*, 1936, 4, 506–524) provide nearly equivalent results for the 10 largest chromosomes from several individuals. These data suggest that the total haploid length in man, at a division stage roughly comparable to the *Drosophila* data, is about 58.46×10^{-4} cm. Letting human and *Drosophila* genes occupy the same mean chromosomal length, man would have a little over 42,000 gene loci.

(2) The notion of lethal mutation permits a second estimate. Assume that 22% of all conceptions terminate in nonviable offspring (A. H. Schultz. *Contr. Embryol.* (Carnegie Instn Wash. Publ. No. 275), 1921, 56, 177–191). This value is a little higher than the mean of 9 estimates by other workers ranging from 14% to 30.3% (A. S. Parkes. *Eugenics Rev.*, 1926, 17, 275–293). Of such abortus assume a sex ratio of 120.25 (Schultz)—a value somewhat lower than that (133.03) obtained by Ciocco (*Human Biol.*, 1938, 10, 36–64, 235–250) for stillbirths; in 27 estimates by various other workers this ratio ranges from 101 to 229 males for each 100 females (Parkes). Among these abortus, assume that the excess of males, E, amounting to 2.04% of all conceptions, is due to lethal mutations in the nonhomologous portion of the X—the chief chromosomal differential (together with the relatively small, nonhomologous region of the Y) between individual males and females. Statistical evidence strongly indicates the occurrence of such sex-linked lethals in man (C. C. Little and M. Gibbons. *Proc. Soc. exp. Biol. Med.*, 1921, 18, 111–115). If each locus in the nonhomologous part of the X mutates to lethal at a rate, r, of 1 in 50,000 conceptions (the approximate mutation rate for the normal sex-linked gene to its allele for hemophilia; J. B. S. Haldane. *J. Genetics*, 1935, 31, 317–326), then the number of loci, n, in the nonhomologous portion of the X, is given by $n = rE = 1{,}020$. It requires mention that small changes in the values of E and r will make for large differences in the value of n. More reliable data on aborted and stillborn fetuses in man (Ciocco; C. C. Little. *In* Mueller, Little, and Snyder's *Genetics, medicine, and man.* Ithaca: Cornell Univ. Press, 1947. P.71) and on mutation rates are much to be desired. On the assumption of a higher sex ratio for abortus, say 133.03 (Ciocco), the value of n becomes 1,560. An estimate of 1,000–1,500 sex-linked loci is in some accordance with indications from data for the sex-linked loci of myopic nightblindness and deuteranopia that the nonhomologous portion of the X is genetically "long"—that is, nearly 50 cross-over units in length (T. White. *J. Genetics*, 1940, 40, 403–438).

The cytological length of the X chromosome in man is about 4.5 μ; the nonhomologous segment is about 2/3 of the total length (P. C. Koller. *Proc. roy. Soc. Edinb.*, 1937, 57, 194–214). The ratio of the nonhomologous portion of the X to total haploid chromosomal length is of the order 1: 19.5. On the basis of these speculations there are then some 19,890–30,420 gene loci in man.

The above two independent approaches suggest that the number of gene loci in man is of the order 20,000–42,000.

J. N. Spuhler

The Ohio State University

On the Solubility of Fibrin Clots

It is generally assumed that the fibrin clot formed by the action of thrombin in fibrinogen solutions or in oxalated plasma is the same as that formed during blood clotting or during the clotting of recalcinated plasma by its own thrombin. There are, however, marked differences between fibrin clots with respect to their solubilities. A clot obtained in purified fibrinogen solution or in oxalated plasma by the action of purified thrombin dissolves readily when an equal volume of 60% urea is added to the clot. The clot formed in recalcinated plasma by its own thrombin, however, does not dissolve in urea solution.

It was found that two factors together are responsible for rendering the clot insoluble in urea solution: one of them is the calcium ions and the other is some serum component which seems to be thermolabile. Adding these two factors in sufficient concentrations to purified fibrinogen prior to the addition of thrombin, the clot formed will be insoluble. Neither the calcium ions nor oxalated serum alone renders the clot insoluble. These observations are in accordance with the results of Kenneth C. Robbins (*Amer. J. Physiol.*, 1944, 142, 581), who studied the solubility of fibrin in weak acids and alkalies.

A detailed account of this work will appear shortly in *Acta Physiologica Hungarica.*

K. Laki and L. Lóránd

Institute of Biochemistry,
University, Budapest