several seasons of unusual rains, and the river, therefore, simply marks a rise of the ground-water level to a point above that of the valley bottom. It is believed that it will disappear when the ground-water is again reduced to its normal level.

M. L. Fuller,
Secretary.

UNIVERSITY OF COLORADO SCIENTIFIC SOCIETY.

During November and December, 1905, the society held seven meetings. The papers presented were as follows:

Professor M. F. Libby: 'Growth in Childhood and Adolescence.'

Dr. E. Barber Queal: 'The Causes of Dyspepsia.'

Professor Charles C. Ayer: 'The Phonograph in Modern Language Teaching.'

Dr. William P. Harlow: 'The Blood in Health and Disease.'

Judge Junius Henderson: 'Extinct and Existing Glaciers of Colorado.'

Dr. Luman M. Giffin: 'The Necessity for Pure Water.'

Professor M. S. Ketchum: 'Sources of Water-Supplies and Methods of Distribution.'

Dr. Dessie B. Robertson: 'Methods of Bacteriological Analysis of Water.'

Dr. George H. Cattermole: 'The Pollution of Water-Supplies.'

Professor Francis Ramaley: 'Noted Typhoid Epidemics.'

Two evenings were given to the papers devoted to the subject of water supplies. An attempt is being made by the society to inform the public in regard to proper means of securing good water. At a city election held in Boulder shortly after these meetings the vote was overwhelmingly in favor of the extension of the water-works. It is thought that the influence of the society was considerable in bringing about this result.

Francis Ramaley,
Secretary.

Boulder, Colo.,
December 22, 1905.

DISCUSSION AND CORRESPONDENCE.

MENDELIAN INHERITANCE AND THE PURITY OF THE GAMETES.

The communication on the above subject by my friend and colleague Professor Morgan, printed in the issue of Science for December 29, offers an ingenious if somewhat complicated interpretation of Mendelian inheritance that is at variance with the current conception in that it substitutes for a disjunction of allelomorphic characters in the gamete-formation a reversal of dominance in half the gametes (which evidently involves some kind of disjunction of the factors that determine dominance). That a complete elimination of the dominant character does not take place in the production of albinos (this character still being present in what Castle has called the 'latent' state) has been clearly recognized by several experimenters and is beautifully demonstrated by Cuénot's work on mice; but Professor Morgan's attempt to find a general basis for the explanation of this is a new and interesting contribution to the subject. I think, however, that his effort to explain the very case (that of Cuénot's yellow mice) that suggested his new interpretation, involves a negation of Cuénot's experimental results. This observer found that yellow was invariably dominant to all other colors, but that after crossing yellow mice with pure-bred grays (or other colors) the yellow mice of F_2, contrary to his expectation, included no pure extracted dominants, nor could such a race be obtained from them. In order to explain this, Cuénot advances the hypothesis that the yellow-bearing gametes are sterile to one another or do not unite, *i. e.*, a selective fertilization occurs, such that the yellow-bearing gametes are fertilized only by those bearing other colors. The interest of the question in relation to sex-production (which I have discussed in a paper now in course of publication) leads me to offer a word of criticism, since I am unable to share Professor Morgan's belief that his assumption will take the place of Cuénot's hypothesis. This belief rests, I think, on a misconception of Cuénot's results regarding the behavior of the yellow mice of F_2 which possibly arose through a confusion of Cuénot's formulas with his statement of fact.

How was the constitution of these mice tested? As in all similar cases, by the nature of their offspring, and Cuénot clearly specifies two methods by which the test was applied,

namely, (1) by pairing the yellow mice with each other, and (2) by crossing them back with pure grays, blacks or browns. In either case, pure or homozygous F_2 yellow mice (or in Professor Morgan's view those that contain only 'latent' as opposed to 'free' gray) should give only yellow offspring, owing to the uniform dominance of yellow, while mixed or heterozygous yellows (yellow mixed with 'free' gray) should produce grays or other colors as well as yellows. Cuénot says: 'J'ai essayé par l'une et l'autre méthodes un nombre considérable (81) de Souris jaunes * * *.' 'Or, a mon grand étonnement, je n'en ai pas trouvé une seule (*i. e.*, homozygote).' Professor Morgan considers this statement, which embodies the principal result of Cuénot's experiments, as 'somewhat ambiguous,' apparently for the reason that Cuénot does not in this passage actually use the words that all the yellow mice produce offspring of other colors as well as yellows; but it must be obvious that only such a result could justify his statement, and if this be not Cuénot's meaning I am unable to discover any meaning in his paper. In point of fact, however, he states specifically on a preceding page (cxxvii) that the cross between a yellow mouse and a pure-bred one of a different color always gives offspring of this color (gray, black or brown) in addition to yellows, the numbers being stated, in the case of the yellow-gray cross, to be equal.

Now, according to Professor Morgan's assumption there should on his own showing be two classes of yellow mice in F_2, of which "the first group $CY(CG)$ (*i. e.*, those containing 'latent' gray) will breed true, the other group $CY(CG)(CY)CG$ (containing 'free' gray) will split up in each successive generation according to the Mendelian formula." Such a behavior of the F_2 yellow mice was precisely Cuénot's expectation, but 'to his great astonishment' it was contradicted by the results. Professor Morgan, nevertheless, insists that the case of the yellow mice is precisely similar to that of extracted gray dominants (both being 'contaminated' by the recessive character in the latent condition) though, as Cuénot was the first to show, the latter breed true save for the rare appearance of a different color, such as black, probably derived from the latent color of the original albino used. If the two cases do not differ, why was so experienced an observer as Cuénot astonished at his results, and why did he go so far out of his way to construct the special hypothesis of selective fertilization to explain the behavior of the yellow mice as distinguished from those of other colors?

The difficulty with Professor Morgan's explanation is that it proves too much, for it explains the special peculiarities of the yellow mice out of existence (!). My criticism is not directed against Professor Morgan's general assumption, but I think that it entirely fails, as far as he develops it, to account for the peculiarities of the yellow mice, and that it leaves Cuénot's hypothesis, which it is supposed to obviate, exactly where it stood before.

E. B. WILSON.

THE LOGICAL BASIS OF THE SANITARY POLICY OF MOSQUITO REDUCTION.

THE excellent address of Sir Ronald Ross under the above title published in the December 1 number of SCIENCE, states the general rules regarding mosquito distribution with great accuracy; but it applies only to certain species, including the *Anopheles* so far as known to me and to *Stegomyia fasciata*. It does not apply in the least to such forms as *Culex cantator* and *C. sollicitans*. None of the suggestions as to erratic flights that practically restrict the distance traveled influence these species, which are truly migratory and are guided by some motive other than finding food or a place to breed. In fact, as I have shown, these migrants never propagate their kind and where they are to be dealt with, all of the carefully reasoned mathematical deductions fall. The matter is of great practical importance in New Jersey where communities within whose boundaries not a mosquito breeds, nevertheless, sometimes find life a burden because of the insects. Local work in such cases is worse than useless. When we find the dominant mosquitoes in the Orange Mountains to be species whose nearest breeding place is on Staten Island, time and money