

DISCUSSION.

SUBSURFACE PLEISTOCENE OF LOUISIANA:

by John W. Frink.

(In "THE SAND AND GRAVEL DEPOSITS OF LOUISIANA" by T. P. WOODWARD and A. J. GUENO) *La. Geol. Survey, Bull.* 19, pp. 367-420, April, 1941.

THIS paper is appended to a detailed, essentially economic report as a separate contribution. On the basis of a great many well records and a few Schlumberger logs, Frink appears to separate all the post-Miocene sands and gravels in Louisiana into five "formations" the uppermost of which is Recent and the other four of which are accepted from Fisk¹ as representing specific interglacial stages of the Pleistocene. Furthermore, Frink correlates the buried gravels directly with the terrace surfaces described by Fisk, and therefore claims to offer subsurface proof of Fisk's hypothesis of the stratigraphic unity and interglacial origin of each terrace, separated by erosional periods correlated with stages of ice advance. Frink's subsurface data also appear to support the sedimentary-load hypothesis for the origin of the Gulf Coast geosyncline set forth by Russell² and others.³ Any short paper which apparently has direct bearing on such diverse major ideas as eustatic sea-level fluctuations, Pleistocene stratigraphy of the Gulf Coast, and the origin of geosynclines invites critical attention.

A prime cornerstone of Frink's conclusions illustrates a lamentable confusion between fact and opinion. The whole correlation (after Fisk)⁴ is based on the statement (p. 409) that "At the outset of this paper the subsurface gravels were assumed to be Pleistocene in age." He (p. 370) "feels that arguments presented by Fisk, Chawner and Roy have sufficiently proved that these deposits are not Pliocene as they have formerly been called." It is pertinent to

¹ Fisk, H. N.: 1938, "Geology of Grant and La Salle Parishes," *La. Geol. Survey, Bull.* 10, pp. 13-75, 149-174.

—: 1939, "Depositional terrace slopes in Louisiana," *Journal of Geomorphology*, Vol. 2, No. 2, pp. 181-200.

² Russell, R. J.: 1936, "Physiography of the Lower Mississippi River Delta," *La. Geol. Sur., Bull.* 8, 1-195.

³ Howe, H. V.: 1936, "Louisiana Petroleum Stratigraphy," *La. Dept. Cons., Bull.* 27.

Barton, D. C., Ritz, C. H., and Hickey, H.: 1933, "Gulf Coast Geosyncline," *Bull. A. A. P. Geol.*, Vol. 17, No. 12, 1446-58.

⁴ Fisk, H. N.: 1938, "Geology of Grant and La Salle Parishes," *La. Geol. Sur., Bull.* 10, Fig. 13, p. 171.

examine in some detail the "arguments" which constitute "sufficient proof" for this assumption.

In the works referred to by Frink, we find that Fisk concludes a discussion of the (Citronelle and correlative) gravels with: (p. 154)—"Since there seems to be no logical reason for attributing the age of the gravels to the Pliocene, the writer is in accord with Chawner who, believing the gravels indicated the special conditions of the Pleistocene, assigned them to that time." Chawner (p. 136) admits that "the Citronelle gravels of Mississippi and Louisiana are older than the deposits on the Pleistocene alluvial terraces which Matson described, but," as he states, "much happened during the Pleistocene on the Gulf Coast and the Citronelle is not necessarily Pliocene because it is older than these terrace deposits." He continues: (p. 137) "It is clear that special conditions did exist during the Pleistocene, when heavier rainfall and the melting of glaciers fed torrential amounts of water into the drainage channels. Great gravel deposits in the upper Mississippi Valley were produced by such torrents of water and it is not too much to expect that the influence of these torrents was felt down in the lower valley of the Mississippi. After a perusal of the literature on the Citronelle and for the reasons given above, there seems to be much better reason for assigning the Citronelle to the Pleistocene than to the Pliocene."

The arguments of Roy referred to by Frink are not clear. He reviews the Citronelle and possibly correlative deposits, indicating considerable controversy in the literature. Roy (p. 1559) "is convinced that the principal difficulty in the terminology of these deposits is the lack of detailed mapping in wide areas." He suggests (p. 1559) "that the term Citronelle be dropped" with the observation that "it seems useless at this time to try to apply any formational names to these deposits." Roy's conclusion is that (p. 1559) "What seems to be a desirable solution to the question of names for these deposits will appear soon in a bulletin of the Louisiana Geological Survey by Fisk" (the same report referred to by Frink).

In view of these opinions Frink's conclusions seem to the reviewer to indicate a high degree of mental carelessness. His assumed "proof" is founded upon Fisk's correlation which in turn leans upon Chawner whose strongest statement is simply one of personal preference. Since Roy's conclusions refer back to Fisk, we have the reported Pleistocene age of the Citronelle based upon one personal opinion, restated without proof by two later writers, and therefore accepted as "proof" in Frink's critical paper. The reviewer is quite convinced of the possibility of Frink's assumption, but he feels that any attempt to countermand correlations accepted by the U. S. Geological Survey and many other authorities must await full and incontrovertible field evidence.

The body of Frink's work is concerned with well records in such number that ten cross sections of Louisiana are presented, apparently showing the structure (both north-south and east-west) of the post-Miocene (Pleistocene?) gravels and sands. Although Fisk recognized four separate terraces, namely Williana, Bentley, Montgomery and Prairie in order of descending elevation, Frink's data show that in no case can any buried gravel bed be demonstrated as Williana. However, the upper three gravel units are confidently identified as representing buried homoclinal layers, the landward exposed edges of which form the Bentley, Montgomery and Prairie terraces of Fisk.

The precise manner in which these important identifications are made is subject to question, as Frink frankly admits. He points out the notable unreliability of well logs, even for established formations, but especially for so-called "surface sand and gravel." He also properly notes (p. 373) that "gravels may be recorded at depths of several hundred feet below where they actually occur"; that "these deposits were lenticular and highly irregular," that "it is impossible to correlate individual beds of gravel over any great distances." He concludes, "In fact, it is rare that one can trace one bed from one well to another in the same field."

In spite of these and many other clearly admitted weaknesses of the method, we find that (p. 375) "after all of the wells were located on a map of the State an attempt was made to contour the base of the Pleistocene." "Considerable difficulty was encountered" "Contours were drawn on the deepest gravels found." The reviewer feels that a more accurate name for the horizon sought would be the "top of the Miocene." In one specific case Frink reports (p. 375-6) "the deposits cannot be considered Miocene because they are well above the first Miocene fossils" (encountered). "Therefore," he concludes, "it was evident that the gravels in these two wells were Pleistocene; and, although they could be contoured after a fashion, it was impossible to derive any orderly structural concepts from contours." It is apparent that the possibilities of such gravels being (1) unfossiliferous Miocene, or (2) Pliocene, were completely ignored.

After noting the serious difficulties encountered in identifying the top of the Miocene (base of the Pleistocene?), delimiting the thick (as many as 4000 feet) gravels, notably lenticular and irregular, into three or four distinct "formations," and correlating these buried beds laterally over 200 miles to correspond with the terraces of Fisk, Frink (p. 378) states that "the gravel recordings showed conclusively that the subsurface gravels were laid down not as one unit, but as at least three, and that the base of each unit could be traced." Also (p. 374) that "the final correlations, although

approximate, were easily discernible, and that the picture of the Pleistocene deltaic deposits is much clearer than was anticipated." Admitting (p. 379) that "it should not be possible to trace a definite bottom to the younger gravels," Frink continues—"The fact that this can be done certainly shows that the deposits are due to more than one period of deposition." "Therefore, Davis' theory of one period of deposition must be erroneous, and Fisk's theory of alternating erosion and deposition appears to be supported by subsurface evidence." These rather strong assertions, the reviewer would emphasize, are based upon reconstructed cross sections of which Frink acknowledges (pp. 399-400), "A solid line in these sections is about equivalent in probability to a dashed line in any other work." "A dashed line in this case is largely hypothetical." The reader will be justified if he proceeds slowly in accepting far-reaching conclusions founded upon unproved assumptions and buttressed by a mass of data heavily weighted with the personal equation and, by their very nature, of questionable reliability.

It is notable that Dr. H. V. Howe, Director of the Research Division of the Louisiana Geological Survey, prefixed a note to "Subsurface Pleistocene of Louisiana" stating (p. 369) in part: "The reader should understand that the opinions expressed in this speculative article are those of the author, John W. Frink, rather than those of the Louisiana Geological Survey." In spite of this note of caution, the Survey has apparently officially accepted the new classification as the standard for their post-Miocene geologic column. In the large report on sand and gravel deposits preceding Frink's paper the stratigraphic column (p. 25) is taken directly from a very recent report by Fisk⁵ in which the Pliocene is shown as absent and gravels of each of the four terraces named by Fisk have been elevated to formation rank and correlated with the standard interglacial stages. Since both Fisk and Roy had vehemently denied the justification for formation names for the post-Miocene gravels (in 1939) Fisk reported (*ibid.*, p. 175) that "since then the terraces have been mapped in detail to the north in Winn and Caldwell Parishes and the generalized position of the surfaces through the State has been outlined." "Because the terraces can be mapped over wide areas, the writer (Fisk) now proposes that they be ranked as formations." It is interesting to note that the stratigraphic descriptions of the new "formations" in Fisk's late report carry citations of Frink's work (under review) as confirmatory evidence for the change in status. This reviewer has difficulty in visualizing the Survey's justification for acceptance of the change in the geologic column, even though it be officially credited to Fisk, in view of the

⁵ Fisk, H. N.: 1940, "Geology of Avoyelles and Rapides Parishes," *La. Geol. Sur., Bull.* 18, Fig. 26.

doubtful validity of the "proof" reported by Frink in the paper being reviewed.

It is clear that geomorphic interpretation of well records has already been attempted with considerable success by Barton, Meyer and others, and that the method has wide possibilities in the Gulf Coast region. Knowing the chances of error, however, one must hesitate to accept hypotheses of wide application and great geologic importance in any study which has clearly failed to pay meticulous attention to the separation of *fact* from inference, assumption, or previously published (but yet unproved) opinions, no matter how many times they have appeared in the literature. In the present paper the critical reader is confronted with such unscientific construction that not only is the author's conclusion in jeopardy, but doubts are raised as to the validity of previously published hypotheses, "proof" for which is based on the new findings.

Doctor Howe's precautionary note appears to have been fully justified.

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