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ORIGINAL ARTICLES.

The Jordan Valley and Judean Highlands.

By Professor BAILEY WILLIS and Dr. LEO PICARD.

THE nature of the faulting which made the Jordan Valley and separated it from the Mediterranean by the Highlands of Judea has been discussed in a memoir by Professor Bailey Willis, "Dead Sea Problem: Rift Valley or Ramp Valley?" *Bull. Geol. Soc. Amer.*, xxxix, 1928, 490–542, and by Dr. L. Picard, *Geological Researches in the Judean Desert*, Jerusalem, 1931, 108 pp., 3 pl., and map.

Both authors further discuss the problems in the following papers.

I. FAULTS IN THE JUDEAN HIGHLANDS.

By Professor Bailey Willis.

The valuable contributions to the geology of Palestine—*Geological Researches in the Judean Desert* and *Tektonische Entwickelungsphasen im nördlichen Palästina*—by Dr. L. Picard show from the mass of facts he has recorded that he follows in the steps of his great forerunner, Dr. Blanckenhorn, and closely adheres to his reasoning regarding structural developments.

We are entirely in agreement in regard to the relative movements of the Judean plateau during the Upper Pliocene. The stratigraphic and physiographic evidence of uplift is quite convincing. That is, at least, a step toward a common interpretation of the causes of the displacement. The plateau has been raised with reference to sea-level, not merely relatively as contrasted with the Dead Sea trough.

Dr. Picard's *obiter dictum* that a theory of the faulting must stand or fall by the proof of the existence of the faults unfortunately does not hold if, as follows from the context in his statement (*Judean Desert*, pp. 93–4), the proof must be that of direct observation.

He says that " I could discover no visible evidence of the ramp-faults ". It would be somewhat surprising if anyone had found the outcrop of the major ramp, which should emerge where the Palestine block rests against the Dead Sea block, deep beneath the waters of that sea. A ramp is a shear which originates in *deep-seated* rocks. If it curves upward and emerges at the surface it raises a mountain or plateau, whose escarpment is too steep to stand. The foot of the escarpment is immediately covered by land slides and these become of great volume if the upthrusting reaches great height, as is the case along the Dead Sea.

Similar slides would occur on the face of a rift-fault as on a ramp fault, and the direct evidence of faulting must in either case be obscured. Recognizing this inevitable condition, I concluded that : " The examination of the Dead Sea trough and its immediate environment on the west led to no definite conclusion, as to the origin of the displacements " (*The Dead Sea Problem*, B. W., 1928, 516).

The very obvious slides at the base of the cliffs that retreat from the Dead Sea were discussed by Rift and Ramp (op. cit., p. 513, where the theoretical structures are thus personified), without much satisfaction to either, but I am inclined to think that Ramp's explanation is strictly in accord with the evidence.

The next item which should logically engage attention is the " El-Muntar " fault. Dr. Picard has overlooked my references to it (op. cit., p. 517). If Mr. Geo. S. Blake, who pointed it out to me, named it, the name escaped me. I traced the fault, however, and satisfied myself that it is a feature of the flexure to which I gave the name Jericho flexure. As far back as 1873 Powell described the occurrence of a similar fault and its passage into a monoclinal fold (Powell, J. W., " Geological Structure of the Country North of the Grand Canyon of the Colorado," *Am. Jour. Sci.*, **3**, v, 1873, 459–6), and the relation is a common and logical one. The bending induces stretching and the stretching results in *normal* faulting. This being recognized, Dr. Picard and I are entirely in agreement as to the location of the El-Muntar fault, and we will agree to call it normal. But we do not necessarily mean the same thing by " normal ".

A normal fault (as I use the term) is one which hades to the apparent downthrow (*Geologic Structures*, 1929, 67). One way in which such a fault may be produced is by gravity, which causes the downthrown block to sink with reference to the other. That I would distinguish as a gravity or rift-fault. The same effect, however, may be, and often is, produced by the upthrust of the upthrown block with reference to the other. The force involved is an anti-gravity force, which displaces the upthrust block and its foundations. The structure may indicate deep-seated ramping, and the fault itself might be described as a minor ramp.

Since we agree that the Palestine plateau has been raised, and

since the El-Muntar fault is the kind of fault that ought to develop in consequence of the upthrusting, it would seem reasonable to Ramp that Rift should agree to that interpretation also.

It would be most gratifying if one could find conclusive evidence in the mere mechanical relations of faulting to answer those questions, but it is unfortunately true that any high-angle fault can be explained indifferently as a gravity slip or an upthrust and similarly any low-angle fault may be explained as an overthrust or underthrust, provided that it is considered by itself. Only when interpreted as part of a general movement can it be classed one way or the other. In the case of the El-Muntar fault the general movement of the plateau was one of uplift and the El-Muntar fault is an upthrust if it occurred during that movement.

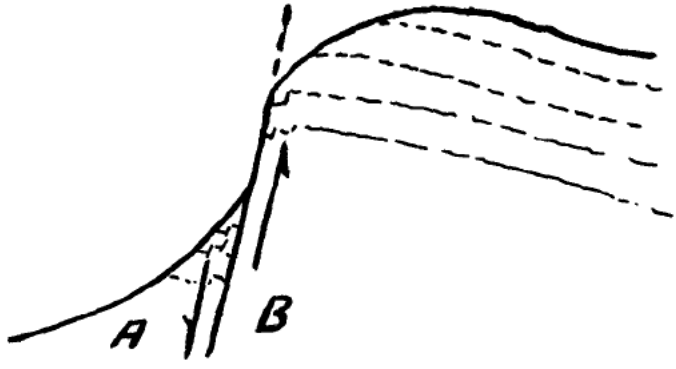

FIG. 1.—Fault of the El-Muntar Type.
Did *A* slip down or was *B* pushed up? Is it a Rift-fault or a Ramp-fault?

Turning now to the overthrust, upthrust, or gravity fault which traverses the escarpment of Mount Carmel above Haifa, there appears to be closer agreement between Dr. Picard and myself than the reader might gather from the statement of the case in his *Geol. Researches in the Judean Desert*, p. 94. I translate his description of the Carmel fault (*Entwickelungsphasen*, p. 173) :—

"The Carmel line, that is the fault along the north-east face of Mount Carmel above Haifa bay, goes back into the older Neogen, as most of the Erythraic faults do. It strikes N.N.W., as the Megido line does, and shows evidence of a peculiarly strong upward movement of the south-west block of Mount Carmel during the entire post-middle-Pliocene time."

Continuing the description, he refers to the marked disturbance of the strata : "There is here, as in Mount Gilboa, a general south-west dip of the strata and occasional marginal bending, accompanied

by steeper dips and by stretching in the direction of the sunken basin, the plain of Haifa."

Our observations as to the general strike and dip agree closely, and we both noted evidence of disturbance which we attribute to faulting. Good.

Turning to *Researches in the Judean Desert* (p. 94), I quote further :

" The only real instances of ramp-faults occur, according to Willis, on the Carmel and at Gilboa. There, indeed, he considers it a case of overthrusts." " The occurrence at Jadpur is by no means certainly Upper Cretaceous ; we have been able to demonstrate that even Middle Pliocene lies there, adjacent to chalk-like rocks. The rocks there, then, are of most diverse age, from Cretaceous to Upper Tertiary, which, like the same phenomena in the Rhine Valley, stand out as blocks at the edge of the mountains or in the plain."

My own statement of the relations which I observed was (*Dead Sea Problems*, p. 519) : " Four miles from Haifa, at the cement quarries of Yabor, there is a small spur consisting of the Senonian, which stands vertically, striking north-west and south-east, a little way up from the valley. The outcrop of the vertical strata can be traced for several hundred yards along the mountain-side and through a portion of that distance the Senonian is exposed in contact with the Turonian-Cenomanian, which forms the higher slopes."

Jadpur and Yabor are the same. The quarry is opened near the level of the plain, where the occurrence of Middle Pliocene beds would neither excite surprise nor bear upon the question of faulting. We are agreed that a fault does occur higher up and that the " rocks are of most diverse age ". At the place which I described I observed certain hard limestones, that I regarded as Turonian, dipping gently south-west and overlying vertical chalk beds, that I took to be Senonian. Quite apart from the question of identification of the horizons, which the discovery of fossils in the beds in contact might prove to be other than I inferred from the characteristic lithology, the relations of the strata demonstrated the existence of a thrust fault. I regarded it as an overthrust, dipping about 30° to the south-west. It might possibly be an upthrust, a real minor ramp, and in that case could be interpreted by Rift as a vertical gravity fault, if one disregarded the fact that the fault " shows evidence of a peculiarly strong upward movement of the south-west block of Mount Carmel ".

Referring to the fault at Gilboa, which I took to be an overthrust because of the crushing of the strata at Herod's Well, Dr. Picard says (op. cit., p. 95) : " In both cases, Gilboa as well as Carmel, we are dealing with faults, respectively faults accompanied by drag and bending of the strata at the boundary of the dislocation." I presume that Dr. Picard uses the word " fault " in this context to mean a normal fault, and with the interpretation of a gravity fault. I saw no reason to assume the occurrence of a fault of that type or of an upthrust, since the mountain slope is too gentle to

suggest a fault scarp. I agree, however, that there is a fault along the base of the mountain, using the word " fault " to include thrusts.

I find a little difficulty in making that recognized faulting fit in with Dr. Picard's statement in the same paragraph that : " The strata themselves follow quite normally upon one another, with a slight pervading dip to the West." I can only infer that in the eagerness of controversy he has failed to describe with adequate precision the difference between the place where he recognized faulting and that where he observed normal sequence.

Again, citing my inference that Gilboa is a locality where " Lower Cretaceous is underlain by Upper Cretaceous ", he says : " Out of these strata I was able to identify indisputable Nummulites of the Eocene." Out of which strata ?

The occurrence of Eocene in the strata of the Besan plain would not be unexpected, but would have no negative bearing on the faulting. Their occurrence in the mountain slope, above the recognized fault, might indicate some complication in the zone of faulting. But so far as I was able to observe, the slope of Mount Gilboa is heavily covered with soil, especially along the base where the critical evidence must be sought, and the occurrence of the crushed limestone in the cave at Herod's Well is the only significant exposure in that vicinity. I find myself at a loss to understand Dr. Picard's account.

I regret to call attention to some other misinterpretations of the text of my article, " The Dead Sea Problem." For instance (p. 95 of *Geological Researches in the Judean Desert*) :—

" Willis maintains that our meridional and Somalic lines of disturbance belong to one and the same structure. We refer the reader here to our investigations which establish for each group of dislocations differences of age, different direction, and totally different manifestations of structure."

The question what constitutes a " system of structure " is one of usage. Conjugate joints, for instance, are commonly simultaneous. Chronologically they belong to one system, but since there are two series of parallel joints it might be said that they constitute two systems if classified by direction. Again, it is impossible to compress a body in one direction without extending it in another, unless there be volumetric change. The different effects of compression and tension may be classed arbitrarily as belonging to different systems, but they are, nevertheless, effects of a common cause, and a single episode. Or, again, it is well established that movements on folded or faulted structures have progressed during long periods or even been repeated many times with intervals of stability. Yet it is one structure. Thus differences of age, different direction, and different manifestations of structure may consistently be included in one and the same system. Sub-classifications and distinctive names are apt to be confusing and misleading.

I write of the system of structures in Palestine as one system

because the different features are effects of related stresses, have had a fairly continuous development during the last few million years, occur within a geographical unit of rather small dimensions, and are probably due to a persistent cause.

On one fundamental point I must not fail to correct the impression (op. cit., p. 93)—" As to the causal aspect of the question how the Judean arch was uplifted, Willis assumes *magmatic forces* " (italics mine).

I have not been able to discover just where that misapprehension arose, but it piques my curiosity because it so sharply contradicts my firm opinion. I do not think " magmatic forces " could possibly have caused the uplift of the Judean arch, if the term be used in any sense consistent with the context in Dr. Picard's paper.

I understand magmatic force or forces to mean a force originating in the internal energy of the magma itself. I would distinguish it from a pressure originating in some condition extraneous to the magma but transmitted by the molten mass hydrostatically, after the manner of a hydraulic press.

Now, had Dr. Picard said : Willis thinks that magmatic forces, i.e. the internal heat and chemically active gases of a body of magma, situated at considerable depth beneath the eastern basin of the Mediterranean, stimulated the molecular forces of its cover and caused the solid rock to recrystallize in such a manner as to elongate horizontally and to push up the Judean arch ; if he had said that I would not take exception to the statement.

It does not appear, however, that he had that theory of orogeny in mind. From the fact that he searched for extrusions of basalt in the Palestine arch (where I certainly would not expect to find them), I infer that he had in mind a laccolithic intrusion as the cause of uplift ; at least, he attributed that idea to me. But it had never occurred to me and would be inconsistent with the mechanics of ramping in any case.

To sum up this quite inadequate discussion of Dr. Picard's interesting papers, it appears to me that :—

(1) We are agreed that the Palestine plateau was arched up during the Upper Pliocene. I would say the uplift is still in progress.

(2) We agree that the fault which bounds the Dead Sea trough along the base of the western escarpment cannot be seen. It may be either rift or ramp, so far as direct observation goes.

(3) We have both examined the El-Muntar fault, and agree that it is a *normal* fault, but he by implication classes it as a gravity fault, whereas I regard it as an upthrust.

(4) We have both observed the Carmel and Gilboa faults, and have noted the pronounced disturbance of the strata. He regards them as normal, gravity faults ; I would not be surprised if they proved to be normal, upthrust faults, but I think the tilted uplift of Mount Carmel (which he also recognizes) favours the alternative

of a relatively low-angle overthrust, curving up to a steeper dip toward the surface.

(5) I am not surprised that some of the chalk is found to be younger than Upper Cretaceous, but I am unable to recognize the bearing of that fact upon the character of the faulting, since the exact relations of the beds in which the fossils are found to the older strata across the fault are not described. In any case, the strata cannot "follow quite normally upon one another" where the marked disturbance of the rocks has led both of us to recognize faulting.

(6) The question of the term "system" in connection with a group of structures is one which we interpret differently, but we recognize the same sequence of events. Dr. Picard lays more stress upon details than I do in describing the movements. Ever since the days of Élie de Beaumont geologists have been prone to set up artificial groupings, as in the case of parallel structures, for instance, even though Suess demonstrated the continuity of winding directrices.

(7) As regards the original cause of the uplift of the Judean arch and related structures including the Dead Sea trough, Dr. Picard appears to have misunderstood my views. I, at least, do not recognize them as stated or implied by his text.

II. The Hypothetical Ramp-faults in Palestine.

By Dr. L. Picard, Department of Geology, University of Jerusalem.

I have read with pleasure Professor Bailey Willis' interesting paper, and refer to his comments in the same order.

How far I agree or disagree with older authors is, I hope, sufficiently indicated in my paper. I am glad to see that Blanckenhorn, who for many years was a strong holder of the pure "horst" theory of the Judean mountains, now also believes in their anticlinal structure.

It would appear that Professor Bailey Willis is of opinion that I did not understand the meaning of his ramp-fault, but I cannot discover that my explanation of ramp-faults on p. 93 differs much from his, and his figures and demonstrations are so lucid that even a beginner in tectonic study must understand them.

I fully appreciate his argument that direct observation of ramp-faults is difficult if not often impossible. For those geologists, however, who reside in Palestine and who have to deal, like myself in the last seven years, with its tectonic problems, it is important to work on such theories or hypotheses for which proof will be possible sooner or later. Professor Willis says that a ramp-fault and the direct evidence of faulting must in a certain case be obscured, and refers to the Dead Sea trough. But I ask, why should the whole zone of cliffs with its splendid outcrops between Ras-es-Feskha and

En-Gedi on the West or between Zerka Main and Arnon, W. Mojib), on the East, with several thousand feet of throw, be insusceptible of direct ramp observations, while Carmel and Gilboa with less throw provide good material for the direct observation of upthrusting? The rocks of Carmel and Gilboa are at their deepest outcrops between sea-level and — 80 m.; but the rocks of the Dead Sea can be observed down to — 400 m., the latter region presenting its well-known and remarkable incision on the surface of the earth.

In contrast to Professor Willis' pessimistic conclusion—"The examination of the Dead Sea trough and its immediate environment on the west led to no definite conclusion as to the origin of the displacements"—I believe, that if any solution of the rift problems can be obtained the investigation of the Dead Sea depression will play an important part.

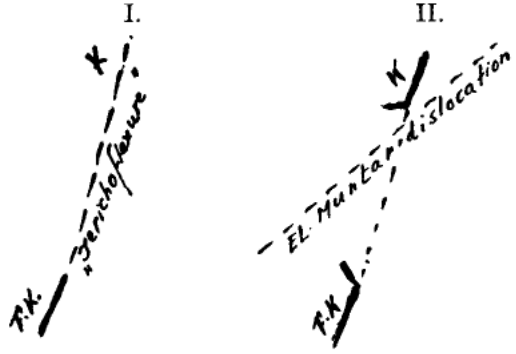

FIG. 2.—The Jericho flexure and the El-Muntar fault.

I = Professor Willis' Jericho flexure. II = my El-Muntar dislocation.
F.K. = position of Feshka-kumran crescentic fault. K. = position of Kuruntul fault.

With regard to the El-Muntar fault, which was determined by Blanckenhorn (1896) as a flexure, and which Professor Willis thought identical with his Jericho flexure, I had not overlooked his description on pp. 516, 517. Unfortunately, however, he brought together both structure-lines—that of the Dead Sea fault and Jericho flexure identified by him with the El-Muntar fault so "that the first is replaced by the latter".[1] He regards other monoclinal faults as similar; e.g. he says of the Es-Salt line: "The cliff retreats and becomes a flexure"; and of the Damascus line (p. 527): that "it died out in a monoclinal flexure, as is so often the case with similar thrust-faults". But the best proof that he had in mind one

[1] There seems to be some confusion here, since my map shows the fault to which I refer as quite distinct and further up the slope.—B. W., January, 1932.

and the same structural line is to be observed from his map. There the so-called Jericho flexure rises out from the Dead Sea cliffs and passes into the Kuruntul fault, thus combining both the crescentic faults.

The sketch above shows clearly that Professor Willis' Jericho flexure has a quite different direction and appearance in contrast to my El-Muntar fault.

Professor Willis' figure, which explains how one part of a monoclinal fault has slipped down or vice versa has pushed up, can easily be transferred to my section AB of the geological map. But we can only observe that the west part of the El-Muntar dislocation (e.g. Professor Willis' block B) always shows a strong puckering of the strata, indicating, therefore, dragging and down-warping of the strata *in* the direction of the trough-valley.

Howsoever we regard the El-Muntar flexure or fault (I myself called it a very big shear—or shift—fissure, see p. 84), it belongs to the series of Somalic (N.N.E.) lines which, as often pointed out,

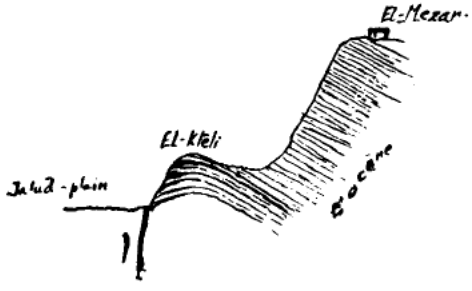

FIG. 3.—Section of Jebel-El-Kleli.

differ totally in structure from the big crescentic faults. The latter bordering the Dead Sea belong to the system of the meridional main rift-faults.

Carmel and Gilboa are the part of the country in which Professor Willis claims to have found evidence of ramp-faults. As regards their structures, I refer to my sections of the geological maps (*Kischonebene*, Haifa map 1928, *Besanebene* 1929). I am sorry to say again that—in spite of a thorough survey—I could not observe upthrusting or overthrusting. My map of Jadpur and description (1928) give enough details to explain my interpretation of the border of Mount Carmel at that place. I repeat that the blocks bordering the fault-line do not consist only of disturbed Cretaceous, but are mainly Pliocene. As regards Professor Willis' explanations, I cannot imagine how the Pliocene has been upthrust or overthrust by the older Cenomanian. I fully appreciate that an upthrust is not an overthrust. (See my remarks on p. 94.) I followed Professor Willis' own description of fig. 1, which bears the

title " Overthrust ", and pp. 518, 519, where he speaks further of " overthrusting " or " overthrust " in the Carmel ; and p. 520, where he wrote with regard to Carmel and Gilboa : " The outcrop . . . indicates a high-angle *upthrust fault* or ramp."

With regard to Gilboa, I could not in my recent paper go into much detail and repeat all the facts described or figured already in my *Geologie der Besanebene* (1929)—to which the reader is referred. Professor Willis asks : " Out of which strata are the Nummulites ? " The reply is : from the strata which he figured in his fig. 1, plate 18 (*Dead Sea Problem*, 1928). This mountain is called Jebel-El-Kleli. To avoid misunderstanding I give again a sketch of the locality.

On the ascent of El-Mezar the strata are seen to follow normally one upon the other. The nummulites are distinctly Middle Eocene of Mokattam character.

With regard to the questions of chronology of structure-lines : There was no doubt in my mind that north–south main (border) faults were not originated by a single action ; and I mentioned also in my paper that earthquakes along these faults, especially along Somalic (N.N.E.) lines, clearly demonstrate the existing continuation of the movements. Nevertheless, the crescentic faults which I have separated into two phases—an older phase, the north–south or meridional faults, and a younger phase or transverse faults (45 degrees)—may possibly have been formed by a single action. I am not able to decide this question, and it remains a matter of opinion. The Somalic or N.N.E. lines, however, are for me the expression of a younger movement. In my paper dealing with Northern Palestine I have given many paleogeographical reasons for considering that the origin of these lines is not older than Upper Pliocene. This N.N.E. direction is, because young, the best preserved and most striking tectonic feature not only in Palestine, but also in Syria ; and it is certainly connected with the latest orogenic movement of the Mediterranean mountain chains.

These reflections bring me to the last point : the causes of the upwarping of the Judean arch and the causes for the origin of the trough. I understood by " magmatic forces " what Professor Willis describes on p. 6 of his paper as " the internal heat and chemically active gases of a body of magma, etc., etc." I am therefore grateful to him for his correction of an expression which may have given a wrong impression to the reader. Nevertheless, regarding the tectonic causes, I repeat, as I remarked in my memoir, that the cause of the uplift is not here (i.e. in my paper) investigated : " A solution cannot be attempted until we are quite clear about the structural manifestations."

Summarizing the discussion :—

To solve the question of Professor Willis' theory—that in the structure of Palestine ramp-faults play an important part, the main

point, namely the existence of ramp-faults, still remains unproved in Palestine. I am sorry to have to abide by my conclusion on p. 105 of my book that there is " no certain evidence of ramp-faults."

My reasons are :—

(1) The Dead Sea faults give, according to his own words, no definite conclusion.

(2) The Mount Carmel upthrust-fault or overthrust as the case may be, is stratigraphically unproved. Also a thorough survey of that region indicates tectonics quite different from those characteristic of " ramp-faults ".

(3) The Gilboa north-west border has for the same reason as in (2) no ramp character. A totally normal succession of the strata from the very beginning at the foot up to the top of the mountain can be observed. There are no outcrops of older rock (Cenomanian-Turonian, Lower Cretaceous). For points (2) and (3) I again refer to my description and geological maps of Carmel and Gilboa (1928–9).

With regard to the different origin in time of the dislocations I believe strongly that the crescentic faults are of older, the Somalic lines of younger, origin.

The Ballantrae Igneous Complex, South Ayrshire.

By D. Balsillie, F.G.S., Royal Scottish Museum.

(PLATES V–VII.)

Introductory.

In the early days of the Geological Survey of Scotland, the rocks of the Ballantrae Igneous Complex were thought to be of metamorphic origin. Thus in the memoir explanatory of sheet 7, scale 1 inch = 1 mile, published in 1869, it is stated that the chief interest attaching to the altered strata of this district consists in the fact that they exhibit certain arrested stages of metamorphic action. But less than ten years after that date the erroneous character of this opinion was made abundantly clear by Professor Bonney. Equipped with experience gleaned in the Lizard, Bonney quickly recognized in Ayrshire a correspondence in the nature of the phenomena. Excellent descriptions were furnished by him of some of the lavas, serpentines, and gabbros, and their true igneous character emphasized.[1]

Lapworth's discovery of a Middle Arenig fauna in shales associated with volcanic ash, exposed upon the shore under the Bennane Head, was the next important advance in knowledge of the Ballantrae

[1] " On the Serpentine and Associated Igneous Rocks of the Ayrshire Coast," *Quart. Jour. Geol. Soc.*, xxxiv, 1878, 769.