fects in illustrations and in statements (such as that which eliminates Paneth's cells from the duodenum), but the feature of this edition is its embryological treatment.

Professor Stöhr believes that histogenesis is too imperfectly known to be included in a text-book of histology, and that morphogenesis is there out of place. Such figures and embryological accounts as I have included he draws and presents in lectures on systematic anatomy. The reviewer in SCIENCE, however, believes that the idea of embryological arrangement is excellent, but that it has not been properly carried out. Thus he notes that the formation of the germ layers is not described in human embryos, although he does not state that human material is not yet available. If the chick and pig are referred to when similar human embryos have been described, it is because the student uses the former in the laboratory.

Another criticism is the failure to recognize American investigators, who are seldom referred to by name, and who, it is said, are "ignored" or "apparently unknown." Many of the papers cited, as those of Bardeen, MacCallum, Hendrickson, Calvert, Bensley, Opie, and Flint were re-read by the editor immediately before writing the corresponding sections of the book. It has been Professor Stöhr's practice to omit personal references, which he believes are out of place in an elementary histology. To do justice the book should teem with such references. The considerable number which I have introduced refer to very recent, or to important controverted work. A student should always have access to the memoirs, but whether or not they should be listed in an elementary text-book is questionable. Since the reviewer in SCIENCE believes that acknowledgment should always be made, it seems unjust to him that Professor Stöhr should have modified his diagrams of the spleen and lung after the appearance of Professor Mall's and Professor Miller's work, respectively, without recording his acknowledgments. I am informed that Professor Stöhr some time ago wrote to the publishers that he had examined Dr. Miller's papers and used them as far as they seemed right to him, and that the diagram was mostly drawn according to Miller.

Some microscopic discoveries may be readily verified. Such was Professor Sabin's finding of the jugular lymph sac in mammals, so obvious a structure that I have a drawing of it made by a student some years before her paper explained its nature. In the text-book this sac is described but its discoverer is not recorded. Other findings, like those of the splenic lobules and units and of the atria of the lung may perhaps be verified after careful study by special methods. If neither the author nor the editor of the book is sure that he can identify the atria, he can not honestly describe them.

Professor Mall's researches on connective tissue which are thought by the reviewer to have received insufficient attention, are referred to on pages 39, 42 and 50 with accompanying figures. Altogether it is quite probable that German work is less fairly treated than American in this text-book, but the national element was not and should not be considered.

This edition of Stöhr's "Histology" was written to assist teachers in using the embryological method of presenting the subject. It is hoped that any teacher who is interested in such a method will examine the book.

FREDERIC T. LEWIS

CAMBRIDGE, MASS.,
July 27, 1907

SEISMOTECTONIC LINES AND LINEAMENTS—A REJOINDER

IN the issue of SCIENCE for July 19, 1907 (pp. 90–93), Professor William M. Davis has reviewed my recent paper, "On Some Principles of Seismic Geology," published in Gerland's "Beiträge zur Geophysik" in March last (vol. 8, pp. 219–292). To his statement that "the seismotectonic lines seem, so far as earthquakes are concerned, to be largely influenced in location and direction by the evidently subjective element of the location of cities and villages in which observers are numerous," I would say, that some modification of the results unquestionably arises from this cause, and this is true of all studies in seismic geography, as is fully set forth in my report. That it has not exercised a controlling influence upon the results, a careful reading of the report should show. Were this not the case, why should New York City, with its population of more than 3,000,000, be represented by nine epicenters, and East Haddam, Conn., by 145? why should Philadelphia have seven epicenters, and Newburyport, Mass., 84? Does it seem likely that in all southeastern New Jersey the little hamlet of Toms River should have been singled out for seismic prominence; in eastern Maryland, Accomac; and in the eastern Carolinas, Snow Hill?

When Professor Davis says: "Indeed, there is even less reason for thinking that seismotectonic lines should be closely related to centers of urban population than that rivers should run by large cities," he is attaching his handle at the wrong end. There is an excellent and most obvious reason why large cities should be located along the course of rivers, and there is an equally potent reason why seismotectonic lines should generally intersect large towns provided seismotectonic lines are expressed as lineaments. The seismotectonic line, like the lineament, and the proverbial horse, should come before the city and the cart, respectively. The relation of seismotectonic lines to cities has been discussed in my report on page 225.

The second portion of Professor Davis's review, which is headed "Fault Scarps and Fault-line Scarps," from the manner of its presentation would give the impression that it is in opposition to my own view, and it will doubtless be generally so interpreted by those not familiar with the paper under review. Stripped of some verbiage (baselevels, cycles of erosion, revived erosion, etc., with which the matter has little to do), the discussion might well have been taken from pages 254–255 of the paper reviewed, where I had supposed that the matter was presented in a somewhat new light. No possible objection can be raised to Professor Davis's borrowing of this idea and adopting it, but I should not like it to be supposed that the view is not also my own.

The subject of the straightness of fault lines and lineaments has been taken up in my report along the line of Professor Davis's discussion of it (pp. 285–286), as it has also in my earlier papers; and, I venture to think, in a more nearly adequate fashion. Better than any discussion of this subject is a presentation of evidence. Early in the present season I suggested to Mr. W. D. Johnson, of the United States Geological Survey, then as now in the Owen's Valley, California, the great desirability of securing photographs, and if possible maps, of the earthquake faults which were formed there in 1872. In response to this suggestion Mr. Johnson has, with painstaking labor, prepared detailed maps covering considerable areas of the faulted region, and these with an unusual generosity he has placed at my disposal for study. These maps will shortly be published and will make, I do not hesitate to say, one of the most important of contributions to the science of seismology. The portion of one of these areas which is printed herewith, sets forth the complex nature of a zone of displacement; especially, however, its zigzagging course, its sudden

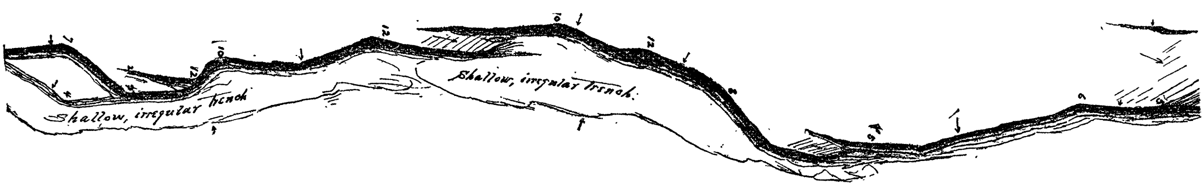

Fig. 1. Map of a zone of dislocation revealed at the surface after the earthquake in the Owens Valley, California, in 1872. Surveyed by Mr. W. D. Johnson, U. S. Geological Survey, in June, 1907. Scale, 240 feet equal one inch. The figures indicate throws, and the arrows the facings of the scarps.

variations in displacement, its distribution of the throw over several near-lying and generally parallel planes, and, finally, the general persistence with which the zone of dislocation adheres to a definite course.

The object of this reply is to make clear that with the exception of the minor differences above referred to, the theses which Professor Davis has defended in his review, are just those which I have myself set up in the report reviewed, as well as in some other papers upon structural geology.

WM. H. HOBBS

UNIVERSITY OF MICHIGAN,
July 22, 1907

RAILWAY SIGNALS

TO THE EDITOR OF SCIENCE: By some inadvertence Dr. J. W. Baird, of the University of Illinois, in criticizing a recent article of mine on "Railway Signals," in the *Century Magazine*, has attributed to me the belief that the human retina at night is color-blind; and he wonders how, according to my doctrine, an engineer ever distinguishes his color signals at night. As a matter of fact, I distinctly state, in the very article he criticizes, that at night the eye is *not* color-blind: "Colors are readily seen at night if they are intense enough."[1] The passage of mine which he quoted speaks explicitly of *faint* lights; for the signal-lights, bright enough in themselves, often become faint by distance, fog, smoke or storm. And of faint lights it is demonstrably true, as I said, and as every careful student of the subject knows, that the eye "no longer detects their proper colors."

2. As to the relative sensitiveness of the outlying portions of the retina for color and for form, it should be said that at a certain angular distance from the fovea a red danger-light can appear "white"—a common sign of safety. But in my own case I can easily distinguish correctly a horizontal from a vertical line, still farther off to the side. And even when, with greater angular distance, the direction becomes obscure, I find no tendency in a line-signal to appear to be its very opposite, as in the case of certain color-signals. So far as the practical problem of signaling is concerned, therefore, it seems probable that indirect vision would be less likely to cause disastrous misperception of a line-signal than of color; and that Dr. Baird's contention here is not *stichhaltig*.

3. The fact that some illuminated semaphores have failed would hardly seem to justify the judgment that what I recommend is "antiquated" and a failure. As I shall attempt to show elsewhere, there is an essential difference between the long line of lights which I propose for signaling, and the devices that have failed.

4. Dr. Baird charges me with promulgating the "erroneous conception" that there are individuals weak in their color sense but by no means color-blind; and declares that "several thousand cases of 'color-weakness,'" examined by Nagel, of Berlin, turned out in every instance to be color-blind. This is certainly astonishing. For Nagel himself, in the very latest issues of his journal,[2] affirms that he has found many cases of markedly weak color-sense that were *not* color-blind *at all*. He finds the color-weak to be usually "anomalous trichromates"; but quite recently he has examined carefully a person who showed in a pronounced way the characteristic marks of color-weakness (Farbenschwäche), and yet was not even "*anomal*." Except for the color-weakness, his color-system was the normal "three-color" system. The "popular" and "erroneous" conception that there are color-weak persons who are not color-blind, seems thus destined to continue.

It is the more striking that these misrepre-

[1] "Railway Disasters at Night," *The Century Magazine*, May, 1907, p. 120, col. 2.

[2] *Zeitschrift für Sinnesphysiologie*, Vol. 41, pp. 250 f.; Vol. 42, pp. 65 ff. Could Dr. Baird's "several thousand cases of 'color-weakness,'" all proved by Nagel to be color-blind, have perhaps been drawn from the following passage in Nagel?—"*Among many thousand persons whose color-sense I have investigated, I have found not a single instance of markedly weak color-sense that did not on closer examination turn out to be an anomalous trichromatic color-sense.*" (Ibid., Vol. 41, p. 251). It is perhaps needless to add that "*di*chromatic" would have been used by Nagel had he meant (even partially) color-blind.