an unprincipled person might make of the ascendancy gained over the subject. These warnings, frequently repeated, are not without reason, as the annals of crimes committed during the last sixty years abundantly prove.

But these are not the only sources of danger; for experience has abundantly shown that the subject himself may be prompted to commit theft and other species of crime after emerging from the hypnotic condition. This fact has become the subject of special judicial enactment in several countries.

Finally, the repeated hypnotization of the subject is liable to be followed by more or less dangerous consequences to himself. Inordinate emotionality, impairment of volition, and a tendency to become spontaneously hypnotized, or at least excessively drowsy, are some of the more obvious features of this post-hypnotic condition. Dr. J. Leonard Corning has at the present time under his care, as we learn from the *Medical Record* of Nov. 8, a gentleman who exhibits this neurosis—for neurosis it certainly is—in a striking manner. He is a man of rare gifts, he has maintained and still enjoys a high position in the community, and yet his mental decrepitude is so obvious that it is a matter of astonishment that he has been able to disguise its source so long. Currently he is regarded as a sufferer from mental overwork, and Dr. Corning confesses that he should have had great difficulty in arriving at the true nature of his difficulties, had the patient not confessed that he had been hypnotized scores of times, and that his present infirmity had come on as the direct result of these abuses, for abuses they certainly were.

Such a person as this is, of course, exposed to manifold dangers; for he had become so susceptible, that not only is it possible for any one to hypnotize him, but he is able without further assistance to induce in himself the sleep-like state.

Here, then, are the more manifest dangers of hypnotism.

## LETTERS TO THE EDITOR.

*** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

*The editor will be glad to publish any queries consonant with the character of the journal.*

*On request, twenty copies of the number containing his communication will be furnished free to any correspondent.*

### Origin of Right-handedness.

PROFESSOR BALDWIN, in *Science* for Oct. 31, gives some interesting observations of his own on the development of right-handedness in an infant. He thinks the fact that the right hand began to be used in preference to the left only when the movement required an effort, is an argument in favor of those "feelings of innervation" whose existence many psychologists disbelieve, and challenges me (as a disbeliever) to explain the phenomena in any other way.

Why, asks Professor Baldwin, should the baby prefer her right hand for strong movements, unless previous experiences of using both hands had left behind them the sense that the nervous discharge that actuated the right one was stronger than that which actuated the left?

I admit that this is a possible way of explaining the facts; but yet, if a memory of the previous superior effectiveness of the right hand is what determines now the choice of it for these movements, it does not seem in any way clear that the memory in question need be of the *efferent* current of discharge (the printer has made Professor Baldwin say "afferent" instead of "efferent"). Why may it not be of the greater promptitude, security, and ease of the right hand's movement, as apprehended in an *afferent* way, during previous performances? Professor Baldwin gives no grounds for his rejection of this equally possible alternative. But to my mind it is by no means evident that memories of any sort of past performances play a part in the preference accorded to the right hand. On any theory we have to suppose antecedently to all memory a natural prepotency in the paths of discharge into the right arm. Professor Baldwin's own idea, that discharges into this arm leave images of their superior strength behind, implies that they *have* greater strength to begin with. Why, now, with this organic peculiarity, may they not also have greater readiness to discharge when the stimulus reaches a certain amount? He who conceives of the mechanism of all these early movements as in principle the same as that of reflex action, ought, if he bears in mind the extraordinarily elaborate way in which different stimuli are correlated in the organism with different paths of discharge, to have no difficulty in believing the nerves which ran down into the right arm to be, on the whole, the most permeable paths of exit from the brain of such currents as run in from objects offered to the baby at a certain distance off. Grasping at such objects was the sort of performance which Professor Baldwin seems most to have observed. It is obviously an instinctive or semi-reflex act; and I should much rather explain it directly by connate paths alone, than by connate paths *plus* memories *plus* choice, after Professor Baldwin's fashion.

I must therefore conclude that Professor Baldwin's observations fail, in my opinion, to throw any positive light at all on the vexed question of whether we feel our motor-nerve currents as they pass out of our brain. In themselves, however, these observations seem very interesting as showing how strong stimuli may produce more definitely localized re-actions than weaker ones. The baby grasped at bright colors with the right hand almost exclusively.

WILLIAM JAMES.

Harvard University, Nov. 5.

### Mount St. Elias.

As the National Geographic Society will shortly discuss the most recent observations on Mount St. Elias, with the full data of Messrs. Russell and Kerr as a basis, I have determined to refrain from taking any part in the newspaper discussion in regard to the height of this mountain or the respective value to be assigned to the different sets of observations due to different observers. This is the only scientific method to pursue; and it is due to those well qualified and energetic explorers that their results, when they are finally made known, should form the basis of discussion, rather than opinions, surmises, or guesses prior to their final computations. The only thing which can be definitely stated at present is that they are of the opinion that the mountain is lower than the height of late accepted for it, and the very rough preliminary computation of their observations appears to sustain this view. The weight to be assigned to their observations, and the final outcome of the revised computations, are matters for the future.

But the article by Professor Heilprin on the Mexican mountains, which you have reprinted (p. 260) under the title of the "Culminating Point of the North American Continent," no doubt unintentionally, but nevertheless seriously, misrepresents the methods by which my results of 1874 were arrived at; and, in the interest of a clear understanding of the subject, it is perhaps desirable that some of its fallacies should be pointed out.

Professor Heilprin is no geodesist, as his discussion of determinations of heights of over 17,000 feet, based on a single pocket aneroid barometer, is sufficient to show. A little inquiry in proper quarters would have made it clear to him that observations taken with such an instrument are far from determinative. If they happen to closely approach accuracy, it is merely accidental; and a range of 500 feet in the results would reflect in no way on the care of the observer or the known reputation of the instrument. They bear to the mercurial barometer much such a relation as sextant angles taken at sea for vertical heights do to those taken on land with a vertical circle or geodetic transit.

This want of familiarity with the subject has led Professor Heilprin into a singular misconception of the relative values of observations cited in my "Report on Mount St. Elias" printed in the "United States Coast Survey Report for 1875," and of the data which are given therein with absolute frankness and full detail.

In that report I aimed to embody every thing which might possess even an historic interest, and therefore printed results which I stated to be more or less unreliable for reasons which would be accepted by every competent judge of such matters. I stated that these results were not adopted by me nor incorporated into the work depending upon observations of a higher class. But

Professor Heilprin compares the irregularities of this bad material, denounced by me as bad, and concludes that it is good evidence for doubting the value of that which was considered to be more reliable. Such reasoning obviously affords only a *non sequitur.* I do not think any one who has passed laborious days and nights in the determination of angles by repetition and reversal will agree with Professor Heilprin that the system of "extracting averages" is "delusive;" and a reference to my report will show that it was a question of comparison of averages with a view to the weighing of methods with which, in that instance, I was concerned, which could hardly delude any one who chose to read what was printed on the pages before him. Averages may be made delusive, but not when used in this manner.

In conclusion, although the whole subject is one for experts and professional surveyors rather than others, I may summarize for those who are interested and unprofessional the main features of what was done in 1874 for the purpose of getting at the height of that unattainable peak.

In the determination of any height by triangulation, there are to be considered the character of the instruments, the distance of the peak, the vertical angle measured, and the refraction of the atmosphere, which distorts the line of sight and introduces an error, tolerably constant for high angles and short distances in ordinary latitudes, but irregular and sometimes very great in angles measured when the line of sight passes near the surface of the earth, especially for long distances and in high latitudes.

In the case of Mount St. Elias the distance depended upon a horizontal triangle observed from two astronomically determined stations, giving an astronomical base-line from which the lines converging on the peak were obtained by an astronomical azimuth. The value of such an intersection depends somewhat upon the size of the angle, which in this case was large, nearly 60°. The liability to error which very small angles of intersection may introduce was therefore measurably avoided.

The positions of the ends of the base-line were well determined. The circumstances of the observation made at sea were eminently favorable. The error of this position could hardly have exceeded three miles on the worst assumption; and the error of distance which this would produce in the base of the vertical triangle, upon which the height depended, was trifling. The instruments were first-class of their kind. The vertical angle measured, I venture to say, is beyond dispute. The uncertainty remaining, therefore, was in regard to the refraction,—a factor beyond our power to determine, and equally undetermined in all observations made to date.

However, the height of Mount Fairweather was tolerably well determined from positions near its base. We reasoned the error of refraction might be assumed to be the same for both mountains at the same moment, both being visible and not differing very greatly in their distance from our station. The difference between the height of Fairweather as measured from near its base, and that which we might obtain for it from our Port Mulgrave station, might be assumed to be due to refraction, and an analogous amount applied to the result for St. Elias as a correction for that unknown error. This was an assumption, of course, but a reasonable one, and was adopted.

The height of Mount St. Elias may very possibly be less than our results would show; but that they were likely to be correct within certain limits seemed probable, from the fact that angles measured by Malespina in the last century, the record of which is fortunately preserved, when computed with a corrected base-line in accordance with our observations for the position of the mountain, gave results approximating our own,—an apparent confirmation which was certainly impressive.

The outline of our proceedings is given, as above, in entirely untechnical language, but those who are professionally qualified to judge the character of such work are confidently invited to examine the report itself in the Coast Survey volume for 1875. This is somewhat amplified from the extra advance copies which were distributed before the publication of the volume. I make no pretence to the character of a geodetic expert, but the comparatively simple computations contained in this report were prepared and reviewed by those who are; and the error, if error there be in the results, is due to factors which were entirely independent of the observers or the computers, under the circumstances.

Smithsonian Institution, Washington, D.C., Nov. 11. WM. H. DALL.

### Chalk from the Niobrara Cretaceous of Kansas.

REFERRING to Professor S. W. Williston's interesting communication in *Science* for Oct. 31, on microscopic organisms from the chalk of the Niobrara cretaceous of Kansas, I should suppose it to be highly probable that the forms met with by him are, as he supposes, coccoliths. Coccoliths are very abundant in, and sometimes form a notable proportion of, the calcareous parts of the Niobrara beds in Manitoba and in Nebraska, and are there associated with foraminifera and with rhabdoliths, to which latter class the slender, rod-like bodies, also noted by Professor Williston, may be referrible. Figures and a description of a number of varieties of coccoliths and rhabdoliths from the cretaceous of Manitoba may be found in the *Canadian Naturalist* for April, 1874 (p. 256). GEORGE M. DAWSON.

Geological Survey of Canada, Nov. 10.

## BOOK-REVIEWS.

*Races and Peoples.* By DANIEL G. BRINTON. New York, N. D. C. Hodges. 8°. $1.75.

DR. BRINTON has undertaken the difficult task of presenting the whole vast field of anthropological science in a concise and readable form, and he has admirably succeeded in giving us a book that is attractive, and, in all its parts, suggestive. Therefore not only will it prove useful in making the public acquainted with the facts and some theories of ethnological science, but it will also incite the painstaking student to more thorough investigation of mooted questions, and open new vistas in many fields of research. Dr. Brinton's theories, even such as may not appear acceptable, are always full of ingenuity, and certainly worth the careful attention of anthropologists. The present book, notwithstanding the briefness with which necessarily all problems are treated, teems with new ideas and excellent critical remarks. In reviewing it, we must confine ourselves to selecting a few of the more important points. On the whole, we might wish that some still very doubtful theories to which the author adheres were not presented with quite as much assurance as finally settled.

The introductory chapter, on "The Physical Elements of Ethnography," strikes us least favorably. We think that not sufficient stress has been laid upon the great variations inside each race, and that too much is made of the peculiarities of the "lower" races, which in some respects might be called rather exaggerated human types than simian in character. The second chapter, "The Psychical Elements of Ethnography," is a succinct presentation of the chief causes governing the development of society. The author distinguishes associative and dispersive elements: the former including the social instinct, language, religion, and arts; the latter, the migratory and combative instincts. Dr. Brinton is inclined to consider the sexual instincts and the resulting parental and filial affections to be the prime cause of association, and rejects all theories based on promiscuity. The third chapter will be found full of interest, more particularly where the author sets forth his ideas regarding the development of man, as well as his classification of mankind. Although he knows how to present his views with much force, we cannot consider his description of the earliest stages more than an ingenious hypothesis, because we have so far no means of reconstructing the history of the period immediately after man had made his appearance. Dr. Brinton believes that mankind during the preglacial period was homogeneous, his industries paleolithic with simple implements, his migrations extensive, his language rudimentary. Such speculations can neither be proved nor disproved. Even the character of the glacial period, as described by Dr. Brinton, is largely hypothetical. He believes the migrations to have been limited at the time, the races to be living in fixed areas. It seems impossible to fix any period for these events which have certainly taken place at some time. The author's general ethnographic classification is based on physical characters. According to these, he distinguishes Eurafrican, Austafrican, Asian, American, and