### Women.

A LATE correspondent of yours is guilty of a species of bad taste, which happily is rapidly becoming extinct. It was once considered both clever and gentlemanly to speak of women as if they belonged to one of the lower orders of animals, but that period has now quite passed by. Remarks of such a kind are hardly ever met with in English publications, and seldom in those of this country within a certain range of longitude. I happened to see it stated lately in a book on etiquette that it was no longer considered good form to make insulting remarks about women, and, when a principle has reached that organ of distribution, it may be considered that it has already become pretty widely disseminated. The change is an agreeable one, not only to women, but also to the rather numerous class of chivalrous-minded men.

If women are not capable of a very high degree of intelligence, it can at least be maintained that they are capable of a higher degree than Americans. An English woman has written greater novels, and a Russian woman has made more important contributions to pure mathematics, than any American man. Neither women nor Americans have had very great incentives to intellectual work hitherto, but it is quite possible to hope that they will both play a more important *rôle* in the future than they have done in the past.

If women are more easily frightened than men, it is as easy to attribute it to a more sensitive organization as to any other cause. Poets and musicians are not as cool and collected in the presence of danger as firemen, nor white men as the American Indians. Many people consider that the delicately balanced nervous organization of the horse indicates as high a degree of development as is to be found in more phlegmatic and thick-skinned varieties of animals.

It is not surprising to find that your correspondent's bad taste is equalled by his bad logic. It is seldom that one finds in so short a space so many pretty specimens of unreason: —

1. The cockroach, when caught between two hot portions of metal, chose to jump down instead of walking over them. If it had broken its neck, and if the metal had not been so hot as to injure it, this conduct would have turned out to be very foolish; but, in fact, the cockroach ran away unhurt. The highest wisdom could not have dictated a more prudent course of conduct, and there is hence no analogy to a case of jumping from a window in unreasoned terror when there are other and better modes of escape.

2. Because an organized being has reached a stage of development where reasonable conduct may be looked for, it does not follow that none of its actions will be instinctive. Both men and women perform many instinctive actions, — a drowning man will instinctively catch at straws, — but that does not prove that they are not endowed with reason in addition to instinct.

3. Your correspondent maintains that what would be instinct in women, and hence proof of a low grade of intelligence, is, in the cockroach, "singularly like the operation of reason." But it is no mark of reason having come into play, that conduct looks intelligent to the outsider. If it were, we should have to attribute reason to the Amœba, which encloses food and not grains of sand, and to the Drosera, which shuts up on bits of meat and not on bits of chalk. The one sure objective test of the action of reason is that different individuals behave differently under the same circumstances, and that test is wanting here. We are expressly told that every one of more than a dozen cockroaches did exactly the same thing. Cockroaches make their constant home by the kitchen range, and there is hardly any source of danger which ancestral experience is more likely to have warned them of than hot metal.

L.

### Ancient Scrapers.

A FACT has lately come to my knowledge which may be of interest to archæological students of the ancient stone age, who have frequently expressed surprise that so few of the ancient scrapers, blades, chipped axes, and other cutting implements, show signs of use.

Lieutenant Stoney, Lieutenant Ray, Nelson, Turner, and others have sent to the National Museum a large number of modern Eskimo scrapers, and also many specimens of the implements used in chipping and sharpening their scrapers. The latter are of two kinds: 1. A curved handle of walrus ivory, with short pieces of antler lashed in a groove cut in the front of the handle (this form has frequently been figured); 2. A single cylindrical handle of wood, into one end of which an incisor tooth of a beaver has been firmly fixed. Indeed, one or two specimens consist of a portion of the upper jaw with the teeth in place. This tool is called by all collectors a knife-sharpener. Lieutenant Stoney informs me that during his late exploration in Kotzchue Sound he saw the natives using these implements, and says that they keep them always at hand, and spend much time in touching up the edges of these scrapers and other stone cutting-tools, and that the beaver-tooth sharpener is also employed by the ivory-carvers to keep a fresh edge on their metal knives. The variation in the length of scraper-blades is due partly to the fact that some of them, when new, are over two inches long, and become worn down by constant sharpening until they are reduced to a mere stub. It will be seen from Lieutenant Stoney's observation that it will be difficult to find in Alaska a scraper-blade showing signs of use, the interest of the artisan depending upon his keeping his edge constantly sharp.

O. T. MASON.

Washington, June 25.

### Volapuk.

I COPY the titlepage of one and a part of another grammar of Volapük, before me. Hachette & Co. is a London house, as you will see. The Paris house is Le Soudier. "Grammar of Volapük: The Language of the World. For all Speakers of the English Language. Translated and published with the consent of the inventor, Johann Martin Schleyer, by W. A. Seret. Glasgow, Thomas Murray & Sons; London, Whittaker & Co." "International Commercial Language. Abridged Grammar. . . . By Karl Dornbusch. London, Hachette & Co.; Paris, H. Le Soudier."

E. A. HORSFORD.

Cambridge, June 25.

### Pineal Eye of Lizard.

THE pineal eye is so well developed in the common pine-tree lizard (*Sceleporus undulatus*) that it may probably seem to warn its owner of the advent of daylight. It is a lenticular, glassy area of the skin of the vertex (about a millimetre in sagittal diameter), surrounded by a yellow border, and having a dark spot in its centre. The dark spot is opaque, caused by a mass of pigment internal to the dermis, set on the extremity of a pineal outgrowth from the brain. The clear area around it is caused by the dermis, which is transparent and free from the pigment which covers it internally in other parts. The eye is covered by an escutcheon-shaped epidermal shield, more transparent in the centre and larger (3 × 3 millimetres) than the normal epidermal scales. The only sign of degeneracy is the central cloudy mass of pigment, like a big cataract.

G. MACLOSKIE.

Princeton, June 25.

### The Charleston Earthquake.

I FEEL thankful to Professor Mendenhall for his forcible criticism of the paper relating to the Charleston earthquake, and fully concur with him in his remarks concerning the uncertainty of the data upon which the insoseismals were drawn. This was commented upon in similar vein in the paper under discussion. He cannot complain of them more loudly than we did. The features to which he calls attention (viz., that the curves of high intensity are less sinuous than those of low intensity) had not escaped our attention, and the results of our reflections were these: 1st, The data indicated that the amount of variation of intensity within any zone or annulus generally bears a smaller ratio to the mean intensity of that zone when the mean intensity is high than when it is low (I think this was to be expected, and is intelligible from the nature of the case), hence there ought to be less sinuosity in the inner than in the outer curves; 2d, In order that the amount of sinuosity may be in due proportion in all curves, the density of observation (i.e., number of observations per unit area) should be inversely pro-

portional to the square of the distance from the origin. Perhaps it is needless to say that the observations had no such distribution. But, after all is said, it must be admitted that there is much justice in Professor Mendenhall's criticism of the isoseismals, and he certainly scores an important point. An earnest and conscientious effort will be made to remedy the defect he has undoubtedly proven.

As regards the 'areas of comparative silence,' I think they have been too well established by the data in hand to be explained away on the ground of defective testimony. They attracted attention at an early stage of the investigation, and were at first thought to be due to defective testimony; but as the information increased, it was seen that they were not so easily disposed of. Special inquiry was then made, and the result was, to our thinking, a full confirmation of their reality.

In his criticism upon the method of computing the depth of the focus, he proposes an argument which we anticipated would be raised against it. He says, "As far as can be seen from the contents of the paper, the result depends upon the unjustifiable assumption that surface destruction is proportional to" the energy per unit area of wave-front. I cannot admit that the paper implies that assumption. But if he will permit me to substitute the word 'effects' for the word 'destruction,' then I will say that the result does depend upon the assumption so modified, and stands or falls with it. And, moreover, I hold that assumption to be not only justifiable, but next door to an axiom. If our estimate of relative intensities were to be derived solely from the destruction of buildings and chimneys by a force which in turn must be measured by the maximum acceleration of the earth-particle in a horizontal plane, our argument would indeed be in a pitiable plight. But we ought not to be, and certainly are not, so limited. Other means of forming an approximate estimate of relative intensity are abundant, even where the destruction is little or nothing. Subject to local modifications, a great earthquake is bound to make itself felt somehow, and in due proportion to its energy, whichever component, vertical or horizontal, predominates. In the epicentral tract, brick buildings were few; but there were plenty of wooden ones, and plenty of intelligent men to tell what had happened. The best but by no means the only inanimate testimony was furnished by the railroads which cross this tract. They were like continuous lines of seismometers; and the men who repaired them had no difficulty in stating where the road-beds were shaken up most, and where least, and how the effects varied from mile to mile.

What Professor Mendenhall really challenges, I infer to be, not the theory, but the competency of the data through which the theory must be applied, if it can be applied. He appears to doubt the possibility of procuring such data; but it seems to me that he overestimates the exactions. He sees, indeed, that the vanishing of the constant $a$ dispenses with the necessity of making any absolute evaluation of a single intensity, or even of the successive ratios between intensities. All that we require is to find, if possible, where these intensities vary most rapidly along a line. It is analogous to trying to locate, without the use of a level, the steepest point of a hill whose profile is similar to our intensity curves. It cannot be done exactly, but it can be done within moderate limits of error; and I have not much doubt, that, when Professor Mendenhall sees the data, he will concede as much. It was distinctly stated in the paper that the method was believed to be incapable alike of great precision and of great errors.

But, though I cannot yield to his criticism on this point, I am still greatly indebted to him for it. It is instructive in pointing out sharply what treatment must be given to the data to enable readers and investigators to judge of the validity of the method, and how the facts must be marshalled.

He also dissents from our inference that there were some facts in Charleston which seemed hard to explain upon the assumption of amplitudes of the earth-particle less than ten inches to a foot. This was suggested as a maximum confined to a few spots, while the mean amplitude was presumed to be considerably less. Let us examine this point.

In all great earthquakes, those who have felt their violence near the epicentrum have been impressed with, and testified to, an apparently large amount of movement in the soil, — an amount to be measured, so far as they could estimate, not by millimetres, but by inches, and sometimes even by feet. To verify these purely sensory estimates was, of course, impossible; but the circumstantial character of the testimony seemed, in the absence of precise measurement, to warrant the belief that the movements probably had about that order of magnitude. When the seismograph was applied in Japan to the measurement of the frequent but moderate shocks, and it was found that an amplitude of a few millimetres would sometimes crack walls and throw down chimneys, it was at once inferred that the unmeasured estimates or guesses of the amplitude in the grander shocks had been greatly exaggerated: for, the energy being proportional to the square of the amplitude, it seemed needful to multiply those already measured only a few times to obtain a destructiveness commensurate with that exhibited in the worst catastrophes. There has been, therefore, a great change of opinion about these large estimates among seismologists; but I think it can be shown that such estimates are not necessarily invalidated by the seismograph.

The intensity of a shock is not alone proportional to the square of the amplitude, but also to the wave-velocity divided by the wave-length. It is, I believe, a general fact that great amplitudes of earthquake-waves are accompanied with great wave-lengths. This does not follow from the accepted laws of wave-motion in elastic solids, but is an independent fact, whose explanation must go back to the nature of the originating impulses. Thus increasing amplitude does not carry with it an increasing destructiveness in so rapid a ratio as might at first be supposed. The displacement is greater, but the time of displacement is longer. Again, the amplitude diminishes as the wave moves on; at least as fast as, and probably faster than, the distance from the origin increases. Let us, then, endeavor to make a comparison, rough though it must necessarily be, between the larger amplitudes measured by the seismograph, and those which may be inferred in localities shaken by the Charleston earthquake with equal energy. I regard it as improbable that the intensity of the most vigorous shocks measured by the seismograph in Japan (so far as published) exceeded that at Atlanta, Asheville, and Raleigh, all of which have been estimated to exceed No. 7 in the Rossi Forel scale. If we take ten millimetres as the average amplitude of those places, we shall not exceed the higher ones recorded by the seismograph for shocks of probably not greater intensity. The mean distance of these places from the centrum is eleven and a half times as great as that of Charleston. This would give an amplitude of about three inches at the latter place, on the assumption that the wave-lengths were equal to the Japanese, and that no energy was dissipated as the waves moved on. The last assumption is certainly untrue, and, whatever allowance may be made for it, must lead to a greater inferred amplitude at Charleston. It does not seem to me that a mean amplitude for the greater waves in that city, of three to four inches, is too much, while local maxima may have been considerably greater. The seismograph has not as yet tackled a first-class earthquake in the vicinity of the central tract.

Although I am still disposed to adhere, either wholly or in part, to most of the propositions advanced in the paper referred to, I must still acknowledge the high value of Professor Mendenhall's criticism. It defines much more sharply the issues involved, and is full of most useful suggestion. C. E. DUTTON.

Washington, June 23.

### Cyanhydric Gas as an Insecticide.

AMONG the insect-enemies to plant-life, of which California has received and is still receiving a full assortment from all parts of the globe, the most formidable is the *Icerya purchasi*, a coccid which, instead of the hard shield that protects most of its congeners the scale-lice, surrounds its egg-masses with a woolly fur that in many respects serves even as a more efficacious protection. It has until recently been supposed to have come from Australia; but, according to late researches of Professor Riley, it is to the Island of Martinique that we are indebted for this most pernicious insect. It there infests the sugar-cane, and may readily have come in with the canes often placed for drainage within the hogsheads of raw sugar. Being apparently omnivorous, it has not been dismayed by the absence or scarcity of its original plant-food. Pine and cypress appear to be nearly as much to its taste as the