nal investigations. Even meteorologists outside the office, or employed by it as consulting specialists, may find it to their advantage to avail themselves of this opportunity for publication. Considering the great future evidently in store for meteorology, it is not surprising that Professor Abbe is, as we understand, diligently inquiring for those who are willing to come to his assistance in the effort to develop a systematic, deductive, and exact science of meteorology. We commend this subject to those whose studies have taken this direction. There are needed the investigator, the teacher, and the expert consulting-meteorologist, precisely as in other branches of science.

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.*

Chemical geology.

IT appears to me, that in his interesting communication in the number of *Science* for Dec. 28, Professor Winchell has fallen into an error, which, while diminishing by more than one-eighth his estimate of the secular increase of the earth's mass, is yet more serious from the stand-point of chemical geology. In determining the amount of carbon dioxide abstracted from the atmosphere and fixed in the earth's crust, he estimates, first, that represented by the carbonate rocks (limestone, dolomite, etc.), and, second, that required for the decomposition of an assumed thickness of decomposable silicate rocks; and both these amounts are included in his grand total. But this is certainly bad book-keeping, for a portion of the carbon dioxide is counted twice. The decay of the silicate rocks is a necessary antecedent of the formation of the carbonate rocks; and the carbon dioxide of the latter is precisely the same as that which has previously decomposed the former. In general terms, this grandest of all chemical processes proceeds as follows: the carbon dioxide of the atmosphere decomposes the felspars, hornblende, augite, micas, etc., of the silicate rocks, leaving the alumina and iron with the silica as a more or less ferruginous kaoline, and forming carbonates of the alkalies and alkaline earths, which are carried away in solution, and ultimately reach the sea, where the latter are deposited as limestone and dolomite, and the former react with the calcium and magnesium chlorides of the sea-water, producing alkaline chlorides (chiefly common salt) and more limestone and dolomite. As Dr. Hunt has so clearly shown, the kaoline on the land, and salt in the sea, are merely incidental results of the fixation of the carbon dioxide of the atmosphere in the carbonate rocks.

W. O. CROSBY.

Osteology of the cormorant.

Dr. Shufeldt's letter in *Science* (ii. 822) calls for a few remarks. In relation to his first statement, that 'the occipital style of the cormorant is not an ossification in the tendon of any muscle' of the neck, Selenka wrote as follows: "Eigenthümlich ist dem Carbo cormoranus und C. graculus, aber auch nur diesen beiden, ein an dem *occip. superius* durch bandmasse verbundener, dreieckig pyramidenförmiger, nach hinten gerichteter knochen, welcher die ansatzfläche der den kopf bewegenden muskeln soz. vergrössert; er ist ein sehnenknochen und gehört nicht zum schädel" (Thierreichs, 19). In view of such eminent authority, it would seem that something more than simple denial is required to upset a statement accepted by anatomists for many years. It is worthy of note that Dr. Shufeldt does not mention the nature of the bone in his article, and that, in ignoring the point to which I took exception, he virtually acknowledges his mistake. It is difficult to understand how one who does not know the position of a bone is qualified to expound its nature; and in all cases it is wise, if we would convince, to give reasons for dissent from authorities.

As to his second statement, that my ideas of the morphology of the rotular process are wrong, I would simply remark that the ideas referred to are not mine, but those of Nitzsch, of Meckel, of Tiedemann, of Owen, of Selenka, and of Mivart, and suggest that it would be appropriate to read such eminent authorities before disposing of them with an empirical denial. Dr. Shufeldt's paper clearly intimates that the rotular process of the divers is the homologue of the patella in other birds. The coexistence of the two disproves this by *reductio ad absurdum.* I would invite Dr. Shufeldt to quote the passage to which he refers when citing Owen as considering any process of the tibia as the analogue of the patella.

Lastly, Dr. Shufeldt states "that, furthermore, I find myself misquoted more than once." I would remind Dr. Shufeldt that I quoted him but once; and of the accuracy of this, any one may satisfy himself by referring to *Science*, ii. 642, 2d column, line 19.

J. AMORY JEFFRIES.

Electric time-signals.

Your correspondent who describes his method of making electrical signals in a recent number of *Science* (ii. 823) can greatly simplify and thereby improve his arrangement by inserting within the clock a couple of thin metallic springs with platinum contacts, the circuit being completed by the pressure of the hammer on the 'outward stroke.' The writer has had such an attachment to an ordinary 'programme clock' in constant use for about ten years, as is doubtless the case with many others who have had occasion to distribute time. The signals are transmitted to several buildings, in one of which an electric gong is struck, and in others a number of 'vibrating' bells are rung.

Mercury contacts are generally troublesome. The arrangement described seems unnecessarily complicated: besides, it is difficult to see the necessity for insulating the clock 'on a square of plate glass.'

M.

Columbus, O.

Capitalization of names of formations.

The use of capitals is a literary rather than a scientific matter; but geologists, nevertheless, suffer as a class from the existing confusion in regard to the names of formations.

Authors who are consistent with themselves in this matter fall into three classes. Those of the first class speak of the *P*otsdam, and of the *C*arboniferous, but of *p*otsdam strata and *c*arboniferous strata. In so doing they class the names of formations as proper nouns, but refuse to recognize proper adjectives. This practice employs a German idiom not otherwise countenanced in our language: we do not say *g*erman idiom. Another objection is, that the practice introduces a distinction difficult to maintain on account of the graduation of the nominal into the adjective sense. 'The Carboniferous' may or may not imply some such noun as formation, and the degree of such implication is variable.

Authors of the second group speak of the *P*otsdam and *P*otsdam strata, but of the *c*arboniferous and *c*arboniferous strata. The distinction thus made is etymologic, being based on the immediate derivation of the name of the formation. To this there are two objections. First, it is contrary to the analogies of the language, for capitalization is generally controlled by meaning. We speak of 'the *P*acific,' although the designation is etymologically a common noun; and we call the recently popular feminine waist-gear a *j*ersey, although the designation is etymologically a proper noun. Second, it has the effect of recalling attention continually to the derivation of names, and thus retaining their connotative meaning. For mnemonic reasons, and for these only, it is convenient that names of formations should originally be connotative, but it is of prime importance that they should eventually become merely denotative. There was a certain original utility in having 'Potsdam' call to mind a place, and 'carboniferous' a character; but the names having become securely attached to their several formations, it is now imperatively demanded that each shall designate a certain portion of the stratigraphic column and a certain portion of geologic time, without connotating place or composition. Indeed, the reason why modern usage employs geographic terms in the naming of new formations, instead of designating them by their physical characters, is that a minimum of connotation is thus secured from the outset.

Authors of the third class capitalize all names of formations, whether used as nouns or adjectives, and in so doing escape these evils. The only objection I see to their practice is, that it classes with proper nouns a group of names which may fairly be compared with other groups not so classed. The demarcation between common and proper nouns is essentially somewhat obscure; and the drawing of the line is largely a matter of practical convenience. It is noteworthy that no author whatever has so drawn it as to include all names of formations with common nouns.

The capitalization of all formation names has the manifest advantage that it enables one to say that the *C*arboniferous rocks are not the only carboniferous rocks, or, in other words, that it does not deprive the geologist of the independent use of words indicative of rock character which have been appropriated for the names of formations. If the use of capitals were altogether discarded in the designation of formations, this advantage would be lost, but another would be gained; for we should then be able to speak of the rocks of *P*otsdam without implying their *p*otsdam age.

G. K. GILBERT.

Remsen's 'Theoretical chemistry.'

Will you kindly allow me to correct an error into which it seems that I fell, in my notice of Professor Remsen's 'Theoretical chemistry' (*Science*, ii. 826)? It cannot be denied that the statement, "Of the substitution products of benzene which contain three substituting groups, more than three varieties have been observed," is literally true. The context and form of expression were such that I could not but think this assertion was made of those derivatives in which the three substituting groups were alike. Had it occurred to me that the statement was not thus limited, I certainly should not have pronounced it rash, but so cautious and incomplete that it must inevitably mislead even the most careful reader.

THE CRITIC.

Synchronism of geological formations.

I trust that you will permit me a little more space to reply to the further remarks of Mr. Nugent on this subject (*Science*, iii. 33), seeing that your correspondent has failed to grasp the point which I had intended to elucidate in my last communication.

Mr. Nugent is correct when he contends that I rest my case on the non-occurrence of 'evidences of inversions;' and, if my line of argument based on this fact fails to meet with his approval, I sincerely regret it. Paleontology, as far as I am aware, has thus far failed to show a single unequivocal case of faunal inversion such as I have indicated; nor does there appear at the present time very much likelihood of its ever being able to do so. Nor would the discovery of a solitary instance materially affect the question, inasmuch as, upon the theory of very broad contemporaneity suggested by Huxley, instances of inversion ought to be about as numerous as those of non-inversion. My courteous critic admits that "there is no reason why such instances of inversion should not have occurred over and over again," and that at the present time their 'occurrence is almost unknown;' but his appeal to the 'imperfection of the geological record' (both geological and geographical), in explanation of the overwhelming negative testimony, will, I am afraid, scarcely meet the situation.

The special cases referred to — Barrande's colonies, and the intermixture of Silurian and Devonian, and Devonian and carboniferous fossils in the old red sandstone of Scotland — are far from being of the character desired. The former need scarcely to be commented upon, since they have always been involved in a certain amount of obscurity; and their very existence as such has very recently been denied by Marr, who personally examined the region, Lapworth, and a host of other geologists. In the case of the old red sandstone of Arran, where there is an intercalation of a band of marine limestone containing Productus giganteus, P. semireticulatus, P. punctatus, Chonetes hardrensis, Spirifera lineata, and other well-known carboniferous fossils, Professor Geikie (who, we believe, first made the observation) distinctly affirms that these organisms must "have been in existence long before the formation of the thick Arran limestone," and that their habitat during the period of the deposition of the underlying sandstone was immediately outside of the basin or basins that through upheaval were now being gradually isolated from the sea: in other words, we have here merely an instance where the range of a certain number of organic forms has been extended somewhat lower down in the geological scale than it had hitherto been indicated. These same forms re-appear in the superimposed lower carboniferous limestones, and, as Professor Geikie observes, they must have been living during the long interval coincident with the sedimentation of the intervening sandstone 'outside of the upper old red sandstone area.' The same relation holds with the Siluro-Devonian mixture in the basal old red of Lanarkshire. No one can deny the local displacement and interchange of portions of two consecutive faunas, especially at about the beginning or close of their own respective series; but these displacements are not of the nature of the inversions that ought to illustrate the doctrine of broad contemporaneity.

To what extent similar or identical faunas indicate *absolute* chronological relationship can probably never