tive, not only from the richness of the flora, but from the large number of endemic monotypes (monotypic genera or sub-genera, or widely distinct species), confined to very restricted areas, and of disjointed species—identical species in widely dissevered areas. With regard to the species of narrowly confined stations, Prof. Grisebach believes that the considerations he has brought forward tell decidedly in favour of the conclusion “that monotypes and other rare organisms are not—or, at least, are not generally—to be regarded as the surviving remains of earlier creations, but as evidences of the productive power of the localities where they are now to be found, and from whence no means of migration are within their reach” (p. 364). He can, however, scarcely have paid attention to the various proofs recorded of the gradual reduction of the areas of several Mediterranean species, even within historical times, and still more since an immediately preceding geological epoch—that of the formation of the tufas of the south of France. He does not, indeed, seem to be aware of the instructive memoirs on this subject of Gustave Planchon (see Nat. Hist. Review, 1865, p. 202).

The “disjointed” species, on the other hand, appear to have puzzled Prof. Grisebach, as they have done and will continue to puzzle all speculators on Geographical Botany. Grisebach endeavours to reduce their number as much as possible; sometimes by the discovery of intermediate stations; then, again, by presumed colonisation through man or other agencies; or by showing that supposed identical forms in distant areas are really distinct species, and, therefore, beyond the scope of inquiries limited to the age of now-existing species. But yet, in the Mediterranean as in the Japanese provinces, he is obliged to admit some which occupy two limited areas separated by enormous intervals. Thus, although he supposes that the appearance of *Rhododendron ponticum* on the coast of Portugal may have been the result of introduction by the Arabs, that *Geum heterocarpum*, now only known from the mountain regions of S. Spain and of Elborus in Persia, may yet be found in intermediate localities; yet such suppositions, he admits, can in no way account for the disseverance of the Cedar in the Atlas, the Lebanon, and the Himalaya, or of the *Pinus excelsa* in the mountains of Macedonia and the Himalayas. Unwilling to admit that these and other instances (far more numerous than acknowledged by Grisebach) of widely dissevered stations may be the remains of once continuous areas, he suggests the possibility of the transference of seeds by winds, birds, &c. Birds are, indeed, probably powerful assistants in the migrations of plants. But the effect of winds has been much overrated, as shown for instance by Kerner in a paper recently published in the *Zeitschrift des Deutschen Alpenvereins*, and is made more of perhaps by Grisebach in the present work than by any other observer, and not always on the safest data. Thus he attaches (p. 389) great importance to an “unpublished memorandum of Berthelot’s,” that is to a label to a specimen of *Erigeron ambiguus*, bearing the words “cette composée, qui a quelques rapports avec les Conyza, est devenue très-commune sur toutes les côtes de Ténériffe après le dernier ouragan.” This memorandum is amplified into “On the Canary Islands whose flora was so well known to him, this traveller saw, immediately after a violent hurricane, an annual *Synanthera*

(*Erigeron ambiguus*) which is generally dispersed over the Mediterranean flora, suddenly germinate and take permanent possession of the soil in the most diversified stations,” the amplification thus including some half-a-dozen statements not contained in the original memorandum, adding especially the *propter hoc* to the *post hoc*. *Erigeron ambiguus* is one of those plants of which a single individual will produce seed enough to cover a considerable tract of country in the next following season, if favoured by a suspension of those counteracting influences which annually destroy all but one out of thousands, either in the state of seed or of the infant plant; and in Berthelot’s memorandum we find no evidence either that the plant was not in the islands before the storm, or that the seed was actually brought by the storm, or that if so brought its germination and early growth were so exceptionally rapid, as to show the plant in an observable stage “immediately” after the storm. The inquiry, however, into the causes of the disseverance of areas, whether due to the gradual extinction of old races, or to the colonisation of new ones, remains one of the most interesting problems for solution in Geographical Botany.

OUR BOOK SHELF

Consumption, and the Breath Rebreathed. By Henry MacCormac, M.D. (London: Longmans, 1872.)

THIS work is written *con amore* by an enthusiastic physician, who has satisfied himself of the truth of the theory he advances, and is now desirous of convincing the rest of the world. The theory broached by Dr. MacCormac is that phthisis or pulmonary consumption, as well as tubercle generally, is always and exclusively the result of the breathing of air that has already been vitiated by respiration. It is well known that air that has once passed through the lungs has undergone important changes. Its oxygen is reduced in quantity, a nearly corresponding amount of carbonic acid has been added, and it also contains certain organic compounds the nature of which has not been very satisfactorily determined, but which are undoubtedly of an effete nature, and analogous in their composition to the disintegrated organic compounds eliminated from the body by the other excretory organs. The extremely deleterious action of the re-introduction into the system of the materials discharged by the intestines is now very generally known, from the inquiries that have been instituted into the nature and origin of typhoid fever; and Dr. MacCormac is perfectly justified from analogy in attributing serious results to the re-introduction into the system by the lungs of the air which has once passed through it, and which is consequently charged with decomposing substances. The carbonic acid alone is bad enough, but even if this were removed as fast as formed and replaced by oxygen, while the animal still continues to breathe the air it has already expired, there can be little doubt that it would speedily feel the effects of the other impurities with which expired air is charged. Under ordinary circumstances the only means of avoiding these effects is to permit free access of air to all and every apartment in which man is confined either by day or night; and so far we cordially endorse the views and recommendations of the author of the work before us. But when Dr. MacCormac states that tubercle is exclusively the result of breathing expired air, we think he carries his theory too far. We cannot put aside in the facile manner he adopts the influence of hereditary predisposition, nor the effects of exposure to damp and cold, when combined with insufficient food. Imperfect ventilation is so common that it is almost always as-

sociated with the other probable causes of tubercle, and it is difficult to give instances where tubercular consumption has made its appearance whilst perfectly pure air is continually breathed. But, we think, various considerations render Dr. MacCormac's views untenable. We will not refer to Iceland or to the inhabitants of the elevated plains of the Andes, or of the Steppes of Asia—all of which are sad stumbling-blocks in his way—because, as he says, they are so far off, and our facts in regard to the frequency of tubercle in these regions are perhaps not quite satisfactorily ascertained. But we may call attention to the circumstance that the disease is more common in England than in almost any other country—than in France, for example; yet, surely, the hygienic relations in regard to ventilation are superior in England to those existing on the other side of the Channel.

If air that has been breathed is so certainly the cause of tubercle, the poor population of London and other large towns should not only be decimated, but should be swept off *en masse*, for they all breathe through the night, and through a great part of the day, air so contaminated. Once more, how is it that one member of a household belonging to the upper class is attacked and dies, though all the rest, notwithstanding their being exposed to the same conditions, are preserved? Looking at animals, again, any Indian medical officer will tell Dr. MacCormac that monkeys kept in confinement, though they have never had a roof over their heads and have consequently never breathed air a second time, will die with their lungs stuffed with tubercle. Lastly, the evidence is very strong in favour of Virchow's view, that tubercular matter is originally composed of cells resembling the white corpuscles of the blood, which are either modified white corpuscles, or, as Virchow himself maintains, proceeds from the proliferation of connective tissue corpuscles. Whilst disagreeing, therefore, with Dr. MacCormac in regarding the breathing of air imperfectly freed from the products of previous respiration as the exclusive cause of tubercle, we may fully endorse his views upon the desirability of thorough and complete ventilation, especially in our sitting-rooms and sleeping apartments. The exigencies of modern civilisation seem to lead unavoidably to the close herding of mankind; but we confess it is with a sigh of regret that we see year by year long lines of close-packed houses, springing up on what were but recently green fields on every side of this great metropolis. To reach green fields and breathe fresh air is now a day's work.

H. POWER

Theory of Friction. By John H. Jellett, B.D., P.R.I.A. (Dublin: Hodges and Co.; London: Macmillan)

THIS book is, to a certain extent, of the character of a supplement to ordinary treatises on mechanics. It deals with the question of friction by the use of analytical expressions very general in the possibility of their application, on which account perhaps some of the significance of their physical character may be apt to escape the general reader, and the book is thus, perhaps, rather more suitable for advanced than for junior students.

The author brings well into prominence the radical difference between problems in statical and dynamical friction, namely, that the latter are determinate, whereas the former are not necessarily so. He says:—

"When a system of material particles, each of which rests on a rough surface, is subject to the action of external forces, it will in general be found that, of these particles, some will be in a state of motion and others in a state of rest. Everything connected with the moving particles, namely, their positions, their velocities, and the forces, geometrical and frictional, which act upon them, is fully determined by means of the dynamical and geometrical equations. The geometrical and frictional forces which act upon the quiescent particles will also be determinate, unless it be possible to form by elimination

one or more equations between the co-ordinates of the quiescent particles *only*. If this be possible, the geometrical force replacing every such equation will be indeterminate in intensity."

The character and cause of the analytical indeterminateness in the case of statical friction is enunciated in the following words, which obviously apply also to forces not frictional:—

"If any one or more of the forces acting upon the particles of a system be not determinate functions of the co-ordinates, the number of the unknown quantities will exceed the number of equations, and there will be in general an infinite number of positions satisfying the conditions of equilibrium, disposed in one or more groups, in each of which these positions succeed one another continuously."

There is an interesting chapter on the distinction between necessary and possible equilibrium, arising, so far as friction is concerned, from the fact that the coefficient of dynamical friction is less than that of statical friction, so that "if the system be disturbed from its position of equilibrium by the communication of infinitely small velocities to its several points, when the friction at each point will, of course, become dynamical, a finite force tending to augment the displacement may at once be developed at some or all of these points." The whole point of distinction between this and ordinary unstable equilibrium, when friction is not taken into account, consists in the fact of the infinitely small velocity calling into play a finite force, which it would not do in the case of ordinary unstable equilibrium, in the lapse of a finite time. Without questioning the analytical excellence and interest of the investigation, we may hesitate in adopting the change from statical to dynamical friction as a consequence of the assumption of an infinitely small velocity. We would point to the following problem (page 170) as a good example of the concrete application of the principles of the treatise:—"Two rods, AB, CD, firmly jointed together at B, rest so that A presses against a rough vertical surface, and CD lies on a rough peg in the same vertical; find the limiting positions and the nature of the equilibrium."

At the end of the book there are several problems worked out, namely, the well-known problem of a top spinning on a rough plane, the problem of "friction wheels," and one or two problems connected with the driving wheels of locomotives.

J. S.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Adamites

I SHOULD not have noticed the letter of "M. A. I.," which appeared in the last number of NATURE, with reference to my paper on "The Adamites," were it not that my silence might be interpreted as an acknowledgment of the justice of the remarks of the anonymous writer. If I had been silent, however, I trust your readers would have had more sense than to accept the dictum of a writer, anonymous or otherwise, who thinks to negate the conclusions of a paper, written at least in a truly scientific spirit, by such nonsense as the reference to *Paddy and Taffy*. One looks for reasoning in the criticisms which appear in such a journal as NATURE, and not for a misleading statement of an opponent's position, supported by reference to general conclusions and the use of weak satire. When "M. A. I." condescends to advance an argument, I shall be happy to consider it; and if it should be unanswerable, I shall not hesitate to admit it to be so. Doubtless I ought to feel thankful for the tenderness with which he has trodden on my toes, but I have scant regard for mere courtesy where questions of science are at stake; and in the interests of truth I would rather that the errors of my