— Houghton, Mifflin, & Co. have just issued a new life-size portrait of Dr. Holmes, which is even better than the earlier one.

— Charles Scribner's Sons have in preparation a handsome popular library edition, in four 12mo volumes, of Bourrienne's well-known "Memoirs of Napoleon," a standard work of which many hundreds of imported sets have been sold every year. This new edition will be an exact reprint of the latest English edition, and will contain the thirty-eight portraits of the original, together with all the other features that give distinction to the work. The price will be sufficiently low to bring these volumes within reach of all would-be readers.

— G. P. Putnam's Sons have in press, as their own commemoration of the centennial anniversary of the inauguration of Washington, a unique limited edition of Irving's "Life of Washington," — a work for which Bryant predicted "a deathless renown." The set will be issued in five volumes, handsomely printed in large quarto form, and will contain 200 illustrations, comprising 130 steel plates and 70 woodcuts printed on India paper and inlaid in the text. The plates include portraits of all the noteworthy generals and statesmen of the American Revolution. But 300 sets will be issued, and the type will be distributed as printed from. The price to subscribers has been fixed at $50.

— Mrs. Stowe has been able to revise the biography of herself, written by the Rev. Charles Stowe and Mr. Kirk Munroe. It will be published at an early day by Houghton, Mifflin, & Co.

— Baron Grancy will shortly issue, in Paris, a volume on American customs. It is to be in the shape of a novel, to be entitled "A French Ranch in Dakota," and will treat wholly of Dakotan affairs. The author, according to a despatch to the New York *World*, is the original founder of the Fleur de Lys settlement of French horse-breeders, whose life in Dakota this book is meant to describe.

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.

Twenty copies of the number containing his communication will be furnished free to any correspodent on request.

An Acoustic Mill.

WHEN a vibrating tuning-fork is brought near to a light body, like a pith-ball or a small piece of paper, the latter moves towards the fork as if attracted by it. This phenomenon was observed by Guyot in 1832, and was rediscovered by Guthrie in 1870. It has been supposed by some that gravitation could be explained by the vibratory motions, such as atoms and molecules are known to have; but it does not appear that gravitation sustains any quantitative relation whatever to the temperature of a body, such as would be the case if molecular vibration was the cause of it. The observed phenomenon may be accounted for thus. When the prong of the tuning-fork beats outwards, the air is driven before it and is condensed, while behind it there is a partial vacuum. If the velocity of the prong was greater than that of a particle of air in its free path movement, then there would be a complete vacuum behind the prong. As the latter beats to and fro, it is obvious that the density of the air adjacent to the prong must be less than if the latter was at rest, the difference depending upon the relative velocity of the prong to that of the molecules of air in their free path movements. As the pressure of the air varies as its density, it follows that the air-pressure is less in the neighborhood of the vibrating fork than at a distance from it. Hence, if an object is near to the vibrating fork, the air-pressure will be greater on the remote side, and will push the object towards the source of vibrations.

Numerous devices have been invented by Doornak and Strop to illustrate this principle. Most of them are too complicated and costly to be had by more than a few. The following is simple enough, and can be available for any one having a Chladni plate.

Cut a disk three or four inches in diameter out of letter-paper, and then cut eight or ten radial slits from the circumference halfway to the centre, and turn up one edge of each sector so as to form a kind of paper windmill. Suspend this by a thread from its centre, and see that it hangs horizontally, which may be done by fixing a bit of beeswax to the middle of the disk, and have the thread go through it. Adjustment will be easy and quick by slight pressure upon the wax, changing the relative position of the thread.

This disk may now be brought over a properly mounted Chladni plate near the edge, and as close to it as possible, while allowing it free space for rotation without touching the plate. If the plate be made to vibrate vigorously, the disk will begin to spin, turning in the same direction as if a current of air were blowing upon it from above. The lower components of the sound of the plate will be necessary to make so large a disk as the above to spin, as the higher ones have too many nodes. The fundamental is the best; and, if it can be produced with an amplitude of an eighth of an inch or more, the disk will go round two or three times a second. Of course, the bow should be drawn across the edge opposite to the disk, in order to prevent a node being formed underneath it, and also to avoid the disturbance from movements of the air. I have found that the fundamental vibration of the Chladni plate can more easily be produced by bowing it with a round wooden rod well rosined, than with the ordinary violin-bow. In this experiment the pressure of the air is lessened between the nodes at the surface of the disk, and the space thus affected extends to the height of an inch or two. It is also evident that the light dust that moves to the place of greatest disturbance is moved there by the difference in air-pressure instead of by little whirlwinds caused by the vibrations, as it was explained by Faraday.

A. E. DOLBEAR.

College Hill, Mass., March 5.

Note on the Robinson Anemometer Constant.

THIS is the factor by which the velocity of the central point of the cups is reduced to the actual velocity of wind. When Dr. Robinson first invented his anemometer in 1860, he determined the value of this factor, for all patterns of the instrument, to be exactly 3, and this has been in use for all patterns ever since. But by the experiments of Dr. Dohrandt at St. Petersburg in 1878, this constant, for the Kew pattern at least, was found to be much less, which led Dr. Robinson to repeat his experiments; and the result was a confirmation of Dr. Dohrandt's result, and showed that his own factor is erroneous. Experiments at the Deutsche Seewarte in Hamburg have also given a factor much smaller.

The labor of the wind-force committee of the Royal Meteorological Society, referred to in my previous note (*Science*, xiii. p. 171), has been directed mostly toward determining this factor for several anemometers of different patterns, which are as follows: —

	Arms.	Diam. of Cups.
	Inches.	*Inches.*
Kew Standard	24.00	9.0
A 19	5.80	4.0
A 21	6.75	2.5

These were placed near the end of a long arm of a whirling apparatus, moved by a small steam-engine with varying velocities. The number of turns of the anemometer compared with that of the whirling apparatus during any given time of uniform velocity, the relations between the length of the arm of the anemometer and the distance on the arm from the centre of whirling being known, gave the ratio between the velocities, and so the value of the constant, which is found to be about the same for all, except very small, velocities.

The average of 58 experiments with the Kew Standard gives 2.15 instead of 3 for the value of this constant. From 51 experiments made with A 19, the value 2.51 was obtained, while the average of 49 experiments with A 21 gave 2.96, which is very nearly that determined by Dr. Robinson, and now in use. It is seen, therefore, that while the Robinson factor is very erroneous for the Kew pattern, and also for A 19, but especially the former, it is very nearly correct for A 21. The use of the factor 3 for all patterns of anemometers now for nearly thirty years has introduced a great amount of error in published wind velocities; so that they are not only not comparable generally with one another, but the errors have likewise affected most, if not all, the results obtained from the discussions of these velocities. It is much to be regretted, therefore, that some standard pattern had not been adopted and its constant accurately determined at the start, instead of deferring it for nearly thirty years; for, if this is even now done, it will be a long time before any adopted standard and its true constant can come into general use.

Since the force of the wind is as the square of the velocity, errors in the estimated velocity of the wind give rise to errors in the pressure of the wind which are proportionately more than twice as great. For instance: if the true velocity of the wind is 30 miles per hour, the Kew Standard with its factor 3 makes it 42 miles nearly, an increase in the ratio of 1 to 1.4; but the force of the wind is increased in the ratio of 30^2 to 42^2, or as 1 to 2 nearly, and so in a ratio more than double the preceding one. In estimating the force of the wind from the indications of the anemometer, the effect of the error in the factor 3 of the anemometer, and of the wind-pressure constant .005, now in general use, are both in the same direction; so that the combined errors of both are very great. For instance: in the case of a wind of 30 miles per hour, we have seen above, that the error of the factor 3 applied to the Kew Standard increases the force of the wind in the ratio of 1 to 2; and if the wind-pressure constant should be .003 instead of .005, then the effect of both errors is to increase the estimated force of the wind above the true force in the ratio of 1 to $2\times\frac{5}{3}$, or to more than three times the real force. Of course, this is an extreme, but not an impossible case; for in anemometers mostly used the error of the factor 3 is not nearly so great as for the Kew Standard, and the true value of the wind-pressure constant may come out a little more than .003 when accurately determined, but still the errors of estimated wind forces, with the constants in use, are undoubtedly enormously large. Mr. Whipple, of the wind-force committee, says, that, "unless the Robinson anemometers could be put into the hands of those who would take care of them, their indications were frequently worse than useless. The instruments require to be continually looked after. Even if carefully attended to and regularly cleaned and well oiled, their records are far from satisfactory."

It is the opinion of the writer that they must in time give way to something better, probably to Mr. Dines' newly invented helicoid anemometer, which is more simple in its mechanical action, and, according to the experiments made with it, seems quite satisfactory. A description of this instrument is found in the *Quarterly Journal of the Royal Meteorological Society* for July, 1887.

WM. FERREL.

The Soaring of Birds.

THERE can be no doubt that the explanation of soaring given by Mr. Gilbert is mechanically sound. The only remaining question seems to be as to its sufficiency. In regard to this question, the following considerations may be of service: —

There is a certain velocity relative to the air such that a bird possessing it can be sustained against gravity without muscular exertion. Let V represent that velocity for a given bird. Let there be two horizontal layers of air, whose relative velocity is i. For simplicity, let the velocity of the lower layer be zero, that of the upper i. Suppose the bird at some instant to be in the upper layer, moving in the same direction with it, and with a velocity relative to it of V, so that he can just be sustained while moving horizontally. His velocity relative to the lower layer is $V+i$. Let him now descend into the lower layer and wheel horizontally 180 degrees. In so doing he necessarily loses some energy, and his velocity decreases. Now, in order that he may be sustained at the same level during the wheeling, his velocity relative to the lower layer must not fall below V. Suppose his decrease of velocity to be a little less than i; he will then be moving opposite to the direction of the upper current, with a velocity greater than V. He can therefore not only maintain his level, but can rise. Let him now enter the upper layer, his velocity relative to it being $V+i$. If, now, he can wheel horizontally through 180 degrees without losing more than the velocity i, he will be in a position to repeat the cycle.

The statement of Professor Oliver in *Science* (xiii. p. 16) seems to imply that the difference in velocity of the air-currents needs to be as great as the relative velocity which will enable the bird to sustain himself against gravity; that is, that i must be as great as V. If the discussion here given is correct, such is not the case. It is only necessary that the bird should have initially a sufficient relative velocity, and should be able to wheel horizontally 180 degrees without losing by "friction" enough energy to reduce his velocity as much as i, the velocity of one air-current relative to the other.

L. M. HOSKINS.

University of Wisconsin, Madison, March 5.

"Shall We Teach Geology?"

WHEN a reviewer bases critical verdicts on ignorance or misapprehension of the work reviewed, he has an advantage over the author, of which, in my own experience, I usually leave him in quiet possession. Still the meekness of silence may not always prove most useful to the public. Your reviewer of my work, "Shall We Teach Geology?" in No. 317, says that I ignore the mental and moral sciences as means of culture; but he should have observed that I do not undertake to discuss the education value of all sciences and literatures, but only of those selected as types by certain pedagogical writers who hold geology in disesteem. Your reviewer states that I mention "history only to slight it, declaring that it trains no faculty but verbal memory." My criticisms on history contemplate it as a study urged upon children in the early stages of education. This is what I have recorded on purpose to forestall such an accusation. "My present investigation concerns studies as usually taught and in schools of the lower orders. In college, history and literature are pursued in a nobler and more cultural way" (p. 148). Your reviewer employs the term "literature" in the wide sense, which makes it a much more valuable thing than literature as used in the narrow sense of the author, whose positions I am examining (note, p. 145). Your reviewer states, also, that I claim for geology that "the subject should be taken up in the primary schools, and pursued every year as long as the student attends school." This is preposterous criticism. Such is not my position, nor is the idea anywhere conveyed. I think the subject should be taken up briefly, two, three, or more times, at successive stages of mental development, not completed in one course late in school-life (see pp. 133, 134).

ALEXANDER WINCHELL.

Ann Arbor, Mich., March 5.

To keep Water-Mounts Moist.

IN my last communication on this subject (*Science*, xiii. p. 170) I recommended glass capillary tubes. I since find that a much simpler plan, and one that serves equally well in most cases, is to suspend from the edge of the cover-glass, to a beaker of water beneath, a moistened piece of filter-paper about four centimetres long and half a centimetre wide.

Likewise, in the study of germination of seeds, the capillary tubes or the moistened filter-paper may be put to good service. Very clean and satisfactory specimens of the first stages of germination may be obtained by placing the moistened seeds in contact one with another on a glass slip over a beaker of water, and suspending from their midst to the water one of the tubes or simply a narrow piece of paper. A bell-jar will exclude dust.

E. B. KNERR.

Parsons College, Fairfield, Io., March 6.

The Wind-Pressure Constant.

IN my note I see you have put Hazen for Hagen. The latter is a German physicist of Berlin. Will you please make the correction in your next number? This is important, since Hazen has also made experiments, the results of which differ very much from Hagen's, and it may seem that I have misrepresented his results.

WM. FERREL.

Kansas City, Mo., March 5.