The action suggested would clearly be to some extent *efficient;* but would it also be *sufficient?* Can the gradients of velocity, etc., from level to level in air-currents far above us, be rapid enough to give the bird more than a slight assistance, although perhaps he would still avail himself of even that little help? For here is much more than "friction" to be overcome. If a hawk, weighing a pound or more to every square foot of effective wing and tail surface, would fall through the air with outspread motionless wings ten miles an hour, which I believe to be a low estimate, then, to sustain his weight without muscular effort, his horizontal velocity relative to the surrounding air should be not less than fifteen miles an hour. Can it be that air-currents hundreds of feet from the earth, and only a few yards apart, often differ in velocity so much as that? And yet I have seen no other theory that could in the least explain the prolonged soaring of birds, if, indeed, their wings do remain motionless. Much has been said of late, in "Science" and elsewhere, of the locking of certain wing-joints: but this admirable contrivance for relieving muscular fatigue is quite irrelevant to the present problem; for, as Mr. Gilbert has recognized, it provides not a foot-pound of that actual work which, to prevent a fall of ten miles per hour, must be expended at a rate of thirty mile-pounds per hour if the bird weighs three pounds.

A naval officer, whose name I forget, once told me of an observation of his own, which, if confirmed, would remove all difficulties. Standing on deck in mid-ocean in a stiff breeze, he observed certain sea-birds hovering near him with apparently unmoving wings, though holding their own against the wind. But when they came within a few yards, he could see that the outspread wings had in reality a rapid motion of very small amplitude, almost a mere tremor. His impression was, that, though the wings had not sufficient play to present the whole wing edgewise to the air during the up-stroke, and flatwise during the down-stroke, yet the individual wing-feathers might be doing this, their vanes automatically separating and turning edgewise, opening like valves or like slats of a blind, as the wing rose, and closing up again, as it fell, by the action of the air itself. Some such action of the feathers would greatly aid this kind of flight. It is for naturalists to say how well they are adapted to it, and whether such flight might be possible while the wing-joints remained locked, and whether it might therefore be sometimes a restful change for the bird. J. E. OLIVER.

Cornell University, Dec. 19.

Cucullaris Propatagialis in Oscinine Birds.

SOME time ago ("Science," ix. 1887, pp. 623, 624), Dr. R. W. Shufeldt announced the alleged discovery in the bird-wing, of a muscle well known as *Cucullaris propatagialis*, as being particularly characteristic of the suborder *Oscines* (or, as I call them, the superfamily *Passeroideæ*), special stress being laid on its taxonomic value in distinguishing the latter from the mesomyodian *Passeres*. In a subsequent number of "Science" (x. 1887, pp. 70–72) I demonstrated that the muscle in question is particularly well developed in parrots and woodpeckers; and I also stated that I had found it, though in a rudimentary state, in some of our typical mesomyodian *Passeres*, notably in *Tyrannus tyrannus*.

Through the courtesy of Mr. Frederic Lucas, I have since been enabled to dissect a fresh specimen of the Nepal Hill-Myna (*Gracula intermedia*), a sturnine bird from India. I found the *Cucullaris propatagialis* quite as rudimentary as in the *Tyrannus* alluded to. As *Gracula* undoubtedly belongs to the *Oscines*, it has been fairly demonstrated that the muscle in question is neither peculiar to the *Oscines*, nor especially characteristic of them.

In looking further into the literature, I find, also, that Fürbringer has recorded the same muscle as being rudimentary in the following oscinine birds, — *Lamprotornis insidiator*, *Pastor roseus*, *Myiagra cærulea*, *Ixos chrysorrhoeus*, *Copsychus macrurus*, and *Turdus pilaris*, — while in many others he found it but very feebly developed.

It is evident, therefore, that this variable muscular slip has no taxonomic value whatever in the direction indicated by Dr. Shufeldt. I even doubt whether it will be found of much service in defining trenchantly even families or smaller groups, since every possible gradation between the rudimentary stage and the most highly developed condition seems to occur within the same group of unquestionably nearly related birds.

LEONHARD STEJNEGER.

Smithsonian Institution, Washington, D.C., Dec. 20.

Answers.

39. ORIGIN OF FISH IN ISOLATED PONDS. — If no one else will answer Mr. C. B. Palmer's question, let me point out that nothing seems simpler than that birds, lighting on the edge of first one pond, then another, should carry on their feet the eggs, larvæ, or whatever it may be, of one to the other. In digging wells in a quite desert region in Arizona, many miles from other wells, I was at first surprised to find them peopled after a short time with animals (frogs, if I forget not) which could not possibly have hopped or crawled from the nearest water, across the burning sand in midsummer, with the thermometer rising above 115° F. But I soon saw the above easy explanation. HENRY M. HOWE.

Boston, Dec. 22.

40. FELSPAR, OR FELDSPAR? — The note on the spelling of the word "feldspar," in "Science" of Dec. 14, is satisfactory with the exception of its closing sentence, which says that the form "felspar," although wrong, had been so long employed that "no one who prefers it can be criticised for using it." It should be added to this, that all other nations except Great Britain and her colonies, and also that ninety-nine hundredths of all mineralogical literature, spell the word with the *d* (or with the substitute *t* if the language requires it), and they do so because this is etymologically right; that the English drop the letter because the error in Great Britain has been persisted in until it has become *English;* and that such national prejudice is not a legitimate ground for scientific action even in Great Britain. Years since, the writer, thinking, like many others, uniformity in scientific nomenclature very desirable, sent a short paper, giving the British history of the word, to the "London Philological Magazine," which was accepted, and published anonymously as was requested. But national prejudice proved to be superior to all other considerations. In this country the prejudice has no right to a place, and the transplanting of its effects should not be allowed without a protest. J. D. D.

41. THE "SUPERNUMERARY MOLAR" IN MAN. — As a partial reply to Query 41 in "Science" of Dec. 7, permit me to state that Dr. Shufeldt will find in skull No. 1327, of the Morton collection in the Philadelphia Academy of Natural Sciences, the finest specimen extant of molars posterior to the third or "wisdom" teeth. It is some years since we saw it, and then "as through a glass" only; but our recollection of it is that the superior fourth molars are quite through the alveolar process, while the inferior are just seen from above in their course to the surface. The superior fourth molars are not "peg-like," but molar-like, though smaller than their neighbors. The specimen is Australian. We have a personal acquaintance who has eighteen teeth in the upper jaw; the "extra" teeth, one on each side, posterior to the third molar or "wisdom" teeth. Like Dr. Shufeldt's specimen, these are "conical, peg-like," as to form of crown. According to dental writers, the African races seem specially favored in this matter. For the best account of this structure in man (assuming it to be "supernumerary"), reference is made to the late Dr. M. S. Dean's translation of Magitot and Segros' "Dental Follicle," in which it is made to appear that the setting of an epithelial structure, and the enamel organ, determine the fact and position of the future tooth. The same process is made to account for "supernumerary" teeth elsewhere in the maxillæ, the anterior part of the upper being particularly favored. As to its significance, facts are accumulating that seem to point to the weeding-out of the third molar or "wisdom" tooth, the number and importance of the facts being directly as civilization. Such being the case, is it unreasonable to suppose this occasionally-cropping-out fourth molar other than a reversion to a past type, and to a time in the history of man when mastication was the primary, and not as now, in the civilized world at least, a secondary function? L. E. J.