the simple theory the best pipe of all would be one having the chimney one-fourth the length of the body and 0·337 of its diameter. The resulting series would be

1, 6, 8, 13, 15, 20, 22 . . .,

that is, $n=7$, with a fundamental frequency of $1\frac{1}{7}$, that given by the body of the pipe, without a chimney, when closed. The tone of such a pipe would be very smooth, with a gentle ringing quality, and none of the open diapason richness given by the octave, twelfth, and double octave, and also none of the dull nasal quality exhibited by the closed diapason when the odd harmonics are present in excess.

The theory, moreover, offers an explanation of a phenomenon which puzzled organ-builders greatly on its discovery. It was found that if the chimney was turned inwards, and faced downwards instead of projecting outside the body of the pipe, neither pitch nor quality was altered. An examination of the formula shows that this would be so, the stationary vibration in the chimney being then composite.

We must also realise that the simple theory must be modified by the introduction of the end-corrections required at the mouth of the pipe and at the upper open end of the chimney. The correction at the mouth is large and amounts to nearly twice the depth if the pipe is of rectangular section (Brillouin), or three and a third times its radius if cylindrical (Cavaillé-Coll). In the case of the Rohrflöte, the correction is increased by the presence of the ears. The correction required at the upper end of the chimney is of unknown magnitude, although it would probably be proportional to the radius of cross-section of the chimney. The difficulty lies in the experimental fact that very little sound indeed is propagated from the open end of an open labial pipe, if the mouth is of the simple bay-leaf type and not inverted. Hence the open-end correction, which is required by the transmission of stationary vibration to spherical propagation, may be imaginary in this case. If, however, the mouthpiece of the pipe were removed and resonance determined by tuning-forks, both open ends would require a correction as given by Lord Rayleigh's formula.

Enough has perhaps been said to indicate the interest of the problem and how well worthy it would be of careful experimental investigation.

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A Sidelight on Bird Migration.

IT is usually accepted as an axiom of ornithology that the annual migration of birds consists of a movement from winter-quarters to breeding-quarters and back. The following notes, taken from observations made by me during the last five years on the North-west Frontier of India, may suggest that this view of migration does not entirely cover every case.

The crested lark (*Galerita cristata*) and the common pied bush-chat (*Pratincola caprata*) appear to come to the Peshawar valley to breed, and, at any rate in some cases, to leave the valley as soon as their young are able to fly, and spend the hottest months in the nearest hills. Between the end of August and the middle of September both species return to the valley. The movements of the two species are not identical, and must therefore be described separately.

Crested larks may be seen in the Peshawar valley at all seasons. During the winter months they congregate in small flocks. About the beginning of March the number of flocks begin to increase. By the end of the month most of them have paired, and during the first few days of April the number of larks seems to decrease. During April the eggs of this species may be found in every field and on every piece of waste ground. The earliest date on which I have found a nest containing eggs in this district is April 1, and the latest April 29. By the end of May, crested larks have become scarcer in the valley than they are at any other season. This scarcity continues until about the end of August, when the numbers again greatly increase. About the middle of October a slight decrease again takes place, after which they appear to remain constant until the cycle of movement recommences in March.

No common pied bush-chats appear to remain in the Peshawar valley for the winter. The earliest date on which I have noted the appearance of this species is Feb. 28. The majority of this species seem to lay in April. The earliest date on which I have found a nest of this species containing eggs is April 7, and the latest (except for two nests containing hatching eggs found on July 11, 1922) is May 17. By far the majority of eggs are laid during the second and third weeks of April. By the end of May, bush-chats, like the larks, become scarce in the valley, about the middle of September their numbers again increase, and soon after they leave for the winter. The latest date on which I have observed them is Oct. 1.

During the summer months, when crested larks and pied bush-chats become scarce in the Peshawar valley itself, their numbers undergo a considerable increase on the hillsides below Cherat, at elevations varying from two thousand to three thousand feet above the general level of the valley. The large proportion of young birds to be seen on these hillsides at this time is very striking, and one is almost irresistibly led to the conclusion that these two species, like the human inhabitants of Peshawar, have taken their families to the hills.

From the foregoing observations the following two deductions are, perhaps, permissible :

First : that the common pied bush-chat, coming from somewhere "down country," breeds in the Peshawar valley, spends, in a great number of cases, the hottest months in the hills, and returns to winter-quarters through the Peshawar valley.

Secondly : that some of those crested larks that breed in the Peshawar valley also go to the hills for three or four months after breeding.

If this is a true reading of the facts, is it not possible that other species of birds also may migrate between three localities instead of the more usual two ? The matter would bear the attention of all observers of birds.

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Rockets and High Altitude Research.

A RECENT book review in NATURE for August 23, 1924, page 270, of Herr Oberth's "Die Rakete zu den Planetenräumen," 1923, calls for a brief comment.

Following the tone of the book, Oberth is credited with being more ambitious than the writer of this comment, in suggesting the construction of a rocket large enough to carry passengers, even to the distances of the planets, whereas my main object is presented as the sending of a rocket to the moon, propelled by successive charges of nitrocellulose.

I first became seriously interested in the general problem of high altitude research in 1899, and in 1907 I submitted, *for publication*, an article discussing the use of heat for expelling material at a high velocity, in order to furnish a propulsive force sufficient to permit navigation of interplanetary space.

The idea of using hydrogen and oxygen in multiple rockets was conceived in 1909; the general theory was put in satisfactory form in 1912; and the use of these substances mentioned in Smithsonian Miscellaneous Collections, Vol. 71, No. 2, for 1919. Oberth, on the other hand, claims in his book to have made his first finished plan of a rocket to use wet gun-cotton in 1909, and to have drawn up his first plan of an hydrogen-oxygen rocket in 1912.

Further, as to the use of liquid propellants and continuous combustion, these were suggested by me, *by publication*, in 1914, and were *tested experimentally* in 1921, the development carried on under my direction since that time having been confined entirely to rockets of this type.

It may be mentioned here that a request was received from Herr Oberth, dated May 3, 1922, stating that he had been working for some years on the problem of passing over the atmosphere of the earth, and requesting me to send any books I might have on the subject. Compliance was made by sending a copy of the above Smithsonian publication; and a copy of Herr Oberth's monograph, which incidentally deals with theory rather than with experiments performed, was received on July 19, 1923.

As to the idea of a passenger-carrying rocket, and the reaching of planetary distances, it is only fair to say that, while I have considered these matters with much care, and have gone so far as to make laboratory tests to check my conclusions, I am now, and always have been, only too well aware of the conservatism of the average person regarding new applications of physics. Thus, because it has been my desire to make actual progress and to conduct actual experiments, I have endeavoured, so far as possible, to focus attention on the problems that lie immediately ahead, the first of which is an exploration of the earth's atmosphere, and have restricted the discussion of certain sensational, but nevertheless interesting and realisable, matters to confidential reports.

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Five- (and Six)-Point Support: "Right as a Trivet."

As practical problems are usually a function of several variables, individual opinions are determined by the order of importance attached to the variables. The more numerous the conditions, the truer becomes the old expression, "Quot homines tot sententiae."

If in this question of fits we place pure mathematical and geometrical principles first, we have one point of view. As there are others which I think largely determine the workshop attitude and possibly the "Enfield Tradition," I should like to refer to some of them very briefly; to do so fully is not possible in a letter.

Let us consider the example from Thomson and Tait quoted by Sir George Greenhill in his letter in NATURE of September 27. It is stated therein that "the rifle may be replaced any number of times in precisely the same position." If much accuracy is demanded, that statement is not correct, because the materials at the points of support, where the forces of impact may be considerable, swage or wear away and quickly so when the parts are handled as they must be in the workshop.

In designing our apparatus we must consider the physical properties of the materials as well as the geometrical principles involved. The materials determine the areas over which the loads must be distributed and, if they cannot be approximately points, what value are we to attach to the principles of geometrical fits?

To demonstrate the practical application of these principles Thomson and Tait selected a bad example; as in the case of a rifle the convex abutments cannot be placed in their more proper geometrical position within the bore. Suppose a cylinder mounted in the manner described has longitudinal freedom and is constrained transversely: the slightest bend of the cylinder under its own weight, or by the action of the sun on one side of it, may destroy the longitudinal freedom, as the circular section of the fixed abutments must then accommodate an elliptic section of the piece.

The geometrical principle is based on the assumption that *all* the parts involved are absolutely rigid. We practical men, whose business it is to understand the limitations of materials, dare make no such assumption. In the workshop most of us are thoroughly familiar with geometrical principles. We were taught them in the universities and colleges. If we do not slavishly adopt them, it is generally with good reasons.

There has been within the past few years a good deal of adverse criticism of the practical trial-and-error type of man in industry, the articles having usually for their text "The Neglect of Science by Industry." If the writers of these articles were better informed, I feel sure they would find the converse text "The Neglect of Industry by Science" a more appropriate one.

JAMES W. FRENCH.
Anniesland, Glasgow, W.2,
October 7.

Fine Structures in Non-Hydrogenic Atoms.[1]

FOR some months we have been studying the data on fine structures of the lines of non-hydrogenic atoms. These fine structures arise, in every case so far studied, from transitions between the components of complex spectral levels. Save for a few isolated examples, it can be readily proved that fine structures are not due to isotopy. (See Scientific Paper No. 490 of the Bureau of Standards.)

It is necessary in certain cases to introduce a *fine quantum number f*, the values of which characterise the different components of a complex level. In some elements, a selection principle for this quantum number makes its presence felt, and the separations of the components are in integral ratios. So far, attempts to establish intensity rules have failed, but this part of the work is not finished.

Nagaoka, Sugiura, and Mishima (*Jap. Jour. of Physics*, 2, 121, 1923) have published measurements of mercury fine structures which are precise to almost 10^{-4} A. These data have enabled us to analyse successfully the complicated fine structures of nearly all the mercury lines they studied.

The phenomena of fine structures are so diverse that we cannot hope to have a unified physical explanation for them. In isolated cases, such as the 4_4 terms of Al^+ studied by Paschen, they may be due to relativity. In other cases, the components of a complex level behave like a tiny multiplet, and the structure may depend on magnetism for its origin. In other cases, magnetic fields do not affect the fine structures, and we must look to unknown dynamical peculiarities of the individual element for an explanation.

A paper covering the whole field will soon be published.

ARTHUR E. RUARK.
F. L. MOHLER.
R. L. CHENAULT.
Bureau of Standards,
Washington, D.C., U.S.A,
August 23.

[1] Published by permission of the Director, Bureau of Standards. of the U.S. Department of Commerce.