*Observatoire National, Paris, XIV^e^*, or to the undersigned.

They should arrive not later than the beginning of March.

R. A. SAMPSON,
President, International Time Commission.
Royal Observatory, Edinburgh,
December 10.

Experiment and Philosophy.

THIS letter is a dogmatic rejoinder to nobody in particular.

There is a wise Scots saying which warns us against showing half-done things to "bairns and fules." It ought to hang as a text by the bedside of every scientific researcher, with a note appended that "bairns" is to be glossed "non-scientific thinkers and the educated public." The Great Educated Public thirsts, very properly, for knowledge; but it loves best, if it can, to get the wine of learning all raw from the presses, dispensed by the vintners themselves, and to suck it up through the sweet straw of analogy. Of late years there has been a remarkable indulgence of this crude taste for "the last word in science"; and one immediate result has been that a good many metaphysically-minded folk have hastily engorged certain possibilities, perceived and put forth by men of science to be further tested by men of science, but forming draughts too heady and unmatured for the novice. Dazzling themselves with the tentative speculations of science, such pseudo-philosophers comfortably conclude—see them at it in any of the current reviews—that, after all, the scientific method can now be proved (with the aid of its own results!) to be merely argument in a circle.

So: all is well; the lamb of mysticism can now lie down with the lion of science, for the poor lion is exposed as nothing but a cud-chewing ruminant. Surely it is time that the lion gave a gentle roar or two, if only as a salutary reminder to the lamb who skips up bleating for more room than the lion is ready to concede?

The root of the trouble seems to lie in this: that non-scientific people are in such impetuous haste to "know" (as they are pleased to call the process of "being informed") that they will not understand the need to wait patiently for years—in large questions, for centuries—before suspicions now private to first-hand inquirers can be turned into public assertions. "Here's the latest," they seem to say; "let's have it; fling away what went before it and what went to make it." Yes; and fling away *this* latest, too, in a little while, so soon as real science shall have caught up with it and have made it a trifle dull and *démodé* because surer and better-balanced; and fling away, at the ghost of a permissive hint, whatever may any more obscure the glorious verities of untutored guessing. "How much better and finer it is (besides being much less trouble) to be able at last to ignore, as of old, the figments of science! (Of course, we don't mind using soap, we don't mind telephoning to our doctor to come quickly in his car to diagnose and cure our ailments, or to the surgeon to save our lives and minds—*these* scientific figments we tolerate, for we *like* them!)"

All this would not matter very much—the unlearned will perhaps know better in a few generations—if it were certain that it will not prejudice experimental science. But we buy a book purporting to teach the experimental man of science the inwardness of his business; we expect, perhaps, a helpful exposition of Whitehead's sane, lucid, and logical "Pan-physics," or else the elements of logical inference; but we get a confounding blend of two or three universes of discourse, an ambitious and premature synthesis of the only partially-analysed, which leads the working human reader into a barren and impotent finality.

The motto of this journal, Wordsworth's sentence, surely abides, nor can it be wrested from its meaning by crying (with the materialists of last century) that Nature includes all mind. No experimenter holds, in his inmost soul, the converse; namely, that mind includes all Nature, has indeed created it; nor will he admit, for generations still to come, that our real knowledge yet suffices to probe the vast theorems that our hurrying philosophers broadly accept as already settled. Of those lugubrious thinkers, those reactionaries whose claim once again is *Totam Asiam peragrare*, and who seek to cast three hundred years of patiently-won lore into the melting-pot, to gain the Indies out of the crucible: of these none are experimenters. They are like the jaded urban hero in the story, whose regeneration depends on his coming "close to the green and growing earth"; they have attenuated honest sense-facts by unremitting abstractions to the *n*th degree; they have lost the last touch with reality.

The working man of science at least has his feet well planted in the solid ground of observation; and if the soaring philosophers accuse him, on the contrary, of having his head too tightly embedded in that same ground, let him at all events show them that his protruding extremities nevertheless have not lost the power of vigorously kicking.

THOMAS HOBBES, Junior.

A *pp′* Group in the Arc Spectrum of Zinc.

ONE of us (R. A. S.) in a study of *pp′* groups in atomic spectra, which will be published in full later, has observed that in many two-valence-system spectra the frequency of the first *pp′* group is nearly a mean between the frequency of the first line of the principal series of singlets in the spectrum and the first line of the principal series of doublets of the once more ionised atom.

The rule holds for the *pp′* group in cadmium given by Ruark (*J.O.S.A.*, vol. 11, 1925, pp. 199) where we have

Cd I, $1S - 2P$	$\nu = 43691$
Cd I, $2p_1 - 2p_1'$	$\nu = 44088$
Cd II, $1s - 2p_1$	$\nu = 46618$

We were accordingly led to apply this rule to zinc where an exactly analogous group to that in cadmium was easily located.

The complete cadmium group as given by Ruark is:

λ.	Int.	ν.	Classification.
2329·27	10*R*	42918·6	$2p_2 - 2p_1'$
2306·61	5*R*	43340·3	$2p_1 - 2p_0'$
2267·46	5*R*	44088·6	$2p_1 - 2p_1'$
2239·86	5*R*	44631·7	$2p_0 - 2p_1'$

$\Delta p_1p_2 = 1170{\cdot}1$, $\Delta p_0p_1 = 543{\cdot}1$
$2p_1' = -2206{\cdot}6$ cm.$^{-1}$.

The group is anomalous in that only four lines appear instead of six lines as in a normal *pp′* multiplet, but the classification is fixed by the known Zeeman pattern of the line $2p_1 - 2p_0'$.

In zinc we find a similar group of four lines which we have classified as follows. The wave-lengths of Eder are used.

λ.	Int.	ν.	Classification.
2104·34	2	47505·7	$2p_2 - 2p_1'$
2096·88	2	47674·7	$2p_1 - 2p_0'$
2087·27	2	47894·0	$2p_1 - 2p_1'$
2079·10	2	48082·2	$2p_0 - 2p_1'$

$\Delta p_1p_2 = 388{\cdot}3$, $\Delta p_0p_1 = 188{\cdot}2$
$2p_1' = -4629$ cm.$^{-1}$.

This group obeys the rule stated in the first paragraph as follows :

Zn I, $1S - 2P$	$\nu = 46745{\cdot}8$
Zn I, $2p_1 - 2p_1'$	$\nu = 47894{\cdot}0$
Zn II, $1s - 2p_1$	$\nu = 49354{\cdot}7$

It also has within reasonable limits the separations of the known $2p$ levels of zinc which are :

$$\Delta p_1 p_2 = 388{\cdot}9,\ \Delta p_0 p_1 = 189{\cdot}8.$$

The Zeeman pattern of none of these lines is available so far as we can find.

We have been permitted to examine these lines on a spectrogram taken with a Hilger E 1 quartz spectrograph by Dr. R. V. Zumstein, National Research Fellow, at the University of Michigan. All four lines are sharp and like in appearance, although λ2087 appears to have a close diffuse companion barely resolved from it on the short wave-length side. There is a fifth line which might conceivably belong to the group, although its diffuse character would seem to exclude it. This line is

λ2070·11 Int. 1 ν48291·2

If this line were classified as $2p_1 - 2p_2'$ and the short wave-length companion of λ2087 classified as $2p_2 - 2p_2'$, we should then have a normal pp' triplet group. In view of the fact that λ2070 is diffuse and quite different in appearance from the first four lines, we hesitate to make this assignment. It may be said in favour of it that the diffuse companion to λ2087 mentioned above might be interpreted to mean that p_2' has a diffuse nature. We know, however, of no similar example of this sort.

RALPH A. SAWYER.
NORMAN C. BEESE.

Physical Laboratory,
University of Michigan,
Ann Arbor, Michigan,
November 6.

On a New Device for the Study of the Compton Effect.

ACCORDING to a recent tentative suggestion of W. R. Smythe, a study of the intensities of the Compton modified scattered X-ray spectrum lines might serve to prove whether the electrons effective in the scattering which produces the modified radiation are completely " free " or whether they are " bound " to atoms of the scattering substance. If " modified " scattering is produced by free electrons only, then the intensity of the modified line should be jointly proportional to the intensity of the incident radiation (number density of radiant corpuscles) and to the number of free electrons present. But this latter is itself at least roughly proportional to the intensity of the incident radiation, since it is safe to assume that most of these free electrons are rendered so by the photoelectric action of the incident X-radiation. Hence the intensity of the modified scattered line should vary roughly as the square of the incident intensity, or at least as some greater power than unity of the incident intensity.

Up to the present date, so far as I am aware, it has been necessary to study the Compton effect by very greatly prolonged X-ray exposures, or by ionisation chamber methods requiring extremely powerful X-ray tubes for the incident intensity, and very sensitive electrometer methods for detecting the presence of the lines. This is largely due to the fact that the original radiation must be scattered twice in succession, once to obtain the effect and once at an analysing crystal. The photographic exposures are frequently prolonged to 100 or 200 hours' duration and occupy a good portion of the life of an X-ray tube.

In order to investigate Smythe's suggestion, and for other purposes, I have constructed an X-ray tube of special design in which both scattering substance and analysing crystal are contained in a small metal box mounted on the end of the anti-cathode. The distance from the source of primary X-rays to the scattering material is so greatly reduced that the radiation available for the scattering is of the order of two thousand times more intense than that heretofore available. *It has been found possible to produce photographic spectrograms of the Compton effect with this apparatus in thirty seconds that would by previous methods have required a hundred hours' exposure.* Very moderate currents such as 5 ma. suffice for exciting the primary X-radiation. The possibilities of such an apparatus for high dispersion studies and many other purposes will be immediately evident to any one familiar with this field of work.

Preliminary studies with the new " target spectrometer " seem to show very little evidence for any intensification of the Compton modified lines greater than a strict proportionality to the incident intensity would require. It is too early, however, to decide this question with precision. If Smythe's tentative prediction is not eventually supported by experiment, then the theory that bound electrons are responsible for the observed " softening " of X-radiation scattered by light elements is strengthened.

Work is now in progress with the new apparatus which I hope will decide the above-mentioned question. A study of the fine structure of modified and unmodified lines by means of the new instrument is also under way. One of the novel features of the present target spectrometer is that it permits the study of radiation that has been scattered at an angle of almost exactly 180°. This angle offers several advantages in the study of the fine structure of the modified line.

JESSE W. M. DUMOND.
Norman Bridge Laboratory of Physics,
California Institute of Technology,
Pasadena, California,
October 28.

Genes and Linkage Groups in Genetics.

I WOULD like to ask Prof. MacBride to clear up one point in his review in NATURE for November 28, p. 776, for otherwise those who are not geneticists may be led astray.

Prof. MacBride writes of " the crude conception of the linear arrangement of the genes in the chromosome, and the idea of crossing-over." He is here, however, confusing two distinct points : (1) the linear arrangement of genes within linkage-groups, and the phenomena of partial linkage ; and (2) the identification of the linkage-groups with the chromosomes. Even if the latter were disproved, the evidence for the former would remain in its entirety, and it is with this alone that I wish here to deal.

Mendelian differences do occur ; some genetic bases for them exist (' genes ') ; genetic research, starting from Bateson's fundamental work on coupling and repulsion (' linkage ') has shown that they exist in groups, and that within each group they are in orderly arrangement ; and finally, Jennings has shown mathematically that a linear arrangement is the only one so far proposed which will fit the genetical results.

These seem to most biologists to be important facts ; and if Prof. MacBride will admit them, I should feel that there was a common basis for discussion. He does go on to discuss ' linkage,' but it is not clear to