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.

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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.

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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