Dr. Goucher's letter, however, is worthy of more serious consideration. His statement that our formula presupposes a type of slip which is inconsistent with experimental fact is surprising, since throughout our letter it was Dr. Goucher's own experimental results which were considered. It may be well, however, to review what assumptions were made. They were two, namely, that for a perfect wedge the atoms slipped on equidistant parallel planes and that the movement on these planes was equal. The only possible alternative to these assumptions would be that the slipping occurred on planes at different distances apart, but that then the extent of the displacement was in all cases proportional to the thickness of the slab of metal between one plane of slipping and the next. Such a supposition is so absurd as to carry its own refutation with it. Unless then all present ideas of the plastic deformation of metals are entirely inaccurate, and the displacement does not occur by slip, there is no alternative to the only two assumptions we have made.

Our analysis was concerned with one part of the wedge only, and there is no question of a difference in wedge angle between one half of the fractured test-piece and the other. Dr. Goucher emphasises what he had already found, namely that the wedges had an included angle of 39° or more than 50°. We are quite unable to see why he should imagine there is any difficulty in this, since not only have we considered the fact but have even offered a reasonable explanation of why it should be so. He complains further that we have not taken into consideration the fact that the larger wedge angles were found only with test-pieces whose {112} planes were slightly asymmetric with regard to the direction of the stress. The reply is clear. We have not been concerned with the explanation of the deformation of the crystal. All we attempted to do was to discover, when a certain deformation had been produced, what was the nature of the atomic displacement in the final wedge. That asymmetry of crystal is necessary for the production of a blunt wedge is merely an experimental fact which has no bearing on our analysis of the deformation when it had actually been effected.

We would point out again that our letter offered a reasonable explanation and, incidentally, the only comprehensive one yet suggested, of all the experimental results which Dr. Goucher has obtained in his most excellent experimental work. Until, therefore, something more satisfactory is offered it must take precedence over an explanation which, as a result as it seems to us of a perfectly arbitrary assumption, is capable of explaining half the results only, leaving the remainder as inexplicable anomalies.

May we in conclusion point out that in the formula in our original letter a misprint occurs, and that $2/\phi$ should be $\phi/2$?

W. E. W. MILLINGTON,
F. C. THOMPSON.

Manchester, August 24.

The Ionisation Potential of O II.

PROF. A. FOWLER has worked out (*Proc. Roy. Soc.*, **110**, 476, 1926) with much elaboration the main spectral characteristics of O II, but has not been able to obtain the fundamental levels because the jumps to them produced lines which were below 700 Å.U. With our high resolution hot-spark spectroscopy for the extreme ultra-violet and assisted by the new methods of identification of multiplet structure recently worked out by Russell, Hund, Heisenberg, and Pauli, and applied by R. H. Fowler and D. R. Hartree (*Proc. Roy. Soc.*) to the classification of Fowler's O II terms, we have succeeded in accurately fixing these fundamental levels as is shown in the accompanying table. Russell assisted us in this search by placing at our disposal his unpublished identification of the quartet p'' level, which should replace the x_3 level in Fowler and Hartree's classification.

The knowledge of this ionisation potential of O II should be useful in fixing the temperatures of certain stars.

I. S. BOWEN.
R. A. MILLIKAN.

Norman Bridge Laboratory of Physics,
California Institute of Technology, Pasadena.

TABLE I.

Series Lines of O II.

Int.	λ I.A.Vac.	ν	Δν	
		Quartet System.		
1	429·97	232574·4		$as - p''$
3	539·067	185505·7	157·1	$as - ap_3$
2	539·524	185348·6		$as - ap_2$
1	539·837	185241·1	107·5	$as - ap_1$
		Doublet System.		
c	440·51	227010·		$x - bp''$
1	441·97	226260·		$x - cd'$
2	481·56	207658·		$x - bd'$
2	484·00	206612·		$x - ap''$
2	485·56	205948·		$x - f$
4	616·325	162252·	185·	$x - 2p_2$
4D	617·030	162067·		$x - 2p_1$

Term Values.

$x(p'$ or $d) = 256202\cdot$
Assuming $ap_3 = 97100\cdot$[1]
$as = 282605\cdot9$

This corresponds to the ionisation potential 34·88 *volts.*

To the foregoing interesting communication, I think it may be usefully added that the designations and values of all but one of the doublet terms, apart from x, are those given in the paper by myself to which reference is made, namely :

$\left.\begin{matrix} ap_2'' = 49590\cdot80 \\ ap_1'' = 49476\cdot81 \end{matrix}\right\}$ $\left.\begin{matrix} bd_2' = 48618\cdot42 \\ bd_3' = 48565\cdot45 \end{matrix}\right\}$ $\left.\begin{matrix} 2p_1 = 94132\cdot52 \\ 2p_2 = 93952\cdot53 \end{matrix}\right\}$

$\left.\begin{matrix} bp_1'' = 29231\cdot39 \\ bp_2'' = 29229\cdot03 \end{matrix}\right\}$ $\left.\begin{matrix} cd_2' = 29974\cdot67 \\ cd_3' = 29972\cdot55 \end{matrix}\right\}$

The f term ($= 50273\cdot5$) was afterwards provisionally identified as such by Fowler and Hartree from lines which I had somewhat doubtfully indicated as involving a term xd_2'. The writers of the letter appear to have overlooked the fact that Fowler and Hartree had already identified my x_3 term as the unresolved first p'' term of the quartet system.

While the value indicated for the as term is probably not much in error, it should be understood that the value of ap_3, on which it depends, remains somewhat uncertain. A Rydberg formula for ap_3 and bp_3 gives $ap_3 = 98850$, but this can only be regarded as an approximation, since only the first two terms of the sequence are known. A more trustworthy value would result if intercombinations between the doublet and quartet systems could be traced.

A. FOWLER.

[1] A. Fowler assumes $ap_3 = 100{,}000$. We have changed to the foregoing value in order to bring the Ritz formula for the sequence of ap and bp terms into better agreement with that for the corresponding terms of N I.