the other *coated inside* with a thin film of pounded charcoal. The latter was most affected.

In a third experiment, a frame of glass threads, or fine wire, placed vertically, was coated with a film of diluted white of egg, applied with a broad hair brush, and kept constantly at the same temperature by applying it fresh and fresh. Then, exposing a heated ball on one side, and an air-thermometer on the other, no effect was sensible on the latter, when the temperature of the ball was low, but when just invisible in the dark the effect was very sensible.

The author also finds that this effect is greater than in the case of glass, and that liquid screens are more permeable to heat than solid ones. He also found that little difference of effect is observed whether the screens be near to or far from the heated ball, *cæteris paribus*; and this he considers as demonstrating that the effect was not due to secondary radiation from the screen.

On the Derangements of certain Transit Instruments by the effects of Temperature. *By* Robert Woodhouse, *A.M. F.R.S. &c.* Read April 26, 1827. [*Phil. Trans.* 1827, *p.* 144.]

In the Philosophical Transactions for 1825, the author alluded to the derangement of the Cambridge transit instrument, arising from unequal expansion of its braces, establishing, as he conceived, the fact and cause of such derangement; and in a subsequent paper instanced its effect in one case as altering, by no less than 20″, the time of the passage of the pole star over the wires. In consequence the removal of the braces was resolved on, but from one cause or other delayed; but the author considers good to have arisen from this procrastination, as enabling him to make further experiments, which he was led to do in consequence of Mr. South's observations, which lead to conclusions opposite to those deduced by himself. To satisfy his own mind, therefore, he instituted the series of experiments described in this paper.

His first care was to determine precisely, by a series of transits, the polar intervals between the wires of his eye-piece. He then observed the pole star at its lower culmination, and after its passage over the middle wire, applied a warm blanket to the upper eastern and lower western brace, and found that a deviation of the telescope to the west had taken place, such as to alter the passage over the remaining wires nearly 19 seconds, and in the direction corresponding to the expansion of the braces. Another observation, under more favourable circumstances, gave a similar result; viz. 18 seconds of retardation.

In another experiment the warm blankets were applied to the upper western and lower eastern braces, when deviations appeared to have taken place to the extent of 36, 29, and 27 seconds in the respective passages over the 5th, 6th, and 7th wires.

In another trial the passages over the three first wires were ob-

served, and warm blankets were then applied to both the western braces, and a deviation to the west, though not above one third of that arising from their application to the alternate braces was the consequence, indicating the difference, not the sum, of the actions in the two cases; and in this experiment it is very unlikely that the two braces should have been equally heated.

The next experiment was varied by observing the passage over the 1st and 2nd wires, then warming the upper western brace, and observing it on the 3rd, 4th, and 5th wires, allowing the instrument to cool, then again warming the *lower* western brace to a yet greater degree, and observing on the 6th and 7th wires. The result was a deviation to the east, caused by the first warming, a return to its mean state, and then a deviation to the west from the second warming.

The experiments were varied by holding the braces some time in the hand, and with similar results.

From these details, the author concludes that the partial heating of the diagonal braces, or of any one of them, deranges the Cambridge transit instrument, according to the reasoning in his former paper; and that this cause may, in certain instruments, and under certain circumstances of temperature, produce balancing effects,—thus giving an appearance of inflexibility which, under other circumstances, would not subsist.

He then enters into a consideration of the circumstances of Mr. South's experiments, in which, as the braces on the same side were equally heated, the difference of temperature in the upper or lower parts of the tube alone could have operated, and might produce an insensible effect. He combats the idea that the apparent rigidity of Mr. South's instrument arose from its peculiar construction; as the Greenwich transit, which is similar, and by the same artists, has been found by the assistants, on holding the alternate braces in their hands, to undergo a considerable deviation by the test of the meridian mark. But to be more sure of the nature of the result produced, he requested the Astronomer Royal to try with the Greenwich transit the first experiment mentioned in this paper. He did so, and the results were found to be in accordance with the views entertained by Mr. Woodhouse, and are here stated. He concludes this subject by inviting Mr. South to an experiment, decisive, as he conceives, of the question as far as concerns his instrument, viz. to observe the passage of Polaris in October with one brace exposed to the sun.

On the subject of the sun's transits, he declares himself unwilling and unprepared to enter, and states himself to have found a difference between the clock's errors, as determined by the sun and the stars, similar to that concluded by Mr. South, and nearly the same in quantity. This, he says, may be partly explained by the increase of the right ascensions of the stars by three tenths of a second, in late catalogues by Mr. Pond, while no corresponding change was made in the catalogue from which the solar tables were computed.

He considers, on the whole, that the differences in question arise

partly from errors in the solar tables, and partly from instrumental derangement.

On some of the Compounds of Chromium. By Thomas Thomson, *M.D. F.R.S. L. and E. Professor of Chemistry, Glasgow.* Read March 29, 1827. [*Phil. Trans.* 1827, *p.* 159.]

The principal object of this paper is to give an account of a singular compound of chromic acid and chlorine, discovered some years ago by the author; but in the investigations to which it gave rise, the author was led to a more careful examination of the oxides of chromium than they had before undergone, and to a knowledge of their composition. An account of these researches he therefore proposes to give in this communication. He begins by describing metallic chromium. That used by him was reduced by Mr. Cooper: it was white, with a shade of yellow, very brittle, not sensibly attracted by the magnet even in fine powder. Its specific gravity was 5·093. Nitric acid boiled on it has no effect, and aqua regia scarcely any, unless the action be very long continued. When heated, however, with a mixture of potash and nitre, it is converted into chromic acid; 3·14 grains of the metal, thus treated, yielded by solution and precipitation 16·23 of chromate of lead, giving for the weight of an atom of chromium 3·966, or in round numbers 4·000. The author's stock of metallic chromium was so small as to prevent the repetition of the experiment.

The author next describes the green oxide of chromium. This is easily produced by the action of de-oxygenizing agents, such as alcohol, sulphurous acid, or sulphuretted hydrogen, on chromate of potash. When thus obtained it is in the state of a hydrate, containing $\frac{1}{5}\frac{3}{5}$ths of its weight of water, and easily soluble in acids. A moderate heat, however, expels the water, and leaves the oxide insoluble in any acid. When further heated nearly to redness, it glows, or becomes of itself suddenly intensely red hot. Its atomic weight cannot be determined from its salts, as it forms none,—at least crystallizable and definite enough for the purpose; but as we know that of chromic acid to be 6·5, if we can determine the number of atoms of oxygen to be abstracted to convert it into green oxide, that of the latter will be known. To this end the author deoxidized the chromate of potash by sulphuretted hydrogen. Hydrosulphuret of chromium, composed of its ingredients, atom to atom, fell in the state of a green powder soluble in acids. The liquor, after driving off the redundant gas by heat, was found to be a solution of hyposulphite of potash. To avail himself of this fact, however, it became necessary to investigate the composition of the hyposulphurous acid. This he effected as follows:—he first analysed a crystallized hydrosulphuret of soda, formed on a large scale in certain soda-leys, which he found to consist of 1 atom bisulphuretted hydrogen, + 1 atom soda, + 6 atoms water. Through a solution of this salt he passed sulphurous acid, which converted it into hyposulphite, and threw down just half