be, so far as possible, free from errors arising from unknown changes in liquid-air areas and from the unknown solid-air and solid-liquid tensions. It will be seen that the capillary-rise method in reality does this, though there is difficulty in measuring the internal bore and keeping it clean. But the following arrangement seems to be as nearly as possible free from these objections, and to be adapted to measure the tensions of interfacial liquid surfaces as well.

The liquid (mercury excepted) is made to drop from a fine capillary tube having thin walls as in Fig. 1. In forming the drop, it spreads upward over the exterior surface and reaches a limiting size, shape, and position. It then slips down the tube at a uniform velocity with little modification in size or shape, and after suffering a slight check in its motion breaks its connexion and falls. We may then equate the whole tension about the tube with the weight of the drop, so that $T_{la} \times \pi d_e = W_L$ or $T_{la} = W_L/\pi d_e$, where T_{la} is the liquid-air tension and d_e is the external diameter of the tube.

The same drop formation occurs when the tube is arranged to drop water downwards in any lighter oil, or by a bent tube (Fig. 2) upwards in a heavier oil. In this case we have the equation $T_{wo} = \frac{W_w}{\pi d_e} \times \frac{\rho_w - \rho_o}{\rho_w}$, where the suffixes w, o, e, and i denote the words water, oil, external, and internal, and ρ is density, from which the tension of the water-oil surface may be calculated.

On the contrary, when oil is dropped either downwards or upwards in water, it does not spread on any exterior water-glass surface, but forms its attachment as in Fig. 3. In this case we have $T_{wo} = \frac{W_o}{\pi d_i} \times \frac{\rho_w - \rho_o}{\rho_o}$.

These all give results agreeing with those recorded in the standard tables.

In accordance, then, with this conception of an elemental force not included in the class of "attractive" forces, we should expect that these changes in area would be always attended by a rise in temperature. If it be asked why the enveloping area about a free molecular mass tends to decrease, there is no answer; and neither is there an answer to the question as to why a large mass tends to approach another.

WILSON TAYLOR.

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The Influence of Science.

THE seeming contradiction in my summary account of the case of Galileo (NATURE, August 5, p. 180), to which Sir Oliver Lodge directs attention in his letter (NATURE, August 26, p. 277), needs an explanation. The great work of Copernicus (1543) was dedicated to a Pope, Paul III.; none of the Roman Congregations found any objection to it, and (Whewell, "History of the Inductive Sciences," I. 418, ed. 1847), says "lectures in support of the heliocentric doctrine were delivered in the ecclesiastical colleges." This was because of its being taught as a purely scientific doctrine.

Sir Oliver writes that Galileo "had endeavoured to get the Church to realise that the doctrine was not really antagonistic to Scripture when reasonably interpreted." This statement needs qualifying. Had Galileo contented himself with teaching the Copernican doctrine as a scientific hypothesis, he would not have been molested. But, being a fierce controversialist, he wanted to confound his many adversaries, the mathematicians, and the Aristotelians, by showing that Scripture was on his side (cp. Sir David Brewster, "Martyrs of Science," p. 58). Hence, disregarding the advice of the Bishop of Fermo "not to raise the question," and that, too, of many other ecclesiastical friends, among them Cardinals and Prelates, to the same effect, he demanded that "the Pope and the Holy Office *should declare the Copernican system to be founded on the Bible.*" *Hinc illae lacrymae.* On the other hand, the attitude of the Churchmen is well illustrated in a letter from Cardinal Bellarmine to the Carmelite friar, Foscarini, one of Galileo's friends, dated April 12, 1615. He writes: "If a true demonstration should be found that the Sun is placed at the centre of the world, and the Earth in the third heaven, and that the Sun does not turn round the Earth, but the latter round the former, then it will be necessary to proceed with great prudence in the explanation of Scripture, which seems to say the contrary, and rather to avow that we have not understood it, than to declare a demonstrated fact false." Astronomers had to wait until the discovery of aberration by Bradley before such a true demonstration was found.

Sir Oliver also writes that "Galileo was made to recant, to abjure, and curse the theory of the earth's motion." Whewell tells us (*loc. cit.*, p. 419), "He (Galileo) was accused before the Inquisition in 1615, but at that period the result was that he was merely recommended to confine himself to his mathematical reasonings upon the system, and to abstain from meddling with Scripture." After his contempt of court, in the second trial, of the year 1632, he was condemned as "vehemently suspected of heresy." He was sent to Arcetri, and had to recite a penance of certain prayers.

There was no implication in my former letter, as Sir Oliver writes, "that there was really no punishment, and that there was no call for anxiety and distress." The implication was that his troubles were largely, if not entirely, of his own seeking, and that his treatment was, according to the quotations I gave from Whewell, and from De Morgan, comparatively mild. I purposely quoted from non-Catholic writers, as they cannot be suspected of partiality towards the Roman Congregations. To these I add the testimony of Sir David Brewster (*loc. cit*, p. 88): "During the whole of the trial Galileo was treated with the most marked indulgence." Sir Oliver Lodge's quarrel is, therefore, with such eminent scientific men as Whewell, De Morgan, and Brewster (see also *op. cit*, p. 77), to whom we may add Huxley, who ("Life and Letters," ii. 424) avowed that "the Pope and the Cardinals had rather the best of it."

But my chief implication was, and is, that the case of Galileo cannot fairly be considered as evidence of the hostility of the Church to natural science, and as a hindrance to her legitimate influence. Finally, I trust Sir Oliver Lodge will not think me discourteous in not treating of the other points raised in his letter, as I do not consider them to be relevant to the present discussion.

A. L. CORTIE, *S.J.*

Stonyhurst College Observatory,
August 31.

The Production of a Standard Source of Sound.

I HAVE recently had occasion to consider the problem of constructing a simple standard source of sound, and have been favourably impressed by the possibility of employing a "hot wire" grid—as used in the Tucker microphone (Phil. Trans. A, vol. 221, pp. 389-430)—for this purpose. In this microphone the grid is made of fine platinum wire and heated by a current of 20-30 milliamperes. It is mounted in the orifice of a Helmholtz resonator, and when the latter is stimulated by a sound of suitable pitch, the vibration of the air in the orifice causes an increase