of two liquids. Moreover, as it has been shown that two free molecules of a gas cohere (coalesce) from this same cause, it is justifiable to conclude that in all cases down to molecular dimensions cohesion is simply a surface force pulling in the periphery of the contact area perpendicularly to that area. It may be remarked also that these results furnish no evidence of the so-called molecular attraction.

By calculation this force binding two molecules of water together is 2.05×10^{-6} dynes; of mercury, 7.7×10^{-6} dynes. These agree with Dr. Chatley's statement that (molecular) cohesion is of the order of 10^{-6} dynes. Again, in the case of two molecules cohering in this manner the enveloping force can have a range not greater than two molecular diameters, while for larger molecular masses the range may be as great as three or four molecular diameters. It thus fulfils the condition for the range of action. In comparison with their gravitational attraction this force binding two water molecules together is of the order 10^{18} times as great. It is of the same order as the electrical attraction of two oppositely ionised molecules just before they come into contact. It causes a pressure on the interior molecular mass of the same order as the intrinsic pressure of the liquid. It does appear, then, that one force can be conceived as having all the properties of cohesion.

The conception of this force as a cause of molecular phenomena appears to be, in fact, fundamental. It solves the problem of surface tension; it explains, as we have seen, both cohesion and adhesion, and it accounts satisfactorily (*Phil. Mag., ibid.*) for the latent heat of condensation. But besides all this there is the remarkable coincidence that the force itself is located, or performs its function, in the precise area bounding free mass that the phenomena of reflection and refraction of light take place and electrons have their movement. These considerations give point to Dr. Chatley's concluding words: "It would appear that a complete solution of the macroscopic properties of matter would also solve the question of the inner structure of the molecules and atoms."

WILSON TAYLOR.

Physics Laboratory, University of Toronto,
Canada, November 15.

The Resonance Hypothesis of Audition.

ADDITIONAL evidence in favour of the resonance hypothesis of audition has been found recently.

The study of long-distance telephony has shown that low-pitched notes travel more rapidly than do those of high pitch, owing to the impedance of the electrical circuits. Mixed tones must, therefore, arrive with phase relationships between the high and low tones quite different from those with which they started.

In spite of this, even such complex sounds as those composing human speech are found to suffer but small apparent change in quality and distinctness during transmission. And this statement appears to apply equally to wireless telephony, where similar changes of phase must occur.

These facts suggest that the ear responds to tones quite independently of their relative phases, and therefore that true harmonic analysis must take place in the organ of Corti. A survey of the different types of harmonic analysers used in physics, for tide production and the like, shows that such harmonic analysis is invariably performed by a series of some type of resonator. Presumably, therefore, since the ear can carry out harmonic analysis, it also must contain resonators.

To test the premises more thoroughly the following experiment was carried out:—

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Two electrically driven tuning-forks, emitting pure tones, were connected to separate battery and switch circuits, and were mounted on separate tables, so that while they vibrated independently their tones entered the ear of the observer simultaneously. They were tuned so that their tones had rates of vibration in the ratio of 1 and 3, this ratio being chosen because of all pairs of tones these give the largest changes in the form of the sound wave-curve as the relative phases of the tones are changed. Thus with one phase relationship the sound wave-curve has a single sharp, well-marked peak, whereas with another phase relationship two peaks are found, having a trough between them. If, then, the ear is affected at all by the form of the sound wave-curve, these two tones, combined in different relative phases, should show it. The experiment was performed by causing the higher-toned fork to sound continuously, the lower-toned one being turned on and off at irregular intervals, so that the relative phases should be chance ones. No difference whatever in the quality of the sound could, however, be detected by the observer. The response of the ear appeared to be quite independent of the relative phases of the tones, and, therefore, we must conclude that the ear effects a true harmonic analysis by means of resonators.

If the above experiment is repeated with two sources of tones that are not free from overtones it is found that the ear does readily detect differences in the quality of the sound as the relative phases of the tones are altered. For example, if Helmholtz's syren is used with, say, 18 holes operating in the lower wind chest, and 6 holes in the upper, then there are found to be 18 regularly spaced positions where the lower tone predominates, and 18 intermediate positions in which the upper tone predominates.

These effects are produced by the summation and interference between the upper tone and the second harmonic overtone of the lower tone. When holes of both wind chests are exposed simultaneously, summation occurs, and the upper tone predominates, whereas when the holes of one chest alternate with those of the other, interference occurs which weakens the upper tone, so that the lower tone predominates.

Many years ago there was considerable controversy as to whether the ear could, or could not, detect difference of phase. The above experiments suggest that pure tone free from harmonics may have been used by one school, and impure tones containing harmonics by the other, because in this way their difference in opinion could be readily explained.

C. R. G. COSENS.

H. HARTRIDGE.

King's College, Cambridge.

The Action of Sunlight.

IN NATURE of December 15 Sir Oliver Lodge is good enough to refer me to some experiments on the antiseptic action of sunlight which he carried out long ago in association with the late Prof. Marshall Ward. I have not yet been able to see the memoir to which Sir Oliver Lodge refers, but I believe that I am already well acquainted with it, and have been able to quote its essential findings on many occasions in connection with the demand for the abolition of the coal-smoke curse—thanks to an admirable account of Marshall Ward's methods and results, referred to the year 1892, in Sir James Crichton-Browne's "Light and Sanitation," an address delivered in Manchester in 1902 (Sherratt and Hughes, 27 St. Ann Street, Manchester). Particularly I value the last paragraph, in which Sir Oliver Lodge praises the anti-

septic and innocent quality of the sunlight just as we get it after filtration by the "unpolluted atmosphere."

But after seeing the clinical action of sunlight at Leysin under Dr. Rollier and at the Treloar Hospital under Sir Henry Gauvain, and reading the papers of Sonne (*Acta Medica Scandinavica*, vol. 54, fasc. 4, "The Mode of Action of the Universal Light Bath," from the Laboratory of the Finsen Medical Light Institute, Copenhagen) mentioned in my previous letter, I am absolutely certain, as anyone else would be, that there is more in the curative action of sunlight than its bactericidal effect. In a recent lecture before the Physiological Society of University College, entitled "The Physiology and Therapeutics of Sunlight: Facts and Questions," I cited instances and showed photographs of many cases where the value of sunlight could not have depended upon its antiseptic power. Sonne's view is that sunlight warms the blood without appreciably raising the general body-temperature, that this produces the valuable, without the injurious, effects of fever, and that this action is obtainable by the proper use of sunlight, and by that alone.

The practical importance of this fascinating physiological problem is apparent to me after recent visits to certain sanatoria, otherwise admirable, where I have been told that I should see the sunlight employed, and have found, for instance, that open air and diffused daylight, the latter reaching the face and possibly the hands, were regarded as the equivalent of Rollier's treatment; or that the children were scrupulously put under awnings or sent to school in an adjacent wood whenever the sun shone. The pitiful statistics of these places, compared with those of Rollier and Gauvain, point the moral.

Since first drafting this letter I have seen, thanks to Prof. Leonard Hill, new records of work done by Prof. A. F. Hess in New York, showing the cure of experimental rickets in animals fed and continuing to be fed on a diet which invariably produces rickets—when they were placed in sunlight for a few hours daily. No mere antiseptic action is here in question.

With rare exceptions, we do not yet know what heliotherapy consists of in this country; no one yet knows its action, nor even the pure physiology of sunlight. Meanwhile, we are carefully depriving many patients of their one chance of life, and quacks and others are using all manner of artificial lights in therapeutics as if they were equivalent to, or better than, the sunlight, which, according to Sonne's experiments—nicely consorting from another point of view with those of Sir Oliver Lodge—is incomparable.

The Smoke Abatement Committee has now published its admirable Final Report, and reiterates now my plea for an inquiry such as Carrel, I am told, is about to undertake at the Rockefeller Institute in New York, but which no one, not even Prof. Leonard Hill, our great student of the air and temperature relations of the body, is yet making here into the action of sunlight. I believe that the restoration of sunlight to our urban populations, mostly darkened in slums and smoke, is the next great task of hygiene in this country.

C. W. SALEEBY.

Royal Institution, January 2.

Units in Aeronautics.

A LARGE number of equations, in aerodynamics appear in the form $R=k\rho SV^2$, where R , ρ , S , V are the reaction, density of the atmosphere, surface, and relative wind velocity.

Since both R and ρSV^2 have the dimensions of force, k is clearly a numerical coefficient unchanged

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by transformation from one self-consistent system of dynamical units to another, e.g. from foot, pound, second, poundal to centimetre, gram, second, dyne.

In transforming to an inconsistent system, k will in general be altered, but the inconsistencies may cancel each other in particular cases so far as to leave k unaltered. For example, if a gravitational unit of force is introduced g times the consistent unit, then we may write $R=k\rho SV^2/g$ pounds weight in the British system, or grams weight in the c.g.s. system. And while the numerical values of both R and g vary inversely as each other, the value of k is not affected if the system is otherwise consistent.

Prof. Bairstow ("Applied Aerodynamics," p. 119) maintains that the gravitational unit of force is the natural one, and to get rid of the local value of g that mars the consistency of his dynamical equations he introduces a new unit of mass, the "slug" of (local) g pounds mass, the use of which he restricts to the measurement of atmospheric density. Then putting $\rho'=\rho/g$ he can write $R=k\rho' SV^2$ in the same form externally as before, and read off the value of R in pounds weight instead of in poundals, all without showing g explicitly.

In this way, in his opinion, "it appears that the divergence of language (*sic*) between science and engineering would disappear."

But if we apply this method of measuring the density of the atmosphere to the estimation of the lift of an aerostat we get, putting m =density of hydrogen/density of air, $(1-m)\rho'\times\text{volume}=R$ slugs weight ($=gR$ pounds weight= g^2R poundals!).

Altogether it seems more satisfactory to teach engineers the physical meaning of Newton's laws of motion than to invent units which evade them, just as it has proved better to teach them the elementary notation of the infinitesimal calculus than to devise "calculus-dodging" demonstrations. A. R. LOW.

London, December 8.

Self-fertilisation in Mollusca.

As the question of self-fertilisation in the more highly organised invertebrates is of considerable importance from the genetic point of view, I would like to direct the attention of readers of NATURE to a publication (*Acta Soc. pro Fauna et Flora Fennica*, vol. 40, No. 2, 1915), a copy of which I have just received from the author, Dr. A. Luther. It constitutes an important addition to the evidence for the occurrence of self-fertilisation in mollusca, as Dr. Luther states that he succeeded in rearing two generations by self-fertilisation in *Agriolimax agrestis*. I have recently pointed out (*Proc. Malac. Soc.*, vol. 14, 1921) the value of Künkel's work on *Arion* in this respect; but at the time Dr. Luther's results were not known to me, as the publication was not available and had not figured in the "Zoological Record." Should these important observations be finally confirmed and the technique improved so as to produce more than two generations, a very valuable contribution to genetic study will be achieved. Dr. Luther's work, however, emphasises the necessity for conducting a study of environmental conditions in order to secure improved viability.

I may point out, perhaps, that though quite a number of cases of self-fertilisation have been recorded in Pulmonata by Lang, Holzfuss, and others, the subject is by no means fully explored either with regard to the distribution of the phenomenon among Pulmonata or the circumstances in which it occurs.

G. C. ROBSON.

British Museum (Natural History),
Cromwell Road, London, S.W.7, December 23.