

has finished posing the question? On the contrary, I regard the names *F. oceanea* and *F. fasciata* as equal in their pretensions, until the choice is made. Once made, the person that attempts to upset it is the true begetter of confusion.

But does Mr. Cockerell's conclusion follow from his premises? The conception underlying his application of the law of priority is that place is to be reckoned as time. Now a specific name has no standing until a description of the species denoted thereby has been published, and until the name in question has been associated therewith. Till then it is a *nomen nudum*. The name *Filistrata oceanea* is, we are told, a *nomen nudum*. Even had it been published in a previous paper, it would, in the absence of a description, have remained a *nomen nudum*. It appears first on page 50 of Mr. Banks' paper, but without description; and it remains without description for five whole pages. During all this space, it remains a *nomen nudum*. Mr. Banks may asseverate as often as he pleases that *F. oceanea* is identical with *F. fasciata*. But *F. fasciata* does not exist (for Mr. Cockerell), except as a *nomen nudum*, till page 55 is reached. Here is a description at last; but the name associated with that description is not *F. oceanea* but *F. fasciata*. It is this latter then that ceases first to be a *nomen nudum*.

The case of *Cucumites lesquereuxii* Knowlton is different; but even this may, on Mr. Cockerell's principles, be defended. For it follows from the axiom 'place = time' that every name is a *nomen nudum* until the diagnosis or description is complete. But the description of the fruit under discussion once finished, Mr. Knowlton calls it, not *Cucumites globulosus*, but *C. lesquereuxii*.

Mr. Cockerell may retort that this is mere hair-splitting and childish chop-logic. It is. But it is the natural outcome of an attempt to subject mere modes of expression to a rule obviously intended to apply to essential matters and not to the niceties of style.

To save all misunderstanding, let me repeat emphatically that I am not defending either Mr. Banks or Mr. Knowlton. I have no

sympathy with people who print names for the mere sake of rejecting them, or who tell us what they might have done or what somebody else might do if circumstances had been different, and so forth. If such action be in any degree checked by Mr. Cockerell's arguments, their publication will have had one good result.

F. A. BATHER.

MOTION OF TRANSLATION OF A GAS IN A VACUUM.
(REPLY TO MR. R. W. WOOD.)

IN the hope that if I bring around Mr. R. W. Wood to my view of the energy required to set a gas in motion of translation in a vacuum, he will not find my explanation of the energy changes which take place when a gas expands into a vacuum unnecessary, I will only take up here that view.

Mr. Wood in his second note (SCIENCE for December 5) on a communication of mine to the American Association says:

We sometimes find the statement in text-books that a gas expanding under such conditions that no work is done experiences no cooling, for example, when expanding into an infinite vacuum. It appears questionable, however, whether a gas can expand without doing work. Leaving out of consideration the internal work, *i. e.*, the overcoming of the forces of cohesion, we still have the gas in the receiver doing work in giving a motion of translation to the mass of gas thrown out into the vacuum.

I think, however, that it can be proved that no work is necessary to set a gas in motion of translation in a vacuum by the following reasoning. Suppose that in a body of gas all the molecules move with the same velocity instead of having, as we assume according to the kinetic theory, velocities varying greatly in magnitude, and that the identical velocity of all the molecules plays in other respects the same part which we attribute to the mean molecular velocity, *e. g.*, that to each degree of temperature of a gas a fixed velocity corresponds, etc. Let that gas be compressed in a receiver and then allowed to enter a vacuum vessel which communicates with the latter. What will happen? To my mind, it can hardly be conceived that anything else could take place than the uniform distribution of the

gas in both vessels, the same temperature obtaining throughout its entire mass. For how could a difference of temperature result when no other action between the molecules is possible than their collisions with one another, collisions which cannot affect the molecule's kinetic energy (the kinetic energy of each molecule being the same according to our supposition). But if it is admitted that in the supposed case the two vessels will be filled uniformly with the gas at the same temperature throughout, it is also admitted that a portion of the gas was set in motion of translation without any work having been done.

The only objection that could be raised to the above reasoning is perhaps this: the gas, while compressed in the receiver, has motion of agitation and, after equilibrium is established upon a portion of the gas having entered the vacuous vessel, it has again the same motion of agitation, but while passing from the receiver into and through the vacuous vessel a portion of the gas had, in addition, motion of translation which must be superimposed on the motion of agitation. There thus seems to be here a plus of energy to be accounted for. But this objection can be met by considering more closely the three stages in time which the phenomenon of the expansion of a gas into a vacuum presents. First, in the compression chamber all the gas has only motion of agitation, then while traversing the vacuous vessel the respective portion of the gas has only or mainly motion of translation at the expense of its original motion of agitation; and lastly, on striking the walls of the empty vessel the incoming gas has its motion of translation reconverted into motion of agitation.

If the above reasoning is correct, it means that just as to set one gas molecule in motion of translation in a vacuum does not require anything else than its own motion of agitation (which will, I believe, be admitted by every one), so with a body of gas.

But if in the hypothetical case no change in the magnitude of the kinetic energy of the individual molecules is required to 'translate' (if I may use the expression) a portion of the molecules, why should it be necessary

in the actual case as understood on the basis of the kinetic theory? It is true that we observe here a redistribution of energy and a 'translation' of a portion of the gas, but this 'translation' would have taken place if there were no redistribution of energy.

PETER FIREMAN.

WASHINGTON, D. C.

WILL-MAKING.

TO THE EDITOR OF SCIENCE: The ever-recurring contests of wills, the disputes as to their validity, their meaning in general and particular, the interpretation of their peculiarities and seeming inconsistencies, etc., are such a damage to private comfort and to the public welfare in the highest sense, that any means of lessening the growing evil must be welcomed by all concerned.

As part remedy at least, I would suggest the establishment by each state of a court or other properly constituted body, whose duty and business it should be, upon application, to consider and validate *during the lifetime of the testator* his will, which, after approval could be deposited with the necessary secrecy, as a thoroughly competent legal instrument. To change a will, the same process should be gone through again. This presentation, validation and placing on record should absolutely bar all actions designed to break or alter the will after the death or subsequent incapacity of the testator. The way in which the Torrens land-title has been instituted in some countries is, if not a precedent, an instance of the successful treatment of a kindred difficulty. An unbreakable will might turn out to be as great a boon as an indefeasible title.

ALEXANDER F. CHAMBERLAIN.

CLARK UNIVERSITY, WORCESTER, MASS.,

[It is said of Charles Darwin in the 'Life and Letters': 'He would declare energetically that if he were law-giver no will should be valid that was not published in the testator's lifetime.' It is not clear how a secret will could be validated in the manner suggested by Professor Chamberlain, but there appears to be no reason why it should not be possible to probate a will during the lifetime of the testator. Such legal and moral scandals as