Prof. Hoffmann, of Berlin, whether that doctrine any longer finds support among scientific men in Germany. *His reply was a most emphatic negative;* the doctrine, he said, being one which no man of science with whom he is acquainted would think worthy of the slightest attention. Yet in Mr. Wallace's judgment (*query* in Mr. Crookes's also?) the unanimous verdict of the scientific world of Germany, to say nothing of England, is a prejudiced one; only Mr. W. and his spiritualistic allies appreciating correctly the real force of the evidence originally advanced by Reichenbach, and confirmed by those trustworthy (?) authorities, Drs. Ashburner and Gregory.

In thus setting his own judgment on a question which lies altogether outside the scientific domain which he has made his own, against the unanimous verdict of the eminent physicists and physiologists who have carefully "tried" the Od-force and "found it wanting," and in rebuking myself and those who think with me for our incredulity, does not Mr. Wallace put himself somewhat in the attitude of his old opponent, John Hampden, who thinks everybody either a fool or a knave who maintains the earth to be round? WILLIAM B. CARPENTER

October 22

Potential Energy

WITH reference to the views of "John O'Toole" on the subject of energy perhaps you will allow me to say how one of the class to which "poor Publius" belongs has conceived the matter of terminology with satisfaction to himself.

1. Energy being unanimously defined by "the doctors" to be "capacity for doing work," and also energy conveying in its derivation the notion of activity, this term is properly applicable only to the bodies of material systems the motions of which are contemplated. Hence all energy is in its nature kinetic—the very term kinetic is logically included in the term energy.

2. When a material system is in motion it actually possesses, *ipso facto*, a capability of doing work, that is to say, it has actual energy.

3. When in any configuration of the system we contemplate as possible the action of causes which will alter its motions and give it a second configuration, the excess of the energy which it would possess in this second configuration over the energy which it possesses in the first is properly called its potential energy in the first configuration.

4. The assertion that in any configuration the sum of the energies, actual and potential, of a material system is constant, is what Kant would call an analytical proposition, or what "X." (quoting Herschel) calls "only a truism after all." But I further remark—

5. That this truism is not the principle of the conservation of energy, but that this principle is a true "synthetical proposition" which some fairly regard as an almost immediate deduction from Newton's third law, and which others regard as proved by often repeated and much varied experiment; and hence that "X.'s" statement of this great principle in the form—"The sum of the actual and potential energies *of the universe* is a constant quantity," (the italics are mine) is not its proper definition.

6. That, leaving the consideration of bodies, and referring to forces, the term to be employed instead of energy is work, and that the term analogous to the "potential energy of bodies" is the "potential work of forces," this latter being the amount of work which they are capable of doing in displacing their points of application from their actual configuration to any fixed chosen one.

7. That by the expenditure of a fixed amount of work on any material system the same amount of actual energy (whose type is $\frac{1}{2} m v^2$) is under all circumstances produced, and that, through whatever forms this actual energy is made to pass, if the whole of it is always utilised, it will finally be reconvertible into the same original amount of work, this being the principle of the conservation of energy.

8. That instead of the statement in 5, we must substitute the synthetical proposition that "the sum of the actual energy of the bodies in the universe and the *potential work of its internal forces* is a constant quantity," and the same is true of every material system which is regarded as complete in itself; or in other words, wherever and however a given quantity of potential work is lost by the forces of the system, this always appears in the shape of a fixed quantity of actual energy, in the form which we call heat, or in some other.

Hence we have energy, actual and potential, of bodies; and work, actual and potential, of forces.

A few remarks in conclusion. "J. M." has very happily illustrated the propriety of the expression potential energy, as, in strict consequence of the definition of energy, a potential capacity of doing work; and if in his illustration the "power of purchasing" is considered with reference to a further object, there may be not merely a "double remotion from" what we may regard as "tangibility," but a remotion of a higher multiple order. "W. G." has well explained that it is only in consequence of the fixedness of the earth that the potential energy of the system of the earth and stone is by the "doctors" located in the stone. Finally, I can hardly conceive how "X.," who has devoted so much attention to the literature of this subject, can have fallen into such a grievous error with regard to the clock.

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Cooper's Hill

YOUR "Potential Energy" corespondents will find three letters on the "Conservation of Energy" in the *Engineer* for January 12 and 19 and February 2 which may interest them. The writer "Φ Π" assumes that all the phenomena of force are explained by the theory that only matter and motion exist, and that what we call potential "energy" is only "quantity and motion," which motion is indestructible but diffusible. Z.

London, October 20

Origin of Contagious Diseases

I HAVE been much struck by the following passage in Dr. Richardson's address, NATURE (vol. xvi. p. 481):—

"(*c*) That as regards the organic poisons themselves and their physical properties, the great type of them all is represented by the poison of any venomous snake. . . . It is the type of all the poisons which produce disease."

Now has it been really proved, by experiment, that the poison of snakes produces the effects characterising the contagia? viz.,

"(*a*) . . . Each particle of any of these poisons brought into contact either with the blood of the living animal or with certain secretions of the living animal, possesses the property of turning the albuminous part of that same blood or that same secretion into substance like itself. . . ."

In other words, if an animal is suffering from snake poison does its blood or any of its secretions acquire the power of transmitting the disease, *i.e.*, the effects of a snake's bite, to another individual, as is the case with an animal affected with carbuncle, glanders, hydrophobia, &c., &c.?

Unless this question has been decided in the affirmative it would appear rather difficult to uphold the sentence (*c*) as quoted above. D. W.

Freiburg in Brisgau, G. J., October 14

[Dr. Richardson informs us that D. W. does not properly understand his argument. Dr. Richardson does not suppose that the person or animal poisoned from a poisonous snake is, in turn, poisonous, although that may be the fact. He merely uses the illustration that as a poisonous snake secretes a poison so an infectious person is for the time secreting a poison.]

I SEE by your issue of October 4, that Dr. Richardson has honoured me by mentioning my name and placing me as the first, in modern times, to advocate the hypothesis that living germs are the exciting agents of epidemic and infectious diseases. But he says further, "I protest, I say, that this hypothesis is the wildest, the most innocent, the most distant from the phenomena it attempts to explain, that ever entered the mind of man to conceive." It may be so, but I look in vain through the whole story he narrates in his lecture to find a rational substitute for it, and it appears to me desirable at the present juncture that the principles of the germ theory, as I have interpreted them, should stand side by side with Dr. Richardson's "glandular theory." It is now nearly thirty years since I endeavoured to find some common root or cause for those diseases which we find in plants, animals, and man, and which are communicable among the individuals of each order in nature; also, in some instances, from one order to another. During that thirty years every step in scientific research and medical experience as far as my inquiries have carrried me, has tended to confirm the views I put forward in my original "Essay" and in subsequent papers read before the Epidemiological Society. Notably the latest advocates of a germ theory are two of our most eminent men, the one a leader in science, the other a leading physician. I need hardly say I allude to Prof. Tyndall and Sir Thomas Watson; surely these