

fossiliferous in the lower part of the St. Lawrence river; further inland it has not been observed to contain fossils. From the author's description of the boulder-clay as seen at low levels in Canada, we think that deposit more closely resembles some of the maritime fossiliferous stony clays of Britain than our Till or lower boulder-clay. Dr. Dawson seems to have satisfied himself that the "real cause" of the excavation of the American lakes "was obviously the flowing of cold currents over the American land during its submergence." He also thinks that "the fiords on coasts, like the deep lateral valleys of mountains, are evidences of the action of waves, rather than that of ice." No glacialist, as far as we know, holds the extravagant belief that fiords have been cut out by ice. They are undoubtedly submerged valleys, and were hollowed out by streams and other atmospheric influences in ages long anterior to the glacial epoch. But however much we may differ from Dr. Dawson in some of his conclusions, there can be no doubt that he has added very considerably to our knowledge of American glacial deposits, and we cordially recommend the perusal of his "Notes" to our geological readers.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Invention of the Water-Air-Pump

STATEMENT BY PROF. BUNSEN *

"A letter addressed to me by Dr. Sprengel, under date of November 1, 1872, in which he says: 'Perhaps it will not have escaped your observation, that the invention of the water-air-pump, which you have constructed after the principle of my mercury air-pump, according to your paper published in 1868 on the washing of precipitates, is almost everywhere attributed to you,' induces me to make the following statement:—

"The interesting discovery, that by means of columns of liquids flowing downwards a more perfect vacuum can be produced, than was possible by the air-pumps hitherto in use, belongs solely and only to Dr. Sprengel. He in his researches on the vacuum (*Journal of the Chemical Society*, January 1865) brings prominently forward, that water is from a practical point of view the only liquid which could come into consideration as a substitute for mercury used in the instrument described by him; and that it is not unlikely that such an instrument, adapted for water, might possess advantages which air-pumps of other constructions have not, particularly in hilly countries, where the large volume of a natural waterfall might be rendered available. In the theoretical considerations on the action of his instrument, which immediately follows the above, it is noticed, that it is simply the reverse of the Trompe, with this addition, that the supply of air is limited, while that in the Trompe is unlimited.

"If in the face of these facts, which are open to all, anyone attributes to me, as I must conclude from Dr. Sprengel's letter, a share in his discovery, I can regret this only all the more keenly, as in my treatise on the new method of filtration I could not possibly have expressed myself with regard to Dr. Sprengel's claims more loyally and precisely than I have done. There, I have stated expressly, that I have constructed the pump used for filtrations and described by me in detail, after the principle of Sprengel's mercury-air-pump. It was the only apparatus of the kind which Dr. Sprengel described, consequently the one to which alone I could refer.

"R. BUNSEN

"Heidelberg, Nov. 5, 1872"

Expressing my best thanks to Prof. Bunsen for the above statement, I beg to add, that since 1860 I have been using for laboratory purposes a water-trompe, as described by me in *Poggendorff's Annalen* for 1861, vol. cxii., which (by reversing the action) led me in 1863 to the new method of air-rarefaction. Water was the first liquid, which I used in my first pump, constructed during the summer of 1863. But the fallacies arising from the tension of aqueous vapour and from the air absorbed in

water, as well as the inconvenience of having to provide for the requisite fall, caused me to discontinue the use of water, and to substitute in its stead mercury as the most suitable liquid for establishing the truth, which I had recognised by means of a water-air-pump with an insufficient fall. My paper of 1865 was written with reference to *all* liquids; in fact, on p. 15 (rendered prominent by italics) I summed up thus:—

"The main fact which I have established in this paper may be shortly stated to be that, if a liquid be allowed to run down a tube, to the upper part of which a receiver is attached by means of a lateral tube, and if the height at which the receiver is attached be not less than that of the column of the liquid which can be supported by the atmospheric pressure, a vacuum will be formed in the receiver minus the tension of the liquid employed."

I regret that the obviousness of the matter led me to refrain from expressing myself in a more detailed manner, believing, as I still believe, that what I wrote sufficiently described the construction of the water-air-pump.

In conclusion Mr. Johnson's aspirator* for establishing a current of air ought to be mentioned here. It was recognised by Prof. Hofmann † to act on the principle of the trompe, and of course might have served as an air-pump, had it been noticed at the time that the instrument would furnish the means of creating a vacuum. And I may also draw attention to the tube ‡ of a vacuum-pan, through which the water is made to escape, which has served to condense the steam of the boiling liquid. This no doubt would in like manner have served as a complete water-air-pump, but it does not appear that its use as such was discovered.

H. SPRENGEL

London, Jan. 22, 1873

Kant on the Retarded Rotation of Planets and Satellites

It is now recognised that the tides are necessarily lengthening the day; but the history of this recognition seems to be incomplete. "It appears," says Mr. Tait in his "Thermodynamics," p. 86, "that the first suggestion of such an effect is due to Kant." Mr. Stewart speaks more positively ("On Heat," p. 356), but adds that Mayer "was the first to give his conclusions general publicity."

The following are the facts with respect to Kant, as they are to be found in Rosenkranz and Schubert's edition of his works, part vi. pp. vii. 3-12. The Berlin Academy of Sciences had proposed, as the subject of a prize essay for 1754, the questions whether the length of day and night had changed, and if so, what the cause could be and how this was to be ascertained. Kant did not compete; apparently he was dissatisfied, as well he might be, with his attempt to estimate the possible amount of retardation; but he published his views in a Königsberg weekly paper.

It is of some interest to compare Kant's position with our own. In the first place, he expresses himself with almost entire generality. He does not speak merely of the tides, but says that the rotation of any planet is necessarily retarded if it contains a considerable amount of fluid. Kant knew as well as we do that the *considerableness* (that is the magnitude) of the cause affected only the magnitude (and not the bare reality) of the effect; so there is nothing to be added to his statement of the condition of retardation but what our own writers do not seem to think worth adding, namely the energy dissipated in consequence of the imperfect rigidity and elasticity of the solid parts of the planet.

Again, with respect to the final result, Kant makes two statements, which, if literally contradictory, yet taken together go to show the fulness of his knowledge. First he says the rotation must ultimately cease; further on that it must diminish till it is equal to the revolution of the moon, so that the earth will constantly face the moon, as the moon now constantly faces the earth. The essay bears marks of hasty writing; and it seems clear that the latter statement is only intended for that part of the effect which is due to the moon. The former may be intended to affirm the ultimate abolition of the solar day; if it means much more (as it ought) I presume it is inconsistent with Kant's express rejection of the hypothesis of an interplanetary resisting medium.

On the other hand, Kant betrays no suspicion of the reaction upon the disturbing bodies, and the consequent lengthening of the month and year. And in speculating on the possibility of

* *Quarterly Journal of the Chemical Society*, vol. iv. p. 186. 1852.

† *Ibid.*

‡ "Elements of Physics," by Neil Arnott, M.D. (Longmans.) 3rd edit., London, 1823.

* Translated from *Ann. Chem. Pharm.* vol. cxlv. p. 159, by H. Sprengel, authorised by Prof. Bunsen.