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To cite this article: Shelford Bidwell M.A. F.R.S. (1886) XXIX. On the magnetic torsion of iron and nickel wires , Philosophical Magazine Series 5, 22:136, 251-255, DOI: [10.1080/14786448608627927](https://doi.org/10.1080/14786448608627927)

To link to this article: <http://dx.doi.org/10.1080/14786448608627927>



Published online: 29 Apr 2009.



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Earth has become almost altogether solid, the moment of inertia of the shell with respect to its polar axis must be always greater than the moment of inertia for its equatorial axis.

The tendency of the fluid nucleus to increase in ellipticity might produce a result worthy of examination by volcanologists, namely, a possible increase in the development of volcanic phenomena in equatorial as compared to polar regions with the progressive solidification of the Earth up to a certain point. Until the thickness of the shell has become very great, recent periods should exhibit a greater development of volcanic energy towards the equator than towards the poles, as compared to remote epochs.

XXIX. *On the Magnetic Torsion of Iron and Nickel Wires.*

By SHELFORD BIDWELL, *M.A., F.R.S.**

IN a paper published in the *Phil. Mag.* for July 1886, p. 50, Prof. G. Wiedemann refers to his well-known experimental researches into the relations between the torsion and magnetization of iron wires. Let a longitudinally magnetized wire NS, be fixed at the south end S, the other end N being free, and let a battery current be passed through it from S to N, then the free end will be observed to twist in the direction of the hands of a clock as seen from the fixed end.

Maxwell † explains the phenomenon thus. The wire being magnetized both circularly and longitudinally, the resultant magnetization is in the direction of a right-handed screw round the wire. Now Joule found that an iron bar when longitudinally magnetized was increased in length, its transverse dimensions being at the same time contracted. We should expect therefore that a spirally magnetized wire would expand in the direction of the magnetization and contract in directions at right angles to the magnetization. And thus the twisting would be produced.

This explanation assumes that the torsional effect is simply secondary to and dependent upon the phenomena observed by Joule, and Prof. Wiedemann, for reasons given in his paper, appears not to be satisfied with it.

Some additional light may perhaps be thrown on the subject by a few experiments which I made at the beginning of the present year. They were intended to be merely of a

* Communicated by the Author.

† 'Electricity,' ii. § 448.

preliminary nature, and I had hoped to be able to carry them further ; but so far as they go they appear to be conclusive, and a short account of them may be of interest at the present time in connection with the recent publication of Prof. Wiedmann.

In two papers communicated to the Royal Society *, I have pointed out that the effect of magnetization upon the dimensions of an iron rod is not so simple as it had previously been believed to be. Joule enunciated the law that the elongation in a given bar is at first proportional to the magnetic intensity, and that it ceases to increase after the iron is fully "saturated." My own experiments show that if the magnetization is carried beyond the point at which the elongation reaches a maximum, the length of the rod, instead of remaining unchanged, steadily diminishes, until, when the magnetizing current has attained a certain strength, the original length of the rod is unaffected, and if this strength be exceeded actual *retraction* is produced. From some further experiments, details of which are not yet published, it appears that effects of the same character occur when rings are used instead of straight rods. The diameter of an iron ring surrounded by a coil of wire was found to be increased when a comparatively small current was passed through the coil and diminished when the current was strong.

I have also carried further Joule's experiments regarding the effects of magnetization upon the length of iron wires under tension and ascertained, among other things, that a wire when stretched by a weight attains its maximum elongation with a smaller external magnetizing force than when it is free, retraction apparently beginning at an earlier stage of magnetization.

Lastly, I have repeated and confirmed the experiments of Barrett † upon nickel. Barrett discovered that the length of a nickel bar when longitudinally magnetized was diminished. So far as appears from his published papers on the subject, which are very brief, he worked only with comparatively strong magnetizing currents, and it seemed to me possible that, as in the case of iron, weaker currents might cause elongation. But on trying the experiment I found that this was not so. Magnetizing forces which were so small as to produce no sensible effect whatever upon iron caused considerable diminution in the length of the nickel bar, and the curve of retraction given in my paper (p. 131) clearly passes through the intersection of the axes, and shows quite con-

* Proc. Roy. Soc. xl. pp. 109, 257.

† 'Nature,' xxvi. 585, and Brit. Assoc. Rep. 1882.

clusively that elongation cannot occur at any early stage of the magnetization.

Here then we have three classes of phenomena, which if Maxwell's explanation were correct would enable us to predict certain variations in the torsional effects observed by Wiedemann.

Let us consider first the case of nickel. According to Maxwell, a spirally magnetized iron wire is twisted in a certain manner, because the iron expands in the direction of the magnetization. Since, then, nickel contracts in the direction of the magnetization, we should expect a nickel wire to twist oppositely to an iron wire under similar conditions. This I found to be so. Whether the magnetizing currents were strong or weak, the twist of the nickel wire was always in the direction opposite to that given by Wiedemann for iron.

I was not aware until I read Wiedemann's recent paper that this experiment had been performed previously*. Wiedemann discusses it fully and admits that the facts accord with Maxwell's explanation (p. 54). But he appears to consider that it is a case of merely accidental coincidence: it is true that when longitudinally magnetized, iron expands while nickel contracts, and it is true that, when spirally magnetized, iron and nickel twist oppositely; but the two sets of phenomena are quite independent and are not related to one another as cause and effect. The torsion is to be explained, he thinks, by supposing that "the obliquely spiral direction which the molecules take up in consequence of the two magnetizations at right angles to each other" is, owing to intermolecular friction, "accompanied by a displacement of the longitudinal fibres and [of the] sections of the wires." In iron the friction of the longitudinal fibres is the greatest, and thus the torsion in the observed direction is accounted for. In nickel the friction of the sections predominates; a nickel wire is therefore twisted oppositely to an iron wire. For a more complete statement of Wiedemann's views reference must be made to the paper †.

* Knott, Proc. Roy. Soc. Edin. 1882-3. Quoted by Wiedemann.

† I confess that I do not find it easy to follow the suggested explanation. Passing over preliminary difficulties, and assuming that the friction between the polar ends of adjoining molecules is different from that between their sides, it seems to me that the observed torsions could only be accounted for by assuming that in iron the friction of the ends is greater than the lateral friction, while in nickel the lateral friction is greatest. This appears to be directly opposed to Wiedemann's statement; but though I have considered the question carefully and from several points of view, I can arrive at no other conclusion. It is, however, possible that I may have altogether misunderstood the argument, the more so as the paper referred to is a translation.

The behaviour of nickel then, though in agreement with Maxwell's hypothesis, is not accepted by Wiedemann as affording confirmation of it.

But we have further means of testing the hypothesis. It has been said that an iron rod when very strongly magnetized is contracted instead of being elongated. Moreover, when the rod is stretched by a weight contraction occurs with smaller magnetizing forces than when it is unstretched. In accordance with Maxwell's explanation, therefore, an iron wire spirally magnetized by very strong currents should twist (just as if it were a nickel wire) in the opposite direction to that indicated by Wiedemann for iron; and this reversal of the twist should take place at an earlier stage of the magnetization if the wire were stretched. Now this is exactly what I have found to be the case. The free end of an iron wire, through which a constant current is passing, can be made to twist in either direction by varying the current through the surrounding helix, while with a certain medium strength of current there is no movement at all. And again, when the wire is loaded with a weight, the current which produces no torsion is considerably diminished, the reversal of the torsion also, of course, occurring with smaller currents.

It is not easy to see how Wiedemann's theory could fairly be made to explain these phenomena. He would be compelled to assume that when the iron is more intensely magnetized, that is, when the molecules are more completely turned in the same direction, the excess of longitudinal over transverse molecular friction, instead of becoming more marked, as might naturally be expected, would be decreased, and, with a sufficient degree of magnetization, even converted into a deficiency. It would also be necessary to assume that when the molecules of a wire are drawn apart by stretching the friction between their ends is nevertheless greater. Such assumptions would, I think, be highly unscientific.

On the other hand, Maxwell's explanation, which does not seek to go behind the magnetic elongations and retractions detected by Joule and others, fits the newly observed facts easily and naturally.

Note on the Experiments.

It seems desirable to give a few details of the experiments above referred to, though too much importance must not be attached to the quantitative results:—

The iron wire used was .7 mm. in diameter and 20 cm. long. It was suspended in an upright position from a fixed clamp, and passed through a helix consisting of 876 turns of copper wire in

12 layers. To the lower end of the suspended wire was fixed a small plane mirror which reflected an indicating beam of light upon an ordinary galvanometer-scale 127 cm. distant from it. Each scale-division was equal to .64 mm. A steady current of about 1 ampere was passed through the iron wire from the clamp to the free end, which dipped into a mercury-cup. The magnetizing coil was in circuit with a battery of 7 Grove cells, a box of resistance coils, a tangent galvanometer, and a contact key.

The deflections of the spot of light when various currents were passed through the coil are given below :—

Iron wire unstretched.

Current.	Deflection.
0.36 amperes.	+7 divisions.
1.2 "	0 "
3.7 "	-4 "

Same wire loaded with 6.3 kilos.

Current.	Deflection.
0.33 amperes.	+2 divisions.
0.59 "	0 "
3.68 "	-3 "

The current which produced no deflection was obtained tentatively by varying the resistance ; a current slightly stronger or weaker than that indicated caused a perceptible deflection in one direction or the other. It will be seen that when the wire was loaded with a weight of 6.3 kilogrammes the current causing no deflection was reduced to one half of its former value. The deflections given above are those produced by the second and subsequent currents of the strength denoted. That due to the first current was uncertain, depending probably upon the previously existing permanent magnetism of the wire. This point, amongst others, requires further investigation.

XXX. *Tests of Herschel's Æthereal Physics*.

By PLINY EARLE CHASE, LL.D.*

JEVONS says ('Principles of Science,' ii. p. 145):—"Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point of space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the Earth's surface, so that the pressure of the æther upon a square inch of surface must be about 17,000,000,000,000, or seventeen billions of pounds ; yet we live and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis ; yet

* Communicated by the Author.