LETTERS TO THE EDITOR.

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The Magnetic Compass and Nickel Cases.

IT is a common practice amongst instrument makers to put the magnetic compass needle of the pocket compass, or the marching compass used in the army, into an external case of nickel; the case is usually furnished with a lid, after the manner of a hunting watch, and is largely used by travellers.

Recently, while in South Australia, I used such a compass, which was lent to me, for the purpose of steering across a rather large "run." On comparing my position with that indicated by the map, I found that I had drifted considerably to the right of the point laid down; on examining the compass, while slowly shutting the lid, I noticed that as it closed the card of the compass was deflected. I next noticed that the case, which was much discoloured, and at first sight looked like bronze, was made of nickel; this revealed the source of error—when the lid was open, the nickel case, which is magnetic, was unsymmetrical with respect to the magnetic needle, and the needle was attracted by the lid from the true magnetic meridian, and the compass thereby rendered useless for steering purposes, and a source of possible danger, when long distances are traversed.

On talking over the matter with a leading instrument maker, I found that nickel is usually supposed to be a non-magnetic metal, and that nearly every traveller's compass case is made of nickel. It is somewhat remarkable, in an age of so many technical schools, that such ignorance can exist about a metal the properties of which were spoken of by Faraday in 1845 thus: "The magnetic characters of iron, nickel and cobalt are well known" (*Phil. Trans.*, 1846, p. 41). On looking through different price lists of leading firms selling marine compasses, I find that nickel enters into their construction also. In marine compasses the presence of a magnetic metal in the cases must be a source of some danger to the navigator. I would suggest that when selecting a magnetic compass, the magnet needle should be removed, and the case carefully tested for magnetic properties, and that should the case show any signs of magnetism it should be rejected. F. J. JERVIS-SMITH.

Oxford, June 13.

Historical Note on Recalescence.

IN his recent presidential address to the Iron and Steel Institute, Prof. Sir W. Roberts-Austen stated (NATURE, No. 1541, p. 43): "To Gore, and to Barrett, we owe the investigation of the nature of a fact which had long been well known to smiths, that iron on cooling from a bright red heat suddenly emits a glow."

I do not know what authority Sir W. Roberts-Austen has for this statement, but as this is not the first time it has been made, perhaps I may be allowed to ask if he has any documentary evidence in support of it. So far as I am aware the history of the matter is as follows:—At the meeting of the British Association in Bradford in September 1873, I read a paper entitled "On certain remarkable molecular changes occurring in iron wire at a low red heat"; this was subsequently published in the *Philosophical Magazine* for December 1873. In this paper, the phenomenon, for which I suggested the name *recalescence*, was first described, and was further investigated in other papers of mine, to which I need not here refer. At the time of the discovery it seemed to me highly probable that this remarkable after-glow in cooling iron and steel was likely to have been already noticed, but after considerable search I could find no previous record of it in scientific literature; nor could I, after persistent inquiry, discover a single smith or iron-master who had even casually noticed the effect until I pointed it out to them.

But the most curious thing was that the observation had entirely escaped Dr. Gore's attention; in 1869, Gore discovered that a momentary elongation occurred in a cooling iron wire after it had been heated to bright redness. Dr. Gore, however, did not pursue the matter further, and informed me in May 1872 that he had no intention of doing so (see *Phil. Mag.*, December 1873, p. 473). Writing to me, after the publication of my paper in 1873, in a letter which I happen to have kept, Mr. Gore says:—

"Edgbaston, Birmingham, December 2, 1873. Your new discoveries respecting the molecular changes in iron, described in the *Phil. Mag.* for this month, have greatly pleased me; especially the sudden development of heat attending the elongation during cooling, and the sudden shortening during heating. . . ."[1] And in a letter to me some years later—after the delivery by Prof. Roberts-Austen of a lecture before the British Association in 1889—Dr. Gore naturally expressed his surprise that the discovery or investigation of recalescence should be attributed to him.

It is to M. Osmond more than any one else we owe the series of masterly investigations that has raised the discovery of recalescence into the importance which it now holds in the metallurgy of steel. The value of M. Osmond's work is well known, but I am glad of this opportunity of emphasising it, for M. Osmond's modesty has led him to attribute in his papers, and in his correspondence with me, more value to my own work than it probably deserves. For instance, in a letter addressed to me from Paris on December 13, 1889, M. Osmond writes:—"Vos observations sur la Recalescence sont de celles qui feront époque dans l'histoire de la métallurgie; elles ont été le point de départ de tout ce qu'on a fait pendant ces dernières années, et pour mon compte, j'ai tant travaillé sur vos traces qu'il me semble vous connaître depuis longtemps. . . ."

I feel sure my friend Sir W. Roberts-Austen will forgive my venturing to correct him in this trifling matter of scientific history. It is quite possible his statement, that recalescence "had long been well known to smiths" prior to 1873, may be derived from his wide metallurgical knowledge, which I do not presume to possess, or it may arise from the common and unintentional blunder of reading our present knowledge into the past. W. F. BARRETT.

Royal College of Science, Dublin, June 2.

IN the Presidential address referred to, I attempted to review the progress made in connection with iron and steel during the past century. I felt that, notwithstanding the very limited space at my disposal for recording the work of individuals, the name of Barrett must find a place, and I greatly regret that my friend considers the reference to him to be infelicitous. As regards the first point raised by him (to take the class of smith's work with which I have most experience), those who have to conduct the very delicate operation of hardening dies for coinage have long been familiar with what is now known, thanks to Prof. Barrett, as "recalescence" in cooling steel. Of course the artificers were ignorant of its true cause, and they usually describe the effect of the sudden glow in steel as being due to "the heat coming from inside the metal." The fact that this industrial knowledge existed, does not in the least diminish the interest of Prof. Barrett's own observation (1873), nor lessen the vital importance of his work in showing that "Gore's phenomenon" (1869) is a reversible one. The relation of the work of Barrett to that of Gore was, moreover, indicated by me nearly ten years ago in the pages of NATURE (November 7, 1889, p. 16) as concisely as I could. In a recent number of NATURE (April 13, 1899, p. 567), a curve obtained by a method of my own is published, and it shows that there are no less than six points at which heat is evolved as iron cools down from 1150° C. to the ordinary temperature. I wish that Prof. Barrett, with his great experimental skill, had hastened the advance of our knowledge by continuing, during the past twenty-five years, investigations which would have led him to the discovery of the several very important points in carburised iron in which, as the

[1] In a note to the Iron and Steel Institute in 1890, and in correspondence with me subsequently, Dr. Gore points out that the discovery of the "sudden shortening during heating" he here attributes to me is more or less implied in his own paper in the *Phil. Mag.* for September 1870, where a molecular change occurring in iron during the process of heating is clearly mentioned. But the only evidence Gore gives of a molecular change during the *heating* of iron is the production of an induced current in a surrounding coil of wire when the iron core reaches a certain temperature; this he correctly attributes to the well-known change in the magnetic state of iron at this temperature. In fact, quoting from his previous paper, Dr. Gore states (the italics are his): "The iron during cooling . . . suddenly elongated by diminution of cohesion . . . a corresponding but reverse phenomenon did *not* occur during the process of *heating* the wire" (*Phil. Mag.*, September 1870, p. 171). In an interesting research on "The changes in length and temperature of iron and steel during recalescence," published in the *Phil. Mag.* for August 1898, M. Svedelius of Upsala supports the historical view I have here taken.