XII. *Further Observations on the Process for converting cast into malleable Iron. In a Letter from* Thomas Beddoes, *M. D. to Sir* Joseph Banks, *Bart. P. R. S.*

Read May 3, 1792.

DEAR SIR,

SINCE I described to you the process known among the workmen by the term, *puddling* of iron, I have several times reconsidered the explanation I attempted, in the same letter, of the phænomena it presents. My explanation could not indeed but be in great measure conjectural; and subsequent reflection excited in my mind a very lively wish to ascertain, in a decisive manner, the nature of the process. The following experiments will, I flatter myself, serve to determine the degree of confidence with which the principal points of my theory may be received, though they will not afford a solution of all the questions which my former communication might suggest to an acute philosopher.

They were undertaken in order to ascertain, 1. whether any elastic fluids are really extricated during the conversion of cast into malleable iron; and 2. what is their nature; and 3. whether they vary at different periods of the process, as I concluded from the appearances in the furnace. It seemed of less consequence to ascertain their quantity. I did not, however, neglect this object of inquiry, but you will find that some very curious circumstances prevented me from attaining it.

EXPERIMENT I.

Six pounds of dark grey melting cast iron were put into an earthen retort; a glass tube was luted to the neck, and its extremity was immersed in water. The retort was placed in a wind furnace. Before the retort and its contents could be supposed to be red hot, inflammable air came over. It burned with a deep blue flame, and was in no degree explosive. It rendered lime-water turbid, and was partly absorbed. When the retort had been heated about an hour and half, the air, which was coming over pretty copiously, that is, at the rate of an ounce measure every three minutes, upon an average, suddenly ceased, and the apparatus, on examination, proved to be no longer air-tight. The retort was found to be cracked; and the lumps of iron had none of them been melted, but they had been softened, and conglutinated together.

EXPERIMENT II.

Four ounces Troy of the same iron were put into one of Mr. WEDGWOOD's earthen tubes, glazed and closed at one end. That end of the tube was inclosed in a barrel-shaped crucible, the interstice filled with sand, and the crucible reclined so as to form a very small angle with the horizon: in other respects the apparatus was disposed as before. On the application of heat, air was again extricated, sooner than I should have expected, of the same inexplosive inflammable kind. About one-seventh of that which came over first, and which traversed the water of the receiving vessels, was absorbed by milk of lime. The residue burned slowly, with a flame apparently not so deep as before the carbonic acid was separated.

In this and the former experiment, the elastic fluids were most rapidly extricated on the first impression of a red or white heat. Afterwards they came over much more slowly; during a considerable part of this experiment you might count twelve slowly between every air-bubble.

When the utmost power of the furnace had been exerted for three hours, a phænomenon occurred which produced some surprise in every person present; and there were several who had been abundantly accustomed both to chymical and metallurgical operations. A considerable absorption took place, and for about half an hour, it was necessary to blow air up the glass tube, to prevent the water from rising into contact with the iron. It afterwards appeared that the lead of the glazing was revived, which sufficiently explains the absorption.

586 grains only of the iron had been completely fused. The surface of two of the unmelted lumps was curiously covered with numerous small blisters of metallic lead.

About seven hours after the fire was first kindled, it was discovered that the apparatus had failed. I had examined the air that came over immediately before this accident, both by means of lime-water, and milk of lime, without discovering any vestige of carbonic air.

The iron weighed altogether three grains more than at first. But the adhering lead, and a quantity of lead also which was incorporated with the iron, concealed a real, and probably a considerable, loss of weight. The phænomena it exhibited, when put into weak vitriolic acid, and the vitriolated lead which was formed, indicated the presence of this metal in all the superficial parts of the mass. When it had been kept some time in vinegar, it dissolved readily enough in vitriolic acid at first, but the solution soon ceased, or became very slow.

EXPERIMENT III.

A coated flint glass retort was employed in this instance. The apparatus resisted a strong heat for two hours; and air, of the heavy inflammable kind, came constantly over.

EXPERIMENT IV.

A coated retort of crown glass, containing six ounces Troy of the same iron, was placed on a crucible nearly full of sand, and disposed as in the former experiments. I now wished to measure the quantity of the air, and I therefore determined to receive it in mercury. It would have been in vain to attempt this in water, on account of the carbonic acid air. About twelve o'clock the retort was judged to be of a dull red heat, and inflammable air came over. The orifice of the transmitting glass tube was now covered to the depth of half an inch with mercury, when the discharge of air instantly ceased: the lute seemed entire. Some of the mercury being removed, so as to leave just enough to cover the mouth of the tube, immediately the air issued again in bubbles, a proof that the apparatus was entire. The mercury was poured into the trough again, and in an instant there was a cessation of air. The mouth of the tube being uncovered, and a lighted paper applied, a blue flame appeared, and continued to burn, so great was the quantity of air discharged. The orifice of the tube was one-tenth of an inch in diameter. We found that this constant flame could be produced at any time during three hours and an half. When water was substituted in place of mercury, air issued slowly, and as if with difficulty, under a pressure of five inches. When only half an inch was left over the mouth of the tube, small bubbles ascended freely. During a considerable time I counted four, slowly, between each of these bubbles. I did not collect above three ounce measures of air, and this contained carbonic acid. It was past four o'clock when the apparatus ceased to be air-tight, and the fire had been kept as strong as possible. The iron was most completely fused. There was a good deal of revived lead within the retort; there were also many globules in the neck. Probably some broken flint glass had been added to the usual materials for crown glass; I cannot otherwise account for the appearance of the lead here. In the last experiment the lead of the flint glass had been revived.

EXPERIMENT V.

Two ounces of the same iron, immediately upon being taken out of a retort, in which they had been kept, at a red heat, for about an hour and a half, and which were therefore as free from water as iron can easily be procured, were put into an earthen tube, unglazed, and closed at one end. This tube was disposed as in experiment II, only the end of the glass tube was immersed in mercury, instead of water. You will not be surprised to hear that air did not now come over so soon as in any former instance. When the fire was raised to its full force, exactly the same amusing variety of appearances took place as in the last experiment. Under the pressure of half an inch of mercury, not a particle of air was discharged; but the moment the pressure was diminished to a small fraction of an inch, the bubbles succeeded each other pretty quickly; and so on repeatedly. Upon lowering the surface of the mercury, and pouring some water upon it, I received more than two ounce measures of air, which, by the test of lime-water, did seem to contain a vestige of carbonic acid, but it was too minute to be appreciated. This experiment with the air was made after a strong white heat had been kept up for three hours. Soon afterwards the bubbles ceased; but we could not then, nor upon examination of the apparatus when cold, discover any failure. The fire was still kept up for three hours. The tube must have been exposed to a strong white heat seven hours in all. The iron had lost eleven grains in weight. Only about one half had been thoroughly fused. The surface of two lumps, that had not been fused, had the close texture, and silvery appearance, of malleable iron. The thin edges yielded to the stroke of the hammer, and a gentleman, perfectly conversant in the nature of iron, agreed with me, that it had all the characters of malleable iron.

EXPERIMENT VI.

Thirty-one grains of artificial plumbago, in shining flakes from the iron works, were exposed in a small retort to a strong heat, for six hours, in the same pneumatic apparatus. It was difficult to separate, even by the help of the magnet, all the intimately mixed particles of iron, and there were also a few particles of coak incorporated with the plumbago. Air, of an explosive inflammable kind, was extricated, and rose freely through five inches of mercury. We had not been sufficiently careful to let the lute fix before we commenced the experiment, and it soon failed. Upon taking off the pressure of the mercury entirely, and repairing the lute as well as we could, we had every reason to believe that the air soon ceased. The air received in the mercury contained one-eighth of carbonic acid. The remainder exploded. The plumbago lost four grains. Mr. PELLETIER, if I remember right, found that native plumbago, exposed to a fierce and long continued heat, lost 10 grains in 200. In the present experiment, its appearance was unaltered. Probably the loss was owing to moisture imbibed by the particles of coak, and to a small combustion by the air in the retort.

It will, I think, be admitted, that these experiments abundantly confirm the inferences I had formerly drawn from appearances by their nature less decisive. The real extrication of air, varying in its nature at various periods of the process, seems to be placed beyond doubt. The experiments in glazed and glass vessels, were made with a view to exclude the possibility of the supposition of the air entering through the pores. I think that Dr. PRIESTLEY, if he should repeat these experiments, and find that they have been accurately made, will, with his accustomed openness to conviction, abandon an opinion he has for some time entertained, and no longer consider water as essential to the constitution of elastic fluids. Several observations might be made upon this point, and those which I have just noticed above; but they will readily occur to persons conversant in chymistry, and it is not the object of the publications of the Royal Society to teach the elements of science. I shall, therefore, confine myself to the unexpected and anomalous appearances, and then attempt to draw a few useful inferences.

1. I was surprised at the extrication of inflammable air in such low degrees of heat. You have seen that cast iron, highly charged with charcoal (the *phlogisto onustum* of BERGMAN) yields air at the temperature of melting lead. For undoubtedly the blisters of lead, which lay upon the iron, are to

be considered as air bubbles caught in a solid film of lead. Perhaps white cast iron would not yield air so readily; possibly iron holds its charcoal with more force as it contains less. I intend to make some comparative experiments upon the varieties of cast iron, but so curious an appearance as these blisters, will always be rather the bounty of accident, than the effect of skill or labour. The obvious method to produce them, would be to cover the iron with lead. All the unmelted lumps of iron were porous, and open in their texture.

2. I am at some loss how to explain the occasional discharge and cessation of air, in one experiment in which a crown glass retort was used, and in another with an unglazed earthen tube. There was no flaw in the lute, nor in the vessels, for it was discharged for the space of several hours under a small pressure. Either, then, it was forced through the softened glass in the first, and the dilated pores of the tube in the second case; or it was absorbed by the substance of the vessels; or it was not extricated from the iron. Of these suppositions the third seems to me the most probable. It is not likely that an hole should be made through the melted glass, under the pressure of the half, and closed under that of perhaps the eighth of an inch; or that pores in the tube should open and shut in conformity to such a variation of circumstances: and, with regard to the tube, there can be no question as to absorption. One principal difficulty, as it appears to me, in the manufacture of iron, is to get rid of the charcoal. The oxygène readily enough unites with a small portion; but the attraction of the iron on the one hand, and on the other, the little disposition of the charcoal to put on the elastic form, in comparison with many other less fixed substances, together form a

very considerable obstacle to the change of charcoal into air; and as I have already observed, the iron probably holds the charcoal more strongly as its quantity diminishes. In this state of things a small additional impediment will prevent the heat from throwing the charcoal into the state of air; and some degree of pressure must be adequate to this effect: and why may not this point, from which as you recede on opposite sides, the attraction of the particles of charcoal for one another, or for iron, either shall or shall not be overcome by heat, have been found in these experiments? The next consideration will both illustrate and confirm these ideas.

3. A chymist, whose notions of iron are derived principally from books, and from the phænomena which are presented by processes not having metallurgy for their immediate object, will be apt to consider some things related above as inconsistent: the violence of the heat, for instance, and the smallness of its effects; since even cast iron was not fused in all the experiments. The fact is, when cast iron exposes a large surface, and heat is gradually applied, it proves almost as infusible as malleable iron: indeed, by the gradual action of heat it is converted, superficially at least, into malleable iron, or approximates towards it: and considering only iron and charcoal, I believe, *the fusibility of iron will be directly as the quantity of charcoal it contains*. Now in the experiments I have described, pieces of one, two, and three drachms, and sometimes less, were used, for larger could not be inserted into the neck of the retort. And, in order to avoid this inconvenience in future, I would recommend cylinders to be cast, of a diameter suited to the mouths of the vessels. This infusible coat would be an impediment to the conversion of the parts below, by

pressing upon them; the elastic fluids could not either traverse the solid surface so freely as a liquid, and perhaps, as I am disposed to believe, they could not traverse it at all. The malleable skin seems close in its texture, and the porosity of the rest might arise from the generation of just air enough to produce an internal expansion. In the puddling operation, it is of the most material consequence to keep the mass in constant agitation. Thus the parts are thoroughly blended, the attraction of cohesion is a good deal counteracted, and there can be no pieces hide-bound, if I may so express myself. This last, perhaps, is the greatest advantage derived from the labour of the workman.

4. I was asked by one of the most ingenious and profound philosophers of the present age, why I had neglected the action of the atmospheric air in the theory of the conversion of iron? It is simply because its action upon the metal seems, in practice, pernicious; I consider its presence as an evil, though a necessary one, according to the present modes of working; I was also anxious to try this opinion by the test of experiment, and you see it has been fully confirmed. In the last experiment, part of the iron was completely converted, and in some others it seemed approaching fast towards *nature*, as the manufacturers express it. It is, indeed, very possible to conceive a way in which air might be beneficial; that is, if it could be applied so as to burn the charcoal merely; but at present, for one grain of charcoal which it converts into carbonic acid air, it converts many of iron into finery cinder; and as I have formerly shewn, this is not the way in which iron is actually converted in the reverberatory, and probably not in the finery, furnace.

5. It is impossible to ascertain the principles of any art, without immediately improving the practice, or opening a prospect of future improvement. The preceding observations may serve to direct attempts to render the metallurgy of iron less difficult, laborious, and expensive. For, 1. if a quantity of oxygène, nearly sufficient to burn the charcoal, could be chymically combined with the cast iron, the operation would consume less fuel, and would not require so long a time. It may be worth while to consider if the ores of iron, containing manganese, owe any part of their value to this circumstance. 2. If it could be contrived to apply a sufficient heat to large quantities of iron in close vessels, and at the same time, to agitate them sufficiently, the loss in conversion would not, perhaps, exceed ten in an hundred. 3. The important object of converting British iron into steel, may possibly be attained by following up reflections suggested by the foregoing experiments. When the oxygène has been separated in the form of carbonic acid, there will remain the charcoal and iron, the constituent parts of steel. Perhaps the materials, at a certain period of the process, may be so nearly approaching to steel as to be easily convertible. The mass will contain also a quantity of sulphur, on which perhaps the difficulty of making good steel from our iron depends. But this difficulty, I am persuaded, will not be insuperable.

It may be proper to add, that whenever attention was paid to it, the hepatic smell in the extricated air was perfectly distinguishable.

I hope you will also permit me to add, that whatever information or advantage may be derived from these facts and observations, must be in a great measure ascribed to the liberal curiosity of WILLIAM REYNOLDS, whose enterprising spirit and inventive genius have improved our machinery, enlarged our manufactures, and changed the face of a large district in his native county.

I have the honour to be, &c.

Shifnal, Shropshire,
February 14th, 1792.

THOMAS BEDDOES.

P.S. The residuum of 486 grains of cast iron, the same as that used in experiment 1. weighed $48\frac{1}{2}$ grains, after being dissolved in weak vitriolic acid, and heated to a dull red heat; the same quantity of iron, after the experiment, afforded a residuum of 39 grains, and a little more; in the residuum left by equal quantities of iron, before and after the experiment in the unglazed tube, there was a difference of five grains; the solution of the iron that had been submitted to the experiment went on very slowly; and would not have been effected by vitriolic acid in many months. In the latter case, I used some muriatic acid, which quickly dissolved it: in the former, weak aqua regia was used for the solution of a very small part of the whole lump. I suspected lead to have caused the slowness of the solution in the first case, but there can be no such suspicion in the second. The difference between these residuums tends to shew that plumbago was consumed by the heat: but they do not shew the loss accurately; for in the residuum of the iron that had been fused in the first experiment, there was a small quantity of vitriolated lead; and in the other there was, besides the plumbago, a small quantity of that difficultly soluble calx of iron, which the solutions of this metal deposit on long exposure to air. The difference was greater, therefore, than it appeared. On the other hand, the long action of the acids might have consumed some plumbago. There was little or no calx attractable by the magnet in the residuums of the fused iron. From the 48 grains of residuum, I separated more than six grains by the magnet.