procrastinatory caveats became less effective in preventing grant of patents. Perceiving a better chance of patent protection, more inventors filed more patent applications: e.g. 7 patents granted in 1750 rising to 455 in 1851. Learned judges Mansfield (1778) & Buller (1785), for demanding a good specification and Lord Brougham, by Parliamentary achievement of examination and fees payable by instalments, much improved the system.

Part II discusses how inventors tried to exploit their patented inventions as the generally educated inventors of the 17th century were displaced in the 18th century by inventors who were actually engaged in the trade they sought to enhance. Chapter 7 lists prices at which inventions changed hands, e.g. D Mushet's 1835 patent to W Mushet for £15,000; D F Taylor's patent relating to a pin-making machine sold for £3,775 provided he made it work; £5 per horse power was an interesting royalty on use of a steam engine. Risk of bankruptcy deterred some inventors from manufacturing their inventions.

Little is said about the actual inventions but the bibliography tells one where to find out more. Those who do business in intellectual property might find the £32.50 well spent to get the explanation of the conclusion ". . . the imperfect patent system . . was probably most appropriate for the period of the industrial revolution".

R A Owen

Letters to the Editor

Phosphorus in low carbon iron

Dear Sir,
I will try to answer the questions raised by Martha Goodway and Robert Fisher in their paper on the influence of phosphorus on the properties of low-carbon steel/wrought iron.[1]

These questions were:
1. The phosphorus contents of the three iron wires from 1732-1782 — 0.1, 0.15 and 0.22% P - are conspicuously high. "Could this have been intentional?"

2. When carbon was not recognised as a constituent of iron "was decarburization the intended result?"

3. "How was this nearly complete decarburization achieved?"

Firstly, I would like to say that I think the results of the chemical analyses are not very exact if they are the result of microprobe analysis — as acknowledged to S S Brenner.

Secondly, for further consideration, it would be very necessary for me to know if the iron used to make these early wires, was produced by the direct/bloomery process, or by the indirect/finery process. Unfortunately I cannot tell because there are no microphotographs of the metal structures, and the silicon and manganese contents are missing in the paper.

So my answers would be:
1. It is possible that the phosphorus content in the wires has been intentional.
I found this problem when studying the phosphorus content of separate ferritic iron layers existing beside low phosphorus iron in knives from LUTOMIERSK 11[2], DANZIG 11[3] and TUM near LECZYCA 12 - 13[4 5 6], and in spearheads from LUTOMIERSK[2].
In all nine specimens the phosphorus content was in the range 0.23% to 0.45% P - arithmetical mean of 0.31%- analysed using the classical method, similar to the Goodway/Fisher results.
High phosphorus iron layered with low phosphorus iron was used to produce pattern welded knives, or other instruments, in Medieval times.[7] Iron containing more than about 0.4% could not be used because it would be too brittle, and also it could not be used for wire production. Ferritic iron, containing 0.15% P, I found in a Roman pattern-welded sword from WACHOCK about 3[8].
All the above mentioned implements were made from iron smelted in a bloomery.
The element phosphorus was discovered in 1677 but early smiths/metallurgists recognised it in metal during forging - its effect on plastic properties, strength, brittleness. They defined high phosphorus iron as "brittle iron"[9 10]. The influence of P on properties of iron and steel was invesigated by d'AMICO[11], in 1913, before Stead.

2. Decarburisation was the intended result because the production of low carbon/steel iron wire was quite easy. It was not a problem for early metallurgists. In the bloomery they obtained ferritic, uncarburised, iron directly from the ore. In the finery, it seems, it was easier to obtain full decarburised metal than to control middle carburisation.

3. So low decarburisation could be achieved without difficulty - but I am not sure that the analysis quoted is correct.

Sincerely yours,
Prof. Jerzy Piaskowski

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10. **J Piaskowski,** *Phosphorus in bloomery iron. Archaeo-materials (in printing).*

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Dear Sir,

This refers to the review of *The Crafts of the Blacksmith* by Raymond Haynes and published in *Historical Metallurgy* 22 (2) 1988 p 118-120. In particular, I am raising a few points regarding the review of my contribution to that volume.

Mr Haynes, of course, had no knowledge of the events leading up to the publication. On the way to the printer, Scott's auto was broken into and among the items taken was my manuscript containing the original illustrations. (Doesn't it show good taste on the part of the thief to have taken **my** article?). The original illustrations all had **micron markers** on them! When Scott telephoned from Belfast all upset by the event, I had to scurry about to replace the illustrations as best I could. The originals were all far better than those finally reproduced time not permitting the finishing touches such as micron markers nor replacing even the ones originally selected to indicate the points made in the text.

Mr Haynes uses what I consider an unfortunate word *misleading* to describe my account of the physical metallurgy of iron and steel when it would have been more accurate and kinder to use the word *simplistic.* I have no idea which subject Mr Haynes read in university; I'm sure however, that he is aware of the need to condense the physical metallurgy of iron and steel into a few pages and at a gathering *not of metallurgists but primarily of archaeologists and historians* such as the one in Belfast at which the paper was read. As a retired former professor of metallurgy with some 38 years of teaching metallurgy both ferrous and non-ferrous metallurgy I am in total agreement with Mr Haynes that the reader would be better served to consult one of the many excellent text books in physical metallurgy. The task, however, at the conference and in the publication was to **condense the text book into a few pages.** I would appreciate reading Mr Haynes' account of the physical metallurgy of iron and steel limited to the number of pages allotted to me. Is being simplistic also being misleading? I expect so for those who wish to consider it as such.

Mr Haynes places in the same paragraph comments regarding the experiments of Nosek and Mazur making it appear as if their results apply to the case of relic structures. Certainly, any one knowledgeable with oxidation mechanisms of iron, those who have read the studies of scientists such as Gulbranson, Birchenall, Mehl, et al will appreciate that the structures produced by accelerated corrosion tests *do not necessarily emulate* long time corrosion structures. Hence the Nosek/Mazur results hardly apply to the cases reported in my chapter; the positioning of the comments make it appear as if they do.

Regarding relic structures, scholars interested in ancient iron metallurgy are divided into three groups; one contains those like Mr Haynes who remain unconvinced by the relic structures; those who say they are convinced (and I know many); and those who are on the side lines awaiting further information.

Lastly, I say again that it was unfortunate that Scott's auto was broken into.

Very truly yours,
Robert Maddin