PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A
MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Walter Rosenhain and Materials Research at Teddington

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Phil. Trans. R. Soc. Lond. A 1976 **282**, 5-36
doi: 10.1098/rsta.1976.0050

Walter Rosenhain and materials research at Teddington

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1. Introduction

At this Conference we aim to honour the memory of Walter Rosenhain who initiated formally the study of materials at the N.P.L. and we wish also to look forward by examining the contribution of scientific studies of the structure and constitution of materials to engineering practice. To produce an impact was one of Rosenhain's aims and I have been told that the concept of such a Conference as this, and its method of organization, would have appealed to Rosenhain were he here.

I have designed this written presentation, of which a shorter version will be presented at the Conference, to cover three themes. The first is that of Rosenhain the man and metallurgist and what he and his colleagues brought about. The second sketches the historical development of the scientific study of materials on the Teddington site and hence must touch upon the organization of this and with the growth of the Engineering Department at the N.P.L. – which gave rise to the National Engineering Laboratory; and with the story of the National Chemical Laboratory. The third part is very much a personal view and deals with the definition of a technologist.

Several memoirs of Walter Rosenhain appeared shortly after his death on 17 March 1934; obituary notices in the *Journal of the Institute of Metals* (D.H. 1934); *Obituary Notices of Fellows of the Royal Society* 1932–35 (Desch 1934) and the Institute of Metals Autumn Lecture in 1934 delivered by Dr J. L. Haughton, one of his colleagues under the title 'The work of Walter Rosenhain' (Haughton 1934). Haughton's account contains a list of all of Rosenhain's published papers. A recent reappraisal of his work appeared in the *Dictionary of scientific biography* (Cahn 1975). I give the main facts concerning his life and the honours he received in Appendix 1.

2. First researches

Walter Rosenhain was born in Berlin‡ on 24 August 1875. The family emigrated to Australia when he was five and he learned English on the voyage out there. He revisited Europe in 1890 and again in 1894, interrupting his University course to do so. He graduated from Queen's College, Melbourne in civil engineering and went to Cambridge as an 1851 Exhibition scholar at St John's in 1897 at the age of twenty-two. He had published one paper in Victoria on determining the specific heat of a liquid, and worked initially on steam jets. However, advice from (the later) Sir Alfred Ewing, F.R.S. led him to examine plastically deformed metals under the optical microscope.

Sorby (1826–1908) had first looked at metals but was primarily a geologist and though a descendant of the first master cutler was not interested in metals themselves but principally

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‡ This fact is ascertained from his daughter Mrs N. Kirsner – the published memoirs, with the exception of Cahn's are in error on this point.

as a geologist. He examined iron and steel because of his interest in meteorites. His own view of his work on metals in 1897 was 'What I really proved was that various kinds of iron and steel are varying mixtures of well defined substances and that this structure is in many respects analogous to that of igneous rock'. Though Sorby first investigated metals in 1863–64 it took him a long time to publish all the results as befitted a man of private means and his work at high power (×650) was published in 1886 more than twenty years after it was done. In 1877 Adolf Martens began a series of microscopical studies of iron and steel, unaware of Sorby's work. By the nineties of the last century a number of eminent metallurgists were using the microscope to study phase changes in iron – Roberts Austen, Stead and Arnold in the U.K. and Osmond in France. Rosenhain looked at a polished surface of a plastically deformed polycrystalline iron and discovered slip within the grains. Slip in crystals was first discovered by Reusch (1867) in rock salt but Rosenhain does not refer to this. Rosenhain and Ewing gave an account of their work in several papers including the Bakerian Lecture in 1899, Ewing & Rosenhain (1899, 1900*a*, *b*, 1901). In those days the *Proceedings of the Royal Society* were not divided between physical and biological sciences and the first paper appears between one on a sugar bacterium and another on the physiological action of choline. The paper reported slip in iron, steel, copper, silver, lead, bismuth, tin, gun metal and brass and in nickel in an added note.

The authors recognized the need for a grain to accommodate itself to its envelope of neighbouring grains as strain proceeds, but say that the region between slip bands appeared to be rigid, and from the observations on nickel deduced a lack of any foreign matter in the intergranular junctions. In later papers they extended the observations to zinc, cadmium, gold and to lead eutectics. They claimed to find deformation twins in lead, gold, silver and copper but these, I believe, were either due to recrystallization or were deformation bands (see, for example, Barrett 1949). They certainly found deformation twins in cadmium, zinc and tin. They concluded that slip and twinning were the modes of plastic deformation of metals. Ewing and Rosenhain also studied recrystallization and deduced some of the laws of recrystallization much as we know them today (e.g. Burke & Turnbull 1952), Ewing & Rosenhain (1901). The explanation of how recrystallization occurs involved the notion of a eutectic being formed between the grains which allowed the metal to dissolve on one side and to precipitate upon the other; since recrystallization did not occur in a non-deformed metal the notion was that deformation led to break up of the eutectic film which was regarded as hard and brittle – the notion of hard grain boundaries persisted in Rosenhain's thinking for many years. The deduction was made that two welded pieces of metal will not allow recrystallization across the weld, which is true unless extraordinary care is taken. If clean sand or other material was introduced between the welded pieces again there was no recrystallization. The experiments were very ingenious but came to quite the wrong conclusion, namely that pure metals would not recrystallize. In fact very pure metals do so much more easily than others (Albert & Le Hevicy 1956).

In all of these papers Rosenhain is shown as a very ingenious, imaginative and thorough experimentalist. The experiments to test his ideas on recrystallization indicate this and his proof that slip bands were steps on the surfaces of crystals is classic. This he did by electro-depositing copper upon a surface after slip and then cutting a section through the two metals to photograph the steps in profile. This is quite brilliant; the steps are only at most 10 μm high.

Ewing and Rosenhain were concerned with polycrystalline metals of commercial importance. They make no reference to Mügge's work which appeared in 1899 which was carried out on native crystals of gold, silver, copper, iridosmium and iron, which established the glide plane as {111} and *slip direction* ⟨110⟩. Mügge also *measured* Neumann bands and deduced they were twins, a fact which the metallurgical fraternity did not regard as proven until 1953 (Paxton 1953; Kelly 1953). Mügge was the great classical crystallographer studying crystals for their own sake. Ewing and Rosenhain, of course, were engineers. Their work proved that polycrystalline metals used in the foundry and in the shop were crystalline. This was a great advance in thinking at the time; they only thought of 'metals'. In the same issue of the *Philosophical Transactions* there is a paper by Adams & Nicholson (1900) which reports an experimental investigation into the flow of marble and slip and twinning are found to occur. Rosenhain and Ewing are referred to. The essential modes of plastic deformation in both polycrystalline metals and in rocks were therefore established at much the same time but the two fields have served different technologies and made only peripheral contact since.

3. GLASS INDUSTRY

Rosenhain's initial work then gave him prominence among scientists in the metallurgical and engineering world. A number of opportunities were offered to him on leaving Cambridge in late 1900 – a chair in Poona, a demonstratorship in Engineering at Cambridge, a position as a civil engineer in Australia, and the one he accepted as 'tame scientist kept on the premises' by Chance Bros Ltd, Glass Manufacturers in Birmingham. In 1901 his probation with the firm ended and he then married Louise Monash, with whom he had corresponded weekly since leaving Australia three and a half years before.

At Chance Brothers he worked on optical glass and light-house apparatus and started his study of refractory crucible materials of high purity. He wrote a book on glass manufacture (Rosenhain 1908) which was published after he had left Chance Bros in 1908 and was revised in 1919. During the 1914–18 war there was a shortage of optical glass in this country – most had been imported from Germany – and to offset this disadvantage glass of optical quality had to be produced in Britain. Rosenhain was instrumental in helping this and to support it was commissioned to carry out research work on glass production at the N.P.L. His views are described in his three Cantor Lectures to the Royal Society of Arts (Rosenhain 1916*a*) in which he describes in detail the properties required of optical glass, the defects in it and the methods of production. The lectures also make a number of comments on the organization of the optical glass industry in Britain.

During the time with Chance Brothers his heart remained in physical metallurgical research and in order to sustain this he set up a small private laboratory at his home in Edgbaston, financed by a Carnegie Research Grant from the Iron and Steel Institute. His wife Louise shared his work and he trained her in the preparation of metallurgical specimens. He published papers on the deformation of iron and steel and discussed the mechanism of fracture (Rosenhain 1904, 1905). At this time he began to believe that work hardening was due to destruction of the crystal structure on the slip bands due to the production of a vitreous layer and, in addition, he began to adopt the view that boundaries between grains also were of an amorphous structure.

He continued to develop instruments and his improved form of coal calorimeter (Rosenhain

1902) was exhibited at a Royal Society Soirée in that year. Because of his interest in pure metallurgical research, he was naturally attracted, when in 1906, the post of Superintendent of the Department of Metallurgy and Metallurgical Chemistry at the National Physical Laboratory was offered to him.

4. The National Physical Laboratory

Work on metallurgy and chemistry was started simultaneously in 1902 at the N.P.L. as two sub-departments of the Physics Department. The work on metallurgy was started at the instigation of the Alloys Research Committee of the Institution of Mechanical Engineers. It was in charge of H.C.H. (later Sir Harold) Carpenter F.R.S., who started his professional career as a chemist. By 1905 Carpenter's interests were almost entirely centred round metallurgical research and he resigned from the N.P.L. to take up the newly created chair of metallurgy at the Victoria University of Manchester in the autumn of 1906†.

A separate department of the Laboratory was then formed entitled Metallurgy and Metallurgical Chemistry and Rosenhain was appointed the first Superintendent.

He stayed there twenty-five years and during this time his Department grew from a staff of four to one of seventy. Among the various biographical notes of Rosenhain, the fullest description of the scientific work he directed at the N.P.L. is given by Haughton (1934).

The immediate need was to understand the constitution of alloys. This started with an investigation of the alloys of lead and tin (Rosenhain & Tucker 1908). In the Ninth Report to the Alloys Research Committee (Rosenhain & Lantsberry 1910) phase diagrams and properties of copper–aluminium–manganese alloys are dealt with and their corrosion resistance studied. Remarkably high tensile strength and elongations were obtained, namely 700 MN/m^2 with an elongation of 16 % in an alloy containing 10 % Al and 1 % Mn.

The report mainly deals with copper-rich material but discovered an alloy of aluminium with 3 % Cu and 1 % Mn to have excellent properties. This is close to the composition of Duralumin (3.5–5 % Cu, 0.5–1 % Mn plus about 0.5 % magnesium) which became a most successful light alloy.

There followed investigations of alloys of aluminium and zinc summarized in the Tenth Report to the Alloys Research Committee (Rosenhain & Archbutt 1912) in which the idea of specific strength (called specific tenacity) is first introduced. Further work on the light alloys was continued during the war but much was unpublished until afterwards. A lot of it is published in the Eleventh Report to the Alloys Research Committee (Rosenhain, Archbutt & Hanson 1921). The constitution and properties of Y alloy (4 % Cu, 2 % Ni, 1.5 % Mg) and others, which were much used in airships, aircraft and car engines in the 1920s and 1930s were described.‡ These alloys were used directly and discussions of the papers show an intimate interaction between the N.P.L. work and those fabricating the alloys in manufacturing industry. Even as early as 1921 the suggestion that age hardening of Duralumin is due to precipitation on a scale too fine for resolution in the optical microscope was put forward. At the same time Jeffries & Archer (1921) advanced a similar explanation in the U.S.A. Preston (1938) later proved this at the N.P.L. a few years after Rosenhain's death.

† In 1913 he left Manchester to take up the Chair of Metallurgy at the Royal School of Mines. He was for a number of years a member of the Executive Committee of the N.P.L.

‡ This alloy has been said (Hunsicker – this symposium) to be a precursor of the alloy RR58 selected for use in the airframe of Concorde (2 % Cu, 1.5 % Mg, 1.2 % Ni, 0.1 % Ti).

By the end of the war the number of ternary phase diagrams which had been investigated was becoming large and three-dimensional phase diagrams being widely used. Rosenhain (1920) introduced a neat wire model for displaying the phase fields.

Investigations were also made of bearing metals (Fry & Rosenhain 1919) and on zinc-copper-aluminium alloys (Rosenhain, Haughton & Bingham 1920). This alloy was investigated during the war as a possible substitute for brass for making shell fuses. Rosenhain had suggested zinc alloys as a possible substitute and Zn with 4% Cu and 7% Al was found suitable. According to Haughton the alloy went into production for a short time.

In all of the metallographic work high standards of accuracy were emphasized. Pure materials, as subject of the experiment, pure crucibles, well calibrated instruments, patience in readings, careful microscopy are all insisted upon (Rosenhain 1909). The phase diagrams then produced persisted for many years.

Other work was carried out at the N.P.L. under Rosenhain's direction and he often wrote the introduction to a series of related papers. A notable example is a whole series of papers on the alloys of iron. In his first introduction Rosenhain (1924) describes how he obtained support for the research and gives an outline of what is to be attempted by his team.

Insistence on high standards of accuracy in scientific observation must provide a drive towards improved and novel instrumentation. Rosenhain's design of method and of apparatus is one of his outstanding achievements.

He championed the inverse method of plotting cooling curves. This rests upon measuring the time taken by a specimen to change its temperature by a small number of degrees – say 3 – and plotting this against the temperature. It was simple and reliable though not as accurate as the difference method in which the difference in temperature from a standard specimen was noted. The good measurer or standards man recognizes the appropriate degree of accuracy for the problem in hand.

Advances in metallography were numerous among him and his colleagues. He supported the edge of a specimen during mechanical polishing by depositing a layer of copper before sectioning so that the edge could later be examined under the microscope. This was applied in many ways and became standard for examination of fractures. In order to quench rapidly he used the (then new) silica tube so as to wash a specimen from a hot surface with a stream of water. Mercury is difficult to examine metallographically, and with Murphy a method was developed (Rosenhain & Murphy 1926). A great variety of etchants were produced. Rosenhain designed a metallurgical microscope. This was patented in 1905 and later manufactured by Messrs Beck.

Rosenhain made two large contributions to apparatus for thermal analysis. These were a gradient furnace and plotting chronograph. The second appears very out-of-date today but was important at the time in enabling readings to be taken quickly according to the inverse cooling rate method of thermal analysis. The first arose from the need to subject a specimen to a constant rate of change of temperature over a wide temperature range and was accomplished by arranging the furnace vertically, by heating the top and cooling the bottom with running water and moving the specimen through it at a constant rate. Heating the top prevented irregularities due to convection. Very simple in concept, after it has been conceived! It worked beautifully.

Rosenhain's scientific work covered three aspects. The investigation of the constitution of alloys, the design of apparatus and experiments. These are lasting achievements of a high order though the experimental data will be repeated and the instruments superseded. What motivated him greatly as a man were certain theoretical ideas concerning the crystalline structure of metals. These acted as great stimulants to himself and others and produced much discussion and experiment and some controversy. I hope that it is not too harsh to point out that few of his theoretical ideas have stood the test of time which brings with it more thought and further experiment.

I give below (β iron and amorphous metal) an account of the science of two of these controversies, in which he was engaged, but before doing so, will deal with his other activities. He published a great book *An introduction to the study of physical metallurgy* (Rosenhain 1914) which in its three editions was the standard work on this subject and led to its recognition as a particular field of scientific endeavour. He was in much demand as a consultant, with wide contacts in the metallurgical and mechanical engineering industries. He wrote widely over many years and conducted the *Metallurgist*, a metallurgical supplement to the *Engineer*. This journal reads oddly nowadays since many of the articles were unsigned. A large number are due to Rosenhain and by writing anonymously he exerted considerable influence. He published in the *Engineer* (Rosenhain 1923*a*) a series of reviews of metallurgy in the United States; his views culled from his visit in that year.

Man of energy as he was, he did very much for metallurgy as a profession; a founder member of the Institute of Metals, President in 1928–30, a member of the Iron and Steel Institute and of the Institute of Physics. He made the work of his Department at the N.P.L. widely known and brought it to its 'customers' through technical committees and the Council of the British Cast Iron Research Association, British Non-Ferrous Metals Research Association, as well as technical committees of the Department of Scientific and Industrial Research, the British Standards Institution and the Air Ministry.

He was a profound internationalist. Speaking French and German fluently he was often asked to lecture on the mainland of Europe and he took a special interest in the International Association for Testing Materials, particularly as it was put together again after the First World War.

His account of metallurgy (and science generally) in the U.S.A. in 1923 makes entertaining reading and then (as we would now perhaps) he remarked 'Thus, while England may claim to lead in the development of metallurgical science, its practical applications have been carried much further in America'.

Rosenhain left the N.P.L. in June 1931 and set up as a private consultant with offices in Victoria Street. He was a consultant to three well-known firms; Stones' at Deptford, which was later joined by A. J. Murphy one of his clever young collaborators, British Aluminium and the Broughton Copper Co. At the first named he helped with the manufacture of lead bronze steel backed bearings just introduced in the U.S.A. Besides work for these firms he was frequently called to Court as an expert witness. He continued to write for the *Metallurgist* and in an editorial in February 1933 rebuked a German for national chauvinism, as the left would call it nowadays, for referring to the 'german metal'.

He died after a long illness on 17 March 1934 and as *The Times* said 'was a brilliant metallurgist...covering the whole field of metallurgy both ferrous and non ferrous'.

The University of Melbourne opened the Rosenhain Memorial Laboratory on 3 July 1935 and the Institute of Metals introduced a Silver Medal named after him in 1951.

5. β-Iron

In the first edition of *An introduction to the study of physical metallurgy* 1914 the iron-carbon phase diagram up to 5 % carbon by mass appears on p. 161 as figure 64. There is a line *FG* at a temperature of about 770 °C linking the pure iron axis and the γ phase field. In the third edition, 1935, published after Rosenhain's death – revised and partly rewritten by J. L. Haughton – the line appears dotted as it does in all modern phase diagrams; figure 1. The change represented by this line is the Curie point of body-centred cubic iron. Above this temperature of 768 °C, the modern value, iron is paramagnetic, below it, it is ferromagnetic. In the early years of this century a mighty controversy raged about this change point in iron. A lively discussion of this has been given by Cohen & Harris (1965) and the nature of the controversy is probably most clearly learned by reading a definitive work by Burgess & Crowe (1913) as well as the paper and the discussion of it which finally settled the controversy by Arné Westgren in 1921 (Westgren 1921).

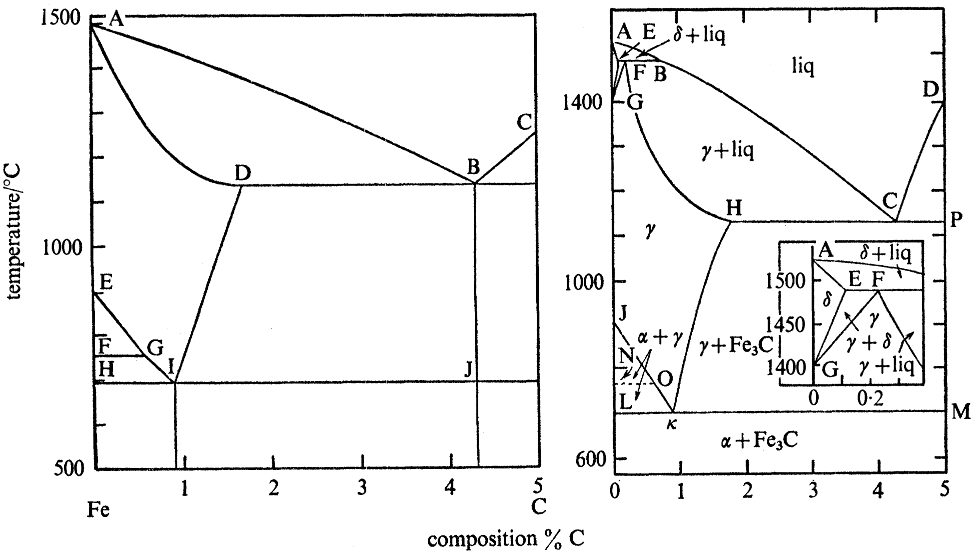

Figure 1. Phase diagram of the system iron plus carbon to 5 % carbon as it appeared, left, in the first edition of *Introduction to physical metallurgy* (1914) and right as it appeared in the third edition (1935).

There are two elements in the background to the discussion. Firstly, one of the main thrusts of scientific metallurgy then, as now, is the need to describe the phase changes accompanying change of temperature and composition in an alloy system. At that time many binary alloy fields had to be explored. The main tools to discover phase changes were measurement of the thermal effects – discovery of thermal arrests upon heating and cooling – and observation with the optical microscope of the surface of the specimen. A thermal arrest indicated a phase change. Measurement of physical properties also indicated phase changes since there were often discontinuous changes in these as a particular phase boundary was crossed. To investigate optically, alloys showing phase changes at high temperatures is never easy and specimens are often examined at room temperature following rapid cooling from a temperature of equilibration.

The classification of transitions now followed is to distinguish those in which the first derivative of the Gibbs free energy with respect to temperature (entropy) changes and the first derivative with respect to pressure (volume) also changes; these are called first order transitions. These are separated from those in which entropy and volume are continuous but their derivatives (specific heat and compressibility) are not; these are called second order transitions. If the entropy is continuous there is no latent heat but usually an anomalous specific heat. Such is the case with iron at its Curie point. This sophisticated idea of different types of phase change was formally established by Ehrenfest (1933).

By 1909 it was known that iron showed an anomaly in the specific heat over a range of temperatures near to 770 °C and this was thought to indicate a thermal arrest, i.e. a latent heat – even today such confusion can arise. Careful experimentation showed no volume change, or a very small one, compared with that known to accompany the change from α to γ iron at 910 °C. These differences could have led to a recognition of the anomalous nature of the phase change in iron at its Curie point. However, the second element in the discussion made this difficult at the time. Steel may be hardened by rapid cooling from the γ range when a diffusionless transition occurs and rapidly cooled iron shows a metastable phase known as martensite. To successfully harden steel martensite is produced by rapid quenching and then annealed to bring about some decomposition of the hard phase to produce a softer but tougher usable product. The production of martensite and its decomposition produce changes in microstructure over a scale of distance which is only partly visible in the optical microscope.

Since the phase changes accompanying cooling of steel at various rates (first sketched clearly by Bain and coworkers in the 1920s and 1930s) was, and is, of such great practical import, it was expected of the scientist that he should recognize the high temperature phases of iron which were of importance in quenching and explain how these initially hardened the steel and how on subsequent ageing the mechanical properties would be affected. The Curie point in iron occurs at a temperature from which γ iron can be effectively quenched. No wonder such a putative change of crystal structure should arouse such interest!

Rosenhain's part in the controversy arose because he decided to establish the modes of plastic deformation of iron at elevated temperature by observation (at room temperature) of the slip lines and other features formed by straining in a partial vacuum at elevated temperature. At the time three forms of iron were recognized, α, β and γ at the temperature ranges shown in figure 2. A_1 does not occur in pure iron. Osmond & Cartaud (1900) had very ingeniously established by crystallizing iron through the reduction of ferrous chloride at high temperature that iron was cubic and since γ crystallized differently, that α and β were isomorphous with one another: β was distinguished from α by a 'molecular change' – such a change does in fact occur since the atomic magnets are no longer aligned above A_2†. It was thought by some that β iron was intrinsically hard and that quenching retained this form at room temperature. Carbon was believed to assist the retention of this hard form at low temperature; thus explaining the difference between pure iron and steel. This was essentially the 'allotropist' theory of the hardening of iron. A contrasting theory was that of the 'carbonists' led by Howe (1890) who maintained that a compound called 'hardenite' was formed between iron and carbon at high temperature and that sufficiently rapid cooling enabled the retention of this hard form at low temperature. There were other explanations

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† According to Howe, the great American metallurgist, Osmond called β iron decipuum because you knew of its existence as you knew of certain planets (presumably Neptune was in mind) by the perturbation which it caused.

too, namely that quenching γ iron produces amorphous layers particularly at the boundaries between crystals and that these were very hard. It was believed at the time (by Rosenhain and others) that work hardening was due to the production of amorphous material.

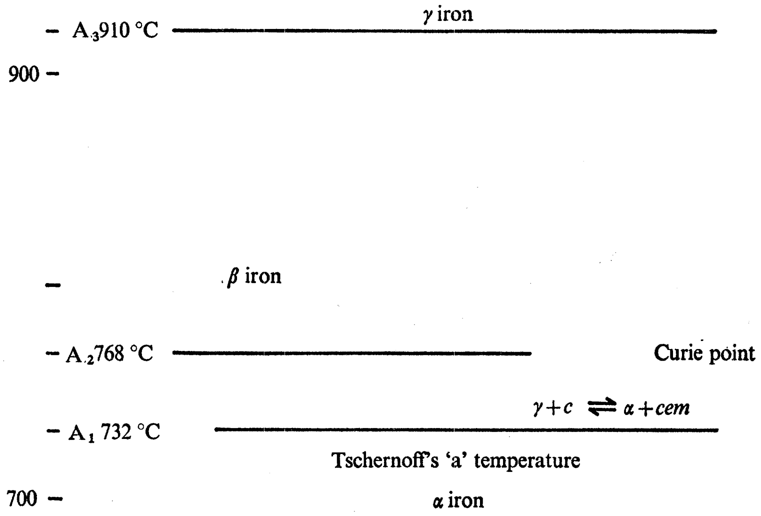

FIGURE 2. The significance of the thermal arrest points in iron and in iron-carbon. A stood for thermal arrest. In very pure iron there is no arrest at 732 °C.

Rosenhain & Humfrey (1909) undertook a series of characteristically ingenious and well conceived experiments on a mild steel. They decided to strain at elevated temperature a suitably shaped specimen with a surface carefully polished for microscopic examination, without allowing the polished surface to be damaged by oxidation. This was achieved by straining in a vacuum (1.33 Pa; 0.01 mmHg). The strain was applied at any desired temperature by releasing a spring which then applied force to the specimen when high temperature was attained. The specimen was subsequently examined at low temperature. Since the temperature varied along the specimen a clever method was devised to measure the temperature at which straining had occurred. Small particles of sodium chloride (melting point 810 °C) and of potassium sulphate (m.p. 1070 °C) were placed upon the back surface of the specimen. Upon recovering the specimen examination showed which particles of salt had melted and which had not. The salts were chosen since the melting points were close to A_2 and to A_3 respectively. Unfortunately, due probably to radiation loss from the specimen the temperature measurement was not exact!

When the specimen was heated without straining a set of interlaced dark lines showing the characteristics of two sets of crystal boundaries were identified at a temperature of 700–800 °C. This was identified with A_2 and so ascribed to the α–β transition. When the specimen was heated to about 1000 °C large crystals were observed which were revealed by a slight amount of oxidation which led to tinting and these were identified as γ iron.

When strained specimens were examined, slip lines characteristic of α iron were found in the cooler parts. Slip appeared to occur more easily at points at which the temperature was higher and more numerous better marked slip lines seen. However, a sudden change apparently occurred at high temperature and along a well defined line running across the specimen no slip appeared to occur. Further along a specimen, i.e. at the hottest parts, slip lines were again seen and twinned crystals, characteristic of γ iron.

Rosenhain & Humfrey also carried out a thermal analysis on their specimens and duly found heat absorption at about 770 °C (770 °C on heating and 763 °C on cooling and at between 900 and 950 °C (941 °C on heating and at 901 °C on cooling) A_2 and A_3 no less, with A_3 much sharper and showing hysteresis, and A_2 spread over a wider temperature range and showing little hysteresis. The results of the straining experiments showed what the allotropists proposed. Rosenhain and Humfrey identified the region showing little slip with β iron. No slip because it was harder than α or γ! In fact the specimen could be fractured and still show, remote from the fracture, a region devoid of slip. So, α iron decreased in strength with increasing temperature and suddenly, when β was formed, it became much harder.

The conclusions to the paper were clearly stated:

'(1) That iron at temperatures up to 1100 °C behaves as a crystalline aggregate and undergoes plastic deformation by a process of slip on the cleavage or gliding planes of its constituent crystals; this may or may not be accompanied by mechanical twinning.

(2) That iron between the ordinary temperature and 1000 °C exists in three distinct modifications possessing widely different mechanical properties, and that the temperature ranges in which the modifications exist are consistent with the view that they are identical with the α-β- and γ-forms of Osmond and Roberts Austen, as indicated by cooling curves.

(3) That β iron although existing at a higher temperature is harder and stronger than α-iron and that the α/β transformation involves a volume change.

(4) That γ iron, as found in approximately pure iron at higher temperatures, possesses the characteristic structure and properties found in the "γ iron" alloy steels.'

The work was elegant and appeared conclusive. It was referred to in Rosenhain's certificate of candidacy to the Royal Society prepared in 1911 which was signed by Ewing, Stead, Heycock, Neville and Sir George Beilby among the metallurgical fraternity – Lord Rayleigh proposed him. The certificate stated that the paper with Humfrey 'gives the final conclusive demonstration of the distinctive properties of the different forms of iron known as α iron, β iron and γ iron.' Rosenhain was elected in 1913 and in a number of short contributions between 1909 and that date expounded the views based on his experiments with Humfrey (Rosenhain 1910*a*, *b*, 1911). He was thus identified as a protagonist for β iron.

In 1913 another paper was published with Humfrey on 'The tenacity, deformation and fracture of soft steel at high temperatures' (Rosenhain & Humfrey 1913). This is a very careful piece of work in which actual stress–strain diagrams were produced for iron up to 1100 °C in a vacuum by the use of a specially constructed apparatus in which the specimen was placed on top of a barometric column of mercury so that this acted as a frictionless stuffing box and the specimen could be strained as in a normal tensile test. The material used was a slightly less mild steel (0.1 % C), heating and cooling curves were again taken and microscopic examination carried out. The curve showing the maximum tensile stress sustained by the specimen, plotted against temperature is shown in figure 3. A small and not very definite break in the curve appears at A_2. A_3 is clear and now γ iron is seen to be harder than α iron. Alas, for hard β. *Hard* β did not exist and Rosenhain partially admitted this 'it seems probable that the microscopic discontinuity observed by the authors in 1909 and ascribed by them to the transition from the α to the β condition, is in reality due to the transition from the β to the γ condition;...as the temperature measurements of the earlier

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work were admittedly of an approximate character there seems no difficulty in reconciling the observations then made with the facts now established. The theoretical conclusions on the other hand may require some modification.'

The interpretation of the results are then conducted in terms of the amorphous cement theory with little reference to the β iron controversy. Rosenhain still thought of β as a separate crystallographic phase and since he knew that the crystal structure of α and β were clearly the same, presumably thought of Osmond's 'molecular change' as distinguishing the two. He recognized that there was no longer evidence for a volume change in the putative β to α transition.

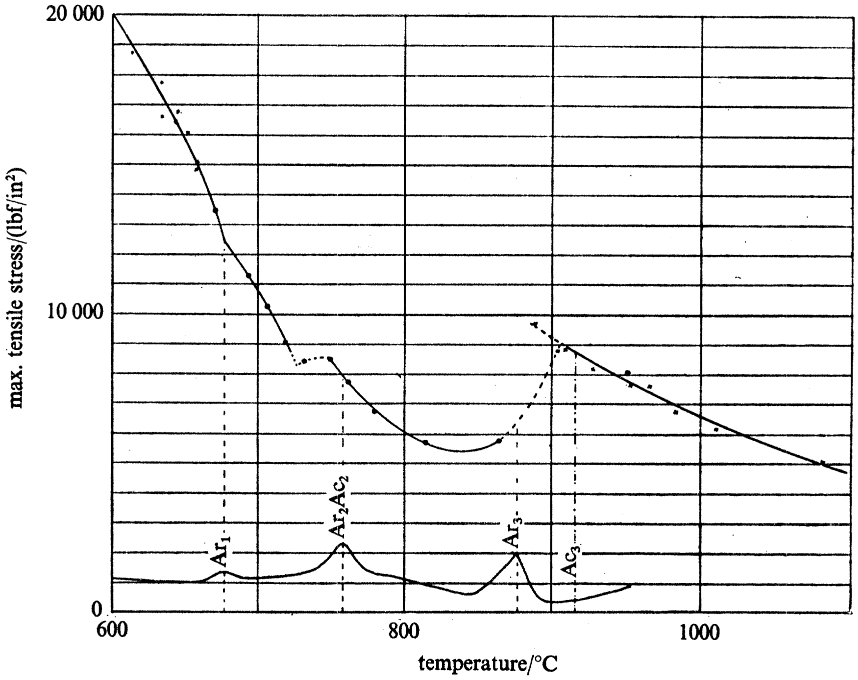

FIGURE 3. Temperature-tenacity curve obtained by Rosenhain & Humfrey (1913) together with their thermal cooling curve.

This paper is in fact a classic in my opinion because, apart from the theoretical views of the authors, it showed experimentally very conclusively that both grain size and strain rate affect the stress–strain curve and that the effects vary with temperature and the practical import of these effects on metal working was clearly recognized. In fact, stating the results in modern form, flow stress σ depends on tensile strain rate $\dot{\epsilon}$ as

$$\dot{\epsilon} \propto \sigma^n$$

where n is an integer, being larger at higher temperatures. Rosenhain & Humfrey found $n = 4.6$ at 836 °C and 6.0 at 948 °C.

The possibility of recrystallization occurring during deformation is also implicit in the results.

The second paper was published in the *Journal of the Iron and Steel Institute* and hence was open to discussion which may not have occurred with the first being published in the *Proceedings of the Royal Society*.†

A furious and vituperative discussion is printed in the *Journal of the Iron and Steel Institute*

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† It was, however, read on 25 November 1909 and if there was discussion we have no record of it.

following Rosenhain & Humfrey's second paper. Their retraction concerning the hardness of β iron is not allowed to pass without comment: J. O. Arnold, F.R.S. reproducing side by side conflicting portions of the two papers with the most scathing comments. Arnold was delighted that β iron was no longer thought hard, though he believed in β iron. What angered him – a convinced carbonist – was that the authors did not explain the fall in hardness on cooling from the γ range below A_3 as due to 'hardenite' precipitating out of γ iron and hence leaving the soft β as able to deform. Carpenter, Rosenhain's predecessor at the N.P.L. and now at Manchester University was equally scathing having first said that 'It was a most difficult thing to admit in public that one was wrong'. Certainly his subsequent remarks would not encourage one in this predicament to do so. Actually he quite properly questioned the authors about the discontinuity in properties at A_2 – see figure 3 which one can see from the points there, and on other graphs in the paper, is not very well established, though clearer on some others than on that reproduced in our figure 3. Rosenhain & Humfrey had been unwise in stating in their conclusions that 'the question whether the β modification of iron plays the principal part in the hardening of steel is still an open one' and Sir Robert Hadfield, F.R.S. quite rightly took them to task for this besides emphasizing that their own results showed no change in crystal structure, heat relief or volume in passing from the α to β phase so that whether β really was different from α was really the point in question. I do not doubt that Rosenhain was being castigated for his own preaching in the intervening years in favour of hard β iron and for the fact, as Hadfield stated, that the earlier results had been quoted over and over again by the authors and many others.

The opening of Rosenhain's reply to Arnold's remarks is often quoted: 'The first part was complimentary which he appreciated, the second part was vituperative, which he was inclined to ignore and the third part was argumentative, and with that he proposed to deal'. In his remarks Rosenhain defended the discontinuity in mechanical properties at A_2 and he clearly still believed in β as a separate phase.

In the same year as this controversy occurred in Britain a very careful experimental analysis of whether or not β iron was a separate phase of iron was carried out at the Bureau of Standards in Washington (Burgess & Crowe 1913). They reviewed the literature in depth, citing evidence for critical ranges of the metal deduced from expansion, thermoelectricity, crystalline structure, mechanical methods, electrical resistance, magnetism, calorimetry and then carried out a very careful thermal analysis on specially prepared iron. Their conclusion was certainly not that β did not exist. They were as near what we consider to be the truth as they could be without investigations by using X-ray diffraction. They stated that 'all the physical properties of iron which have been studied with the single notable exception of crystallographic structure, has shown in the hands of one or more skilled experimenters a distinct discontinuity for the A_2 range as well as for the A_3 range. For several of the phenomena, such as electrical resistance, thermoelectricity, specific heat and magnetism it would appear that the discontinuity is at least as great for A_2 as for A_3, while the thermal effect has of course long been recognised as being much the more pronounced at A_3.' Having carried out their very careful experiments and measured with some precision the effects, Burgess in the final discussion of their paper makes the hypothesis that A_2 will probably be found to be an allotropic point and that when sufficiently exact expansion measurements are made on pure iron a minute but abrupt volume change will be found at A_2. He goes on to say that if experiment proves this not to be the case, then the A_2 transformation must be linked entirely with the passage from the ferromagnetic to the paramagnetic state and hence would not be classed as an allotropic transformation 'and β iron would be really dead'.

In 1921 Westgren's X-ray experiments in Gothenburg killed it (Westgren 1921). Hull (1917) showed, using the Debye–Scherrer method, which he independently invented, that α iron is body-centred cubic and following his method Westgren took X-ray photographs up to 830 °C. There was no change in crystal structure. By examining alloys containing 25 % nickel in which γ iron was stabilized at room temperature it was established that this form has a face-centred cubic Bravais lattice. Hence any difference between α and β was not of an allotropic nature. In the discussion Rosenhain tacitly conceded this and used the term β iron 'for a certain condition of iron which existed'. Only F. C. Thompson continued to resist and said specifically that 'he refused to be converted even now to the view that β iron had no existence' and he said that perhaps there was a discontinuous change in the atomic distance in iron as it passed from the α to the β state. For the purist this point was finally dispatched in 1955 when careful experiments showed no abrupt change in lattice parameter of iron as it passed through the ferromagnetic Curie temperature (Basinski, Hume Rothery & Sutton 1955).

The discussion of Westgren's paper throws to me an interesting sidelight on Rosenhain's intellect. A central theme was where was the carbon in α iron and particularly in martensite which Westgren had just revealed to have (approximately) the crystal structure of α. Westgren had also shown that in 12 % manganese steel containing 1.34 % by mass of carbon, of which he had measured both the lattice parameter and the relative density, that the carbon atoms were most likely arranged interstitially rather than replacing iron at the lattice points of the face-centred cubic space lattice. Sir William Bragg took up this point in discussion and suggested that if this were the case the γ iron would be very highly distorted. When Rosenhain entered the discussion he attempted to explain a large number of facts vividly known to him. Because of his own ideas on amorphous layers between crystals and his recognition of the importance of the polycrystal as the true metallic specimen, he suggested that martensite contained α iron as crystals in a mass of non-crystalline material and since Bragg indicated distortion if carbon was in the lattice then the carbon was most likely to be in the amorphous boundary. The amorphous boundary hardened the material; the α-iron was magnetic; at once an explanation of the high coercive force and high remanence of quenched carbon steels struck him. The width of his vision then led him to suggest that it was unlikely to be the case that carbon was interstitially dissolved in iron. In this, of course, he was wrong. His very quickness of intellect and ability to grasp a number of ideas at once as well as his desire to explain a little too much led him to defend a number of false theoretical conceptions.

From the β iron controversy Rosenhain and Humfrey, I believe, emerge rather well. In the second paper they had to admit that the first was in error and this must indeed have been a blow to Rosenhain knowing how important the paper appeared to others and had been to him. Arnold's public attack really does sound vituperative mainly because Rosenhain did not use Arnold's ideas to explain the hardening of steel. One must recall that this whole controversy occurred before X-ray diffraction could be widely used and the α/β transition as Burgess & Crowe summarize had all the recognized features of a phase change. A phase change in the solid state was automatically assumed to indicate a form of allotrophy. Only a volume change was missing and Rosenhain & Humfrey believed they had established this, and Burgess & Crowe accepted their evidence.

Rosenhain & Humfrey's second paper is a classic for its investigation of the high temperature mechanical properties of iron. Rosenhain's defence of his position in the discussion is dignified. Had all protagonists been sufficiently humble to let nature lead them, they would, of course, have discovered the idea of phase changes of two different types, but this would have required an attitude of patient determination which seldom goes with the ability to inspire others and to get experiments done.

There is a marked difference in the attitude to the controversy revealed by the work at the N.P.L. and at the (National) Bureau of Standards. That at the latter is extremely careful, well planned and seriously executed. The experiments are straightforward. Nothing new or fancy here except the serious and patient treatment. Quintessentially the more common view of the work of a standards laboratory. Careful conclusions admirably hedged and, judged on its own, it did not resolve the controversy. A_2 might still be an allotropic transformation or it might not. The N.P.L. work was quite original in conception, the experiments were quite new. They were ingeniously executed; in the first case not quite carefully enough, but in the second case admirably so. They were promulgated with great vigour and aggression. Men could not be neutral on the issue! After Rosenhain & Humfrey's second paper the issue was practically decided. Nobody in the U.K. believed in β iron any more as an important hardener of steel†. The N.P.L. had settled the question, not in the way Rosenhain expected, but nonetheless the function of a standards laboratory to enable controversy to be resolved had indeed been discharged!

6. AMORPHOUS METAL

According to one of his close collaborators, Hanson, Rosenhain had two principal objectives in his life, '(*a*) the development of his Department at the N.P.L. and (*b*) to prove and get generally accepted his amorphous metal hypothesis'. The second was the idea, introduced in two papers with Ewen (Rosenhain & Ewen 1912, 1913) that the boundary layer between the unit crystals of a polycrystalline metallic mass consisted of metal atoms in a non-crystalline amorphous arrangement, 'chemically identical with the substance of the metal or alloy in question, but in a widely different physical state'.

The theory was ingenious and comprehensive. It rested on the facts that in contrast to most inorganic crystals such as minerals, fracture of metals does not normally occur at grain boundaries. That the boundaries are in fact stronger than the crystals is suggested by the fact that slip bands in one grain do not penetrate into another. Grain boundary matter was, therefore, assumed harder than the crystals at low temperatures. At high temperatures fracture in metals more often occurs at grain boundaries. These facts were explained by assuming that grain boundary matter is stronger than the crystals at low temperature but weakens more rapidly as the temperature is raised, the flow stress depending on strain rate as in a glass, with the behaviour of which Rosenhain had great familiarity. Rosenhain & Ewen also called on Sir George Beilby's experiments on the surface structure of abraded and drawn metals (e.g. Beilby 1903, 1907) observed under the optical microscope. Beilby found flow to occur so easily and the metal to be smeared in such a fashion that he assumed an amorphous

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† There were still some foreign recidivists, e.g.
Howe, H. M. 1916 *Metallography of steel and cast iron*, McGraw Hill.
Sauveur, A. 1935 *The metallography and heat treatment of iron and steel*, Cambridge, Massachusetts.

state. Such heavily worked material also shows differences in electric potential and various thermal properties from annealed material. Without the concepts of sliding friction (for example, Bowden & Tabor 1950, 1964) involving some melting and the idea of large densities of dislocations providing an increased internal energy to the deformed metal, no modern explanation is possible.

Rosenhain & Ewen also drew on the conception of 'crystal molecules' a sort of nucleus as being necessary for the proper crystallization of metals and reasoned ingeniously that between contiguous growing crystals in a solidifying mass, there was not room for a proper crystallization of metal in the interstices and so the material solidified in an amorphous state. The concept of a 'lineage structure' or of a large crystal being composed of a set of blocks with some irregularity between them, a type of analogue of the 'crystal molecule', persisted for many years. It was still being reviewed twenty years after the (theoretical) discovery of dislocations (Hirsch 1956). Rosenhain correctly reasoned that the metastable amorphous cement would have a higher vapour pressure than the normal crystal and hence that the grain boundary would evaporate more easily. He found larger mass losses on heating fine grained specimens than on heating coarsely crystalline ones. He also found that metals heated slowly while sustaining a small mass, broke at the grain boundaries at a temperature a few degrees centigrade below the melting point and he discovered grain boundary grooving when metals are heated at high temperatures. From the discussion of these papers by Desch & Carpenter and others the concept of more rapid diffusion occurring at grain boundaries than that which occurred within the grains, emerged.

When one reads the papers with attention one finds, despite the (to us) prolixity of style, and the discursiveness allowed by editors in those days, that the authors almost cast a spell on the reader. They placed together well remembered and relevant experimental results of themselves and others and almost command assent by presenting an idea capable of rationalizing many observations by a consistent hypothesis.

It is now possible to show that the hypothesis is wrong in detail and in generality but to do so requires knowledge of the short range of the forces between metallic atoms, the concept of the dislocation, the use of X-rays, electron microscopic and Auger electron techniques and, most importantly, experiments on very pure specimens in high vacua. Amorphous metal in any quantity may only be produced metastably by very rapid quenching (Duwez 1967). Many of the effects reported by Rosenhain, his co-workers and others, both protagonists and antagonists of this theory, were due to segregation of impurity atoms to the inter-crystalline regions, a subject much clarified and illuminated by later work at the N.P.L. (McLean 1957; Seah & Hondros 1973).

Rosenhain & Ewen's particular experiment of showing that metals melted at the grain boundaries before doing so in the bulk, occupied metallurgists for a number of years. Rosenhain's nephew repeated their type of experiment in the later 1930s (Chalmers 1940) and claimed to find grain boundary melting. That any real effect must be due to non-obtainment of equilibrium was demonstrated by Shewmon (1957).

Rosenhain thrived on controversy and promulgated his theory with some objectivity as well as much debating skill, in the literature of the learned societies; and it runs throughout the various editions of his book *Physical metallurgy* from the first edition in 1914 to the last in 1934. The theory was wrong in scientific detail but it was of great utility. It enabled the metallurgist to reason and to recognize that at high temperatures grain boundaries are fragile, that heat treatment involving cold or hot work coupled with annealing can lead to benefit in some instances and to catastrophies such as 'hot shortness' in others. The existence of 'burning' and 'overheating' would be avoided. Advances in technology and practice do not always require exact theory. This must always be striven for it is true, but a 'hand waving' argument which calls salient facts to attention, if readily grasped in apparently simple terms, can be of great practical utility.

7. Physical metallurgy and the role of the N.P.L.

From three of his publications in particular we can obtain an idea of Rosenhain's views of the nature and relevance of his subject and of the institution he served for most of his working life. These are his account in the middle of the Great War 'The National Physical Laboratory – its work and aims' (Rosenhain 1916*b*); his presidential address to the Institute of Metals (Rosenhain 1928) and his guest lecture to the German Society for Metallurgy given in Dusseldorf on 7 September 1929 (Rosenhain 1930).

He saw physical metallurgy as the science relating the properties and particularly the mechanical properties of metals, hardness, toughness, strength, elongation and anelastic properties to the chemical constitution and to the microstructure. In this physics and metallurgy were partners. In 1930, as now, there was a feeling among physical scientists that physics is somehow superior to metallurgy because of its more exact and (hence necessarily) abstruse nature. But the physicist must, to obtain his accuracy of prediction, work with pure materials so well characterized and defined that their utility to the engineer and their charm for the metallurgist has been 'researched' out of them. Rosenhain saw the two sciences as a partnership 'Der breite Strom des Wissens und der Forschung wird aber von wielen Quellen gespeist, und unter diesen darf auch die Metallforschung eine anerkannte Stellung beanspruchen'. He saw the metallurgist as leading the physicist so that the latter worked on problems of importance to the engineer – not measuring very accurately the ephemeral 'constants' of a piece of metal of ill-defined microstructure but towards understanding in sufficient detail all the complexity attendant upon casting, forging and joining metal, the necessarily impure metal used in practice.

Thus when addressing physical scientists he had to defend the severely practical so that too much manpower and brainpower was not wasted on the too detailed explanation of trivia, yet when addressing the practical men of industry as President of the Institute of Metals he had to defend the scientific, detailed and analytic approach. Members of the Institute of Metals in 1928 objected to the large number of scientific papers appearing and wanted just to know how to make their alloys better. Rosenhain did this for them. First he explained the 'abstruse' simple stress–strain curve and the even more recondite binary phase diagram. Not things the practical man at the time wished to know about. He goes on to explain age hardening and the basis of the heat treatment of aluminium alloys, so necessary for the burgeoning aircraft industry of the time. He showed how the science gave guide lines to new alloys of commercial advantage and pointed out that had the results of the Ninth Report to the Alloys Research Committee been picked up quickly and prosecuted at the time it was published (1910), Duralumin would have been the commercial property right of an English firm and not of a German. The subject was and is a complex one. The principle of age hardening as deduced by the scientist is not patentable because deducible from prior principles, whereas of course specific age-hardening alloy systems are. Science freely published can aid all, countryman and competitor alike. It is just so today.

Further the 'practical' men were told that defensive research is necessary in a practical commercial situation, when Rosenhain asked 'Is it quite certain that our age-old products 'ordinary' brass and bronze, will always be able to maintain their position?'

Rosenhain's view of the work of the N.P.L. and of its aims can be most easily told by quoting *in extenso* a number of passages from his lecture to the West of Scotland Iron and Steel Institute in 1916 (Rosenhain 1916*b*). He saw the N.P.L. as serving industry by doing applied science, by which he meant enough of the pure variety to enable practical problems to be attacked. The pure science must be of the best not static but 'it must be living growing science' and mindful, not impatient of the difficulties of application.

> 'When, however, we come to apply the results obtained by the worker in pure science to practical ends an entirely different set of conditions is met with. For practical purposes we have to deal with natural phenomena as we find them and have to produce the desired result in an approximate but efficient manner. We have, in fact, to face the extreme complexity of ordinary conditions, and we soon find that every effort to lessen this complexity in order to reduce a process to conditions more nearly resembling those which have been studied in the research laboratory introduces practical complications which tend to increase cost.'

> 'We thus see that on one side the worker in applied science finds the field of his activities not only in the most intimate contact with, but actually overlapping that of the worker in pure science. This contact and overlapping, however, bears good fruit on both sides, since the experience of practical problems frequently serves to open up whole regions of new phenomena to the purely scientific investigator, while on the other hand the stimulus of the most advanced scientific thought prevents the worker in applied science from losing touch with the true spirit of his science and its latest development.'

The difficulty, of course, is to maintain the balance of the two!

He was not unmindful of the real complexity of technical manufacturing industry:

> 'but one further factor requires consideration. I refer to the question – admittedly difficult and delicate – of labour conditions. The growth of the demands of labour is a phenomenon which no observer can miss, and in any rational consideration of the future prospects and policy of industry that factor cannot be left out of consideration. This is not the place to enter into any discussion of the thorny problems with which this subject bristles, and I wish to refer to one aspect only. If increasing demands of labour are to be met, and at the same time reasonable profits are to be maintained, there can be only one sound solution – and that must lie in increasing the efficiency of production. To the labour advocate one might say that if the earning power of the workman is increased by the better, more efficient, more scientific, utilisation of his labour, then can his return for his labour be correspondingly increased, while to the employer the same consideration may be urged in regard to his capital as represented by his plant, stocks, etc. It is not perhaps too much to say that the real hope of peace and prosperity for both lies in the possibilities of the application of science and scientific methods to their common problem.'

Prominent Trades Union leaders would today agree with part of the diagnosis but the touching faith of the last sentence is denied to us. The current view is of the paramount importance of sufficient investment capital.

What a central government research institution can do and which a university cannot do was a question to be answered then as now. Rosenhain thought as follows:

> ‘This ‘something more’, which is additional and supplementary to the work of Universities and Colleges, and in no sense intended to rival or supersede their research activities, is an institution for research in which the educational bias is entirely absent, and whose activities can therefore be directed in an entirely untrammelled manner to the solution of problems suggested by public utility, scientific importance, or industrial demand. Such an institution would possess a staff not burdened by educational duties and selected without reference to teaching ability. Such a staff should constitute a more or less permanent body of investigators and their highly-trained assistants, thus affording opportunities for long-continued researches which can scarcely be undertaken by a Professor working with successive generations of young students. The equipment of such an institution could be designed on lines free from educational considerations; apparatus and experimental plant could be set up, involving, if necessary, the permanent employment of skilled labour as well as scientific supervision. Further, if only one or a very few such institutions were set up in the country, equipment could be carried out on a scale of cost which could not be reproduced at any considerable number of Colleges. Further, appliances could be used which it would be unsafe to place in the hands or even within the reach of students.’

He went on and described the ‘remit’ as we might call it of the N.P.L., serving as a national base of the measurement system and carrying out the needs of government and assisting industry.

> ‘Further, such an establishment would occupy a unique position as a central national institution which would naturally undertake certain duties of a scientific or technical nature required by the Government. Such matters as the national standards and the standardization of measures and measuring instruments of all kinds would be undertaken there under national authority. Then, in another direction, where certain industries require national aid on account of their vital nature as “key” industries, the national central institution would provide the Government with the means of affording the scientific assistance demanded. The same provision of facilities of the best kind under national auspices would prove invaluable in connection with military, naval, and aeronautical matters. Here centralization would be essential in the interests of that degree of secrecy which is essential. Then again there are such matters as arbitration on points of scientific dispute which in modern times frequently arise out of industrial questions; the standardization of testing and in some cases even routine testing for the Government are all activities which would rightly occupy such an institution.’

> ‘At the same time the institution would also be in hourly touch with industry – helping, advising, assisting, testing – taking up special investigations proposed by industry and dealing with them in the laboratory and with experimental plant. The institution would also be in closest touch with the Government, giving – together with the eminent specialists to be found in the Colleges and Universities – that expert advice which is now so frequently needed and undertaking such tests and investigations as the Government required.’

Although serving governmental needs, Rosenhain did not see the N.P.L. as only drawing its funds from the government.

> 'With regard to the general organisation of the Laboratory, it must be pointed out that the National Physical Laboratory is not actually a "Government" institution in the sense of constituting or forming part of a Government Department. The management of the Laboratory rests with a Committee appointed in accordance with certain Treasury regulations by the Royal Society, and the staff of the Laboratory are employees of the Royal Society and not civil servants. The Treasury makes to the Royal Society an annual grant of £7000 towards the cost of the Laboratory, and in return exacts certain conditions in regard to the work to be undertaken, and, among other things, in regard to the maximum salaries to be paid to members of the staff. I need hardly point out that this annual Government grant is entirely inadequate for the maintenance of the Laboratory; the last balance-sheet issued before the war reached a sum just over £40,000, of which something over £30,000 had been earned by the Laboratory in the form of fees. It is only right to point out, however, that a large part of these fees were derived from Government sources, in the shape of charges for work carried out for or on behalf of various Government Departments, such as the Admiralty, the War Office, the India Office, etc. Private firms and individuals, however, also contributed to a large extent to the fees received for work done, and this aspect of the work of the Laboratory is capable of very great and useful extension once the nature of its facilities becomes better known among those who could avail themselves of its services. Beyond this, technical institutions, like the Institution of Mechanical Engineers, Institution of Civil Engineers, Institute of Naval Architects, and others have contributed to the Laboratory budget, in some cases by simple donations, but more frequently by grants in aid of some definite scheme of research.'

Within two years of this being written the National Physical Laboratory became a civil service institution when it was absorbed into the Department of Scientific and Industrial Research in 1918. The method of funding of the work then changed radically, but gradually; some of the difficulties are described in articles by Hutchinson (1969, 1970).

Rosenhain (1916*b*) goes on to describe the work in his Department current at the time. There were four sections. He regarded physical metallurgy as the most important of these, dealing with methods of metallography, the elucidation of phase diagrams and the effect of thermal and mechanical treatments on the properties of metals. Another dealt with chemical analysis, carrying out a lot of work for the India Office, and as it was wartime assisting manufacturers to deal with requirements formerly filled by German goods, e.g. chemical glass ware, porcelain and filter paper. There was also a section dealing with aircraft fabrics for balloons and airships. There was a section on optical glass, the origin of this again to help deal with the shortage of optical quality glass which had before the war all come from Germany.

8. Origins of present materials work at the N.P.L.

Rosenhain left the Laboratory in 1931. He was succeeded as Superintendent by C. H. Desch who stayed until the Second World War. During the whole of this period at the N.P.L. there was also a flourishing Engineering Department. This, with the Observatory and Physics Departments was one of the three original departments at the Laboratory. Metallurgy under

Rosenhain flourished in concert with the Engineering Department, which for three-fifths of its total period of life, namely from 1901–30, was led by (Sir) Thomas Stanton. The present work on materials at Teddington has origins not just in Rosenhain's work but also in that of the Engineering Department and the work of the Chemical Research Laboratory, which became the National Chemical Laboratory in 1958. Most of the work in the Metallurgy Department under Rosenhain was concentrated upon the elucidation of alloy structure, the accurate determination of phase diagrams and attempts to relate the metallurgical variables, such as composition and heat treatment, to the properties of materials; there was, in addition, a good deal of work on development of instrumentation. This was supported throughout by the work on the properties of materials and on the long term testing of materials carried out in the Engineering Department. There, under Gough and others, work on fatigue and creep of materials under simulated service conditions yielded results which were easily known to the metallurgists and were commented upon by them, as well as stimulating the metallurgists own ideas.

TABLE 1. REPORT ON THE METALLURGY DEPARTMENT – 1927

General research

Pure metals; Refractories; Spectrographic analysis; Physical structure of metals and X-ray study; Magnetic phenomena; Metals and alloys under strain and plastic deformation; Surface tension of liquid metals

Government research

Light alloys; Spring steels; Minor metals; Chains and hooks; Alloys for high temperature; Steel castings; Gas Cylinders; Fabrics research

Special investigations

Alloys of iron; Dental alloys; Impurities in copper; Special brasses; die casting

Maintenance of standards, etc.

Metallurgical analysis; Temperature standards; Thermostats; Viscometry; Experimental methods; Furnaces; Test-work, etc.

REPORT ON THE ENGINEERING DEPARTMENT – 1927

General research

Friction of fluids; Hardness tests research; Gear research; Properties of concrete

Government research

For the Lubrication Research Committee
For the Engineering Coordinating Research Board and the Aeronautical Research Committee
    Fluid friction and heat transmission; Big-end bearings; Springs research; Materials at high temperatures; Explosions in closed vessels; Rust prevention; Steel strip for aircraft; Fatigue of single metal crystals; Light alloys; Failure of chains; Sizes of nuts
For the Building Research Board
    Wind pressure; Vibration of structures
For the Gas Cylinders Research Committee
Roads Research

Special investigations

Tower of London; Various investigations; Test-work; St Paul's Cathedral

A list of the topics which were being studied in the Metallurgy Department in 1927 when Rosenhain was 52 and his Department in the main stream of activity is given in table 1. It is interesting to note that the researches are classified under general research, which meant research suggested by members of the Metallurgy Department themselves, Government research in which work was carried out for various of the Advisory Committees working for the Department of Scientific and Industrial Research, special investigations which were usually paid for by various outside bodies and maintenance of standards which was work in the main stream of the Laboratory and usually meant testing to ascertain whether or not specific standards of performance were obtained. Steels, light alloys, brasses, coppers, were of course there and also some work on fabrics research, which involved the attack of sunlight on cotton. Dental alloys were examined for the Dental Investigation Committee of the D.S.I.R. by means of funds provided by the Dental Board of the United Kingdom. This work, like so much of the others, resulted in alloys which were tested in industry, in this particular case at Guy's Hospital. I have also reproduced in table 1 the headings of research in the Engineering Department. This is broken down into similar though not exactly equivalent headings and the Annual Report for that year states that more of the work of the Engineering Department was carried out specifically for various Governmental bodies. Road research, of course, figures which later left the N.P.L. and was carried out at the Road Research Laboratory; founded in 1930 under the Ministry of Transport and transferred to the D.S.I.R in 1933. There was a particularly close connection between the work of the Engineering Department and that of the Metallurgy Department. Looking at some of the headings in Engineering and comparing them with Metallurgy one sees how similar they are, e.g. springs research and spring steels, materials at high temperature and alloys for high temperatures, rust prevention and steel strip for aircraft, fatigue of metal *single crystals*, light alloys, failure of chain. This was work which clearly could have been carried out either in the Metallurgy Department or the Engineering Department; the precise location depending on how one drew the line or wrote the label on the door. That both Departments benefited becomes very clear from reading the Annual Reports.

An interesting feature which later disappeared from the N.P.L. Annual Report is the statement for whom the work was carried out. This, indeed, would have delighted Lord Rothschild in 1971 and perhaps current attempts to state customer and contractor principles are just a means of going back to what was a rather clearly thought out means of stating within a Department of N.P.L. who the customers for the work might be.

It was in the Engineering Division† that work on plastics at the Teddington site began in 1939 when in collaboration with the Chemical Research Laboratory, (N.P.L. Annual Reports 1940–48) attempts were made to utilize the high strength to density ratio and stiffness to density ratio of engineering plastics and also to investigate the possibilities of large mouldings for aircraft parts. Of course the stiffness of plastics without fillers is not high and this led naturally to a collaborative programme between Engineering Division and the Chemical Research Laboratory on plastics reinforced with fillers – paper was the most useful and important at the time – this was the first work in Britain on the principles of fibre reinforcement. Before the Second World War, the work of the Engineering Division was on two main subjects – the determination and their use in design, of the properties of engineering materials and the development of the principles of fluid dynamics and their application to gas/liquid flow and to lubrication. During the Second World War a good deal of work went on the high temperature properties of metals and it became necessary to build a large creep laboratory to house 50

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† Departments at the N.P.L. became named Divisions during the second war, in order to avoid confusion with the *Department* of Scientific and Industrial Research.

creep testing machines. Early in 1940 work began on an extensive study of creep and fatigue properties of high temperature alloys for use in gas turbine blades, in conjunction with the Mond Nickel Company, from whom Dr Allen, the Superintendent of Metallurgy Division, who succeeded Sir Charles Sykes, was also to come. Tests were made on the creep properties of Nicrome alloys 18 % nickel, 20 % chromium, called at that time Nimonic 75 and well known at the N.P.L. to have good creep properties since it had previously been investigated in 1924–25 from German aircraft captured in World War I. When American alloys became available during 1940–43 for investigation, notably vitallium and hastoloid, these were also investigated. During the second war a good deal of work was carried out on the stress distribution and stability of aircraft structures and attention was given to the development of sandwich structures (that is composites), in which two very thin sheets of material were stabilized by filling with a core of very light material. As no material for the core was available at the time, and experimental confirmation of theoretical results was wanted, the collaboration of the Chemical Research Laboratory was sought. A core material was developed for experimental purposes from *foamed* calcium alginate. The successful development of the Mosquito aircraft called in question the shortage of balsa wood and the calcium alginate foam was selected as a possible substitute.

Stress corrosion was one of the subjects in which there was much cooperation between engineering and metallurgy. This arose because the hydrogen needed for barrage balloons was carried in cylinders made from nickel-chromium-molybdenum steel, which had been originally developed for cold cast traction purposes. Trouble was encountered through failures when they were first used for hydrogen; the cylinders burst. The other material-type problems investigated in Engineering Division were the design and failure of helical springs and the development of resistance wire strain gauges. Work in the Metallurgy Division at the N.P.L. during the second war complemented and extended that of Engineering. For instance, the perforation of steel plate by bomb fragments carried out in the Metallurgy Division could have been carried out either in Metallurgy or in Engineering. Another metallurgical problem investigated was the use of Duralumin scrap. Some problems in connection with the loss of ductility in cast alloys made from Duralumin scrap were investigated and solved. Porosity of castings, problems relating to armour piercing shot, electrical tests of hardness of armour piercing caps as well as naval shells are problems of such an engineering flavour, that then, as now, it would be much more likely to find such problems investigated outside the Metallurgy Division. Nonetheless there were metallurgical problems in worrying about how it is that 14 and 15 inch (35–38 cm) shells were retarded by 12 inch (30 cm) carbon steel plate and it was this work carried out under the direction of the then Dr Sykes, Superintendent of the Division, which led Sykes to become not only Superintendent of the Metallurgy Division but also of the Terminal Ballistic Section of the Armament Research Department of the Ministry of Supply. Welding problems were investigated in the Metallurgy Division, cutting tools, gun barrel breeches, high temperature alloys, moisture proof packagings and magnesium alloys. An experimental transmission-type electron microscope was set up for the first time in England in the Laboratory in 1940 – this was destined for the Imperial College of Science and Technology and made by Metropolitan Vickers. Besides this, work on the microstructural changes accompanying deformation of material at high temperature and the cracking of boiler plates and of the wear of refractories was carried out in Metallurgy Division. This recitation of the topics covered illustrates again the close relationship between Metallurgy and Engineering and, indeed, the close relationship between Engineering and the Chemical Research Laboratory in some of the aspects of the C.R.L.'s work.

After the second war the Engineering Division carried out work for the Mechanical Engineering Research Board of the Department of Scientific and Industrial Research and it was planned that this work should be controlled by the Mechanical Engineering Research Board rather than directly by the N.P.L. Advisory Board. In the Annual Report for 1947 it was stated 'A large part of the research programme of the Engineering Division is now carried out for the newly formed Mechanical Engineering Research Board of the Department. The smaller amount of routine work undertaken has enabled fundamental research to take an increasingly prominent place in the programme' (N.P.L. Annual Report 1947). It was decided to transfer the bulk of Engineering Division to a new laboratory. By 1950 progress on the Mechanical Engineering Research Laboratory at East Kilbride had been made and during 1951 the main transfer of equipment and staff was taking place.

The work on materials at high temperatures so intimately interwoven with that of Metallurgy Division was to remain at N.P.L. until 1953. By the end of 1951 the major change had been the transfer of most of the work of the Engineering Division to East Kilbride. That remaining was distributed amongst the Divisions of the N.P.L., e.g. the deadweight standards of load and strain gauges went to the Metrology Division and work on physics of the solid state to Physics Division. This included measurements of the single crystal elastic constants by sonic methods, which today would be regarded as an integral part of materials science. Some work on high temperature materials stayed at the N.P.L. The creep laboratory still exists and there is a programme in the Division of Materials Applications on high temperature materials.

The separation of the work on engineering properties of materials by the move of the Engineering Division to East Kilbride was regretted in the Metallurgy Division at the time and it is clear from Annual Reports that a sense of frustration filled Dr Allen's mind. There was some attempt at continuing cooperation over the years since 1950 but I believe that both organizations have suffered; the materials work from the absence of engineering practitioners on the site and the Engineering Research Laboratory has been unable to draw on first rate scientific work on microstructure.

The other strand in the organization of materials science and engineering at Teddington developed from the Chemical Research Laboratory, set up in 1925, which became the National Chemical Laboratory in 1958 and was amalgamated with the National Physical Laboratory on 1 April 1965. The Chemical Research Laboratory, C.R.L., was set up in 1924 but the first report did not appear until 1934 and contains a summary of the work before that date. The main Divisions were Corrosion of Metals, High Pressure Research, Tar Research, Chemotherapy, Water Pollution, Microbiology, some work on dental materials and on organic and inorganic chemistry. At the period of the amalgamation, the National Chemical Laboratory, which it had been called since 1958, contained work on chemical thermodynamics including thermodynamic measurements and some attendant molecular spectrometry, which enabled one to calculate some thermodynamic properties from observed vibrations of molecules; inorganic chemistry, which contained work on mineral processing besides chemistry of beryllium and electrochemical methods of metal production; a divison of organic chemistry and a division of surface chemistry. The work on mineral processing was transferred to the Warren Spring Laboratory and the remainder of the work on inorganic

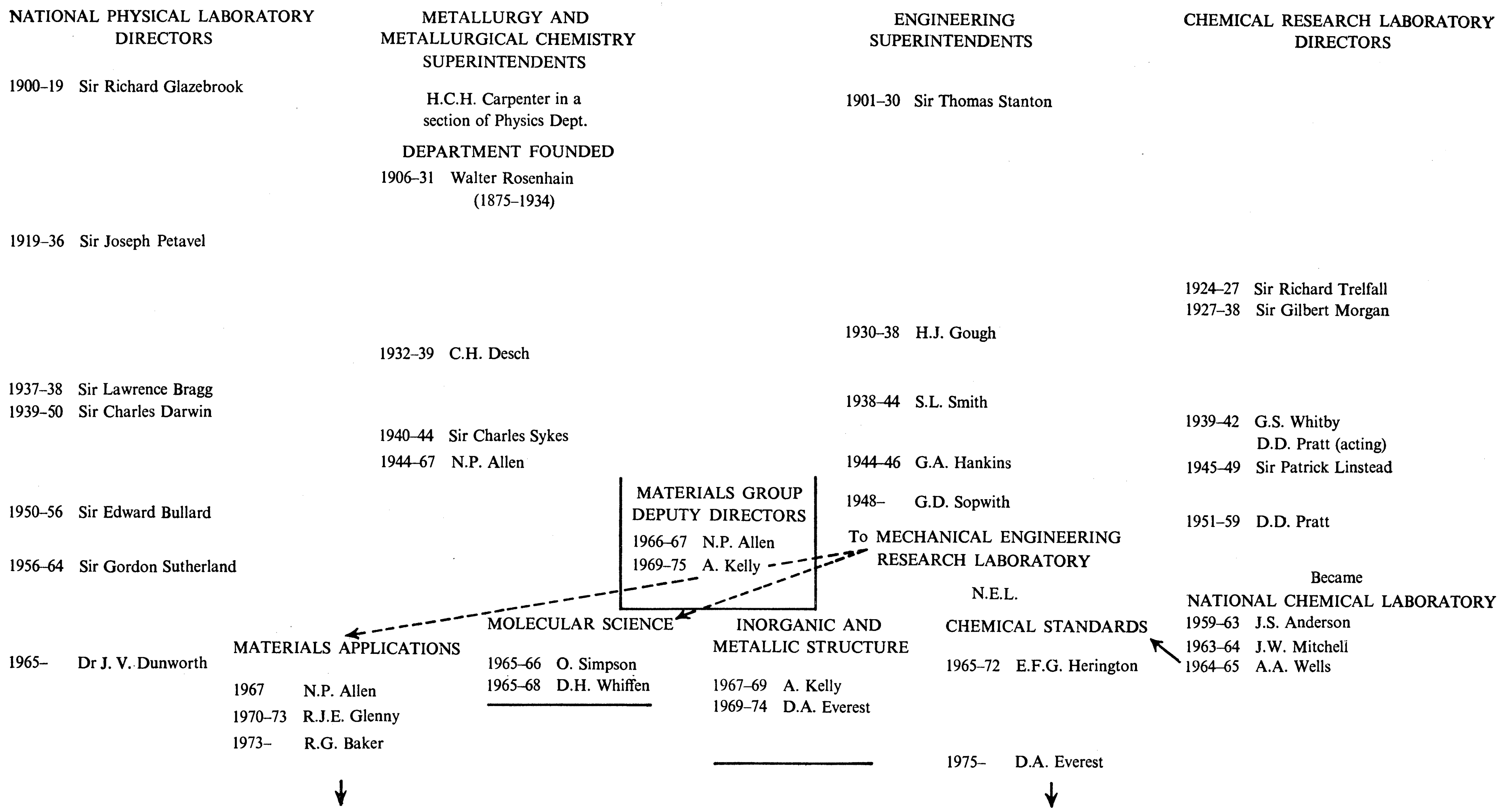

Figure 4. Directors of the N.P.L. and of the C.R.L. (N.C.L.) with Superintendents of Divisions concerned with properties of materials.

chemistry came into the Division of Inorganic and Metallic Structure, while that on organic chemistry went principally to a Division of Molecular Science and later was brought together in the Division of Materials Applications with the work on metals. The division of Surface Chemistry had been partly a particular interest of the Director, Dr J. S. Anderson, F.R.S., namely work on field emission and field ion microscopy and also a good deal of work on corrosion mechanisms. This naturally, of course, went into the present Division of Materials Applications. The chemical physics section of the National Chemical Laboratory, which was carrying out accurate physico-chemical measurements on gases and fluids became the Chemical Standards Division of the National Physical Laboratory. The work on organic and inorganic materials was put together with that of the Basic Physics Division of the N.P.L. in a Division called Molecular Science. This included a good deal of work on polymers carried out in the Basic Physics Division of the N.P.L. which had been started under J. A. Pople, F.R.S. when this Division was set up in 1958. All the work on materials was carried out for a number of years under a Deputy Director in charge of a Materials Group. This period has now come to an end and the major strands of the work are as follows. These will be briefly sketched because I think they have indications for us of the importance of the present Conference, but before doing so, it is worth just glancing at figure 4 which attempts to summarize the organizational structures and the personalities involved as head of these over the period from the inception of the N.P.L. and the N.C.L. up until the present time.

The present work concerned with Materials at the N.P.L. is contained in two Divisions. The first, the Division of Chemical Standards, is concerned with the maintenance of secondary standards and the transfer of these to British industry. The topics covered include general chemical reference materials, e.g. surface area standards, metal in oil standards, and standards for characterizing molecular mass distribution in polymers. Reference materials for measuring thermal properties, e.g. benzoic acid for calibrating bomb calorimeters and standards for differential thermal analysis. A data bank of thermal properties including those of metallurgical interest is maintained. A capability to calculate phase diagrams for multicomponent systems, four or more is being developed. All data banks need to be developed in terms of the relevant soft ware for computer manipulation. Materials are sometimes processed at high pressure and so high pressure standards are maintained. Surfaces are characterized in terms of surface segregation and propensity of elements to segregate to grain boundaries. Chemical analysis capability is maintained in this Division. The development of many of Rosenhain's interests is clear.

The second Division presently named Division of Materials Applications contains the majority of the work aimed to support the writing of specifications of materials for engineering use. This involves both the characterization of materials and of engineering needs appropriate to specific applications as well as the provision and development of some standard methods of testing; some standard data is also acquired. The Division carries out work on creep of chromium–molybdenum–vanadium steels for power generation aimed at recognizing microstructural features to predict creep lives, and work on structural steels and hard metals, also designed to support the best specification of chemical composition and heat treatment. Corrosion prevention and oxidation prevention figure largely and a Corrosion Advisory Service, a part of a National system for advice on corrosion, is maintained. A large section of the Division deals with elucidation of properties which will support the writing of engineering specifications for polymers by measurement of elastic moduli and of anelastic effects. Finally, a section deals with composites and concentrates on understanding the modes of fracture of these and especially of those based upon cements.

The relation between the two Divisions is a close one and the boundaries of the work are not exact. In Chemical Standards some work is carried out on silicate materials, with a view to improving the engineering specification of these.

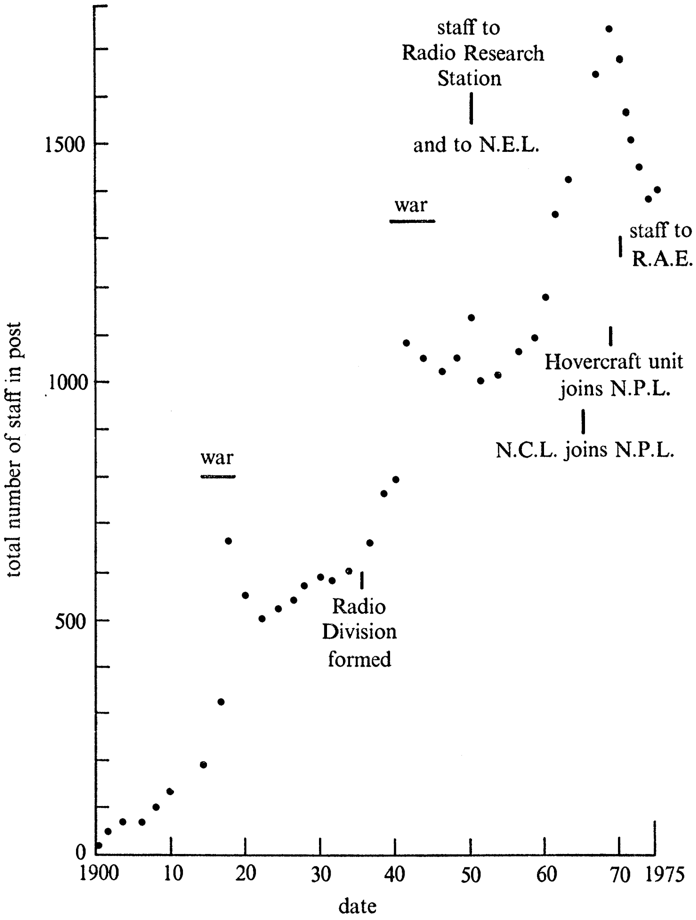

FIGURE 5

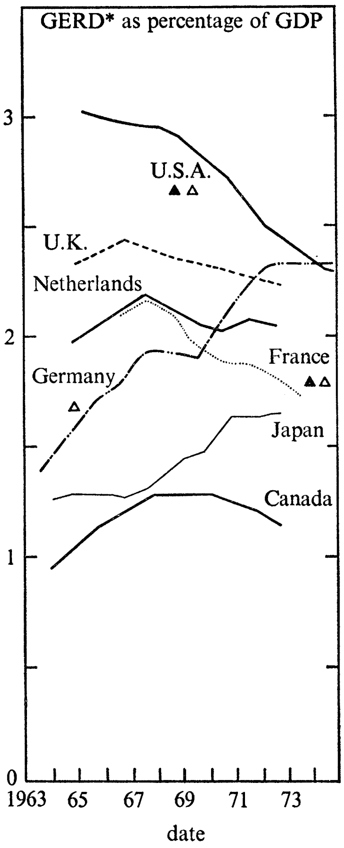

FIGURE 6

FIGURE 5. Total number of staff in post at the National Physical Laboratory from 1900 to the present.

FIGURE 6. Gross expenditure on research and development in a number of industrialized countries as a percentage of gross domestic product from 1963–75. (From *Financial Times* 24 June 1975). *, gross expenditure on research and development (GERD); △, these data include world sciences and/or humanities; ▲, national total approximately GERD.

The Division of Materials Applications will have increasingly close links with the recently formed Division of Mechanical and Optical Metrology. This will recreate, in a somewhat different form, and with specific reference to standards, the valuable interaction which was a feature of the relationship between the Metallurgy and Engineering Divisions.

All of this work is carried out at the behest of the Chief Scientist of the Department of Industry, who finances the work through the Requirements Boards, which contain advisers drawn from industry and the universities.

9. The present scene

When Rosenhain joined the N.P.L. he had a staff of four and when he left that staff was 70. This rise in numbers was paralleled in the growth of the Laboratory. Figure 5 shows the numbers from 1900 until the present day. At the time of the transfer of the N.C.L., the N.P.L. absorbed approximately 196 non-industrial Civil Servants and 43 industrials. This produced a large increase in the size of the N.P.L. which can be seen on the graph of total numbers in figure 5. Figure 5 shows that there have been two large increases in the number of staff at the N.P.L.: one following the first World War in 1920, the second following the outbreak of war in 1940 and there was then a steady rise in numbers during the early 1960s which culminated with the transfer of the staff from N.C.L., so that the total number at the N.P.L. reached about 1750 souls. The largest size of the Laboratory was obtained in 1970 and thereafter there has been a decline in numbers, as there has in many other scientific research establishments.

Figure 6 shows that the growth of spend on research and development in a number of countries – the most advanced technologically – is over and the spend now decreases as a proportion of gross domestic product.

The total amount of science and technology has grown so enormously since Rosenhain's time that the problem of relating the position of the physical metallurgist or of the materials scientist to that of the engineer is almost unrecognizably complex. This may be a truism but I believe it to be worth stating.

Firstly there is a change in the range of materials available. I list for some materials of interest to engineers the date at which they were introduced in tables 2 and 3. I have taken the date as that at which pilot plant scale production started.

Many of these were in their infancy when Rosenhain worked of course, and many have been developed since.

Concurrently with the introduction of new materials, there have been large changes in engineering technology. Some of those listed in *Encyclopaedia Britannica* as occurring between about 1930 and 1970 are:

gas turbine engine	1930–45
catalytic cracking of petroleum	1935–41
atomic power	1942
plastics	1909–54
synthetic fibres	1938
very high rise building	1900–32
automobile mass production	1913–23
television	1920–45
liquid fuelled rockets	1926–67
Man on the Moon	1969
Wankel engine	1968
computers	1944–
computer controlled chemical manufacture	1950s
heavy earthmoving equipment	1945
plastic design and steel framed buildings	1947–55
air cushion vehicles	1958
Xerography	1937–46

TABLE 2. DATE OF DEVELOPMENT OF SOME MATERIALS

iron blast furnace	1735	uranium	1943
crucible steel	1740	tungsten	1909
Bessemer steel	1855	tungsten carbide	1926
oxygen steel	1952	synthetic diamond	1958
copper	1556	fibres	
nickel	1840	nylon	1938
Nimonic alloys	1940–	polyester	1947
tin	B.C.	glass fibre	1938
lead	B.C.	(glass wool)	1930
magnesium	1886	carbon fibre	1968
Aluminium	1886	glass reinforced plastic	1940
titanium	1944		

TABLE 3. DATE OF DEVELOPMENT OF SOME PLASTICS

thermosetting		thermoplastics	
phenolic	1909	low density polyethylene	1933
urea base	1926	high density polyethylene	1954
alkyd	1910–20	polypropylene	1959
polyester-styrenated types	1942	vinyl chloride	1930
epoxy	1939	polyvinyl acetate	1917
		styrene polymers	1935
		cellulose plastics	1900–24

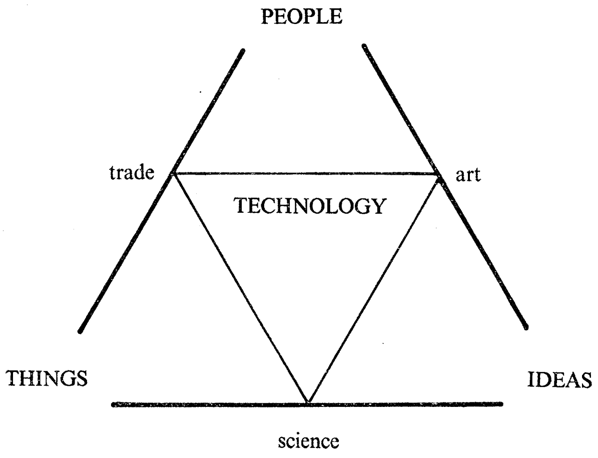

FIGURE 7. The definition of a technologist.

It is worth recalling these changes because they illustrate that no man can encompass now the source of materials serving all of these technologies. We are going to sectorize the discussion and concentrate upon physical metallurgy and what its relation has been, and hopefully may be, to the progress of engineering. Because of the complexity one must inevitably deal with teams engaged in this endeavour; teams of scientists, teams of engineers. Once a team is formed this human grouping develops an inner logic of its own and a good deal of its time is spent in ensuring the proper communication between the component parts of the individual team; i.e. between the individual members of the team. If this is necessary even more time must be spent in ensuring a proper interface for communication between the various teams.

We tend to idealize the team as a single man, say the physical metallurgist or the engineer. But it is important to remember that these are just idealizations.

The *metallurgist* has as his concern to supply the engineer with the alloys he needs, with information about their properties, about how to avoid problems when using them. In order to do this he has to know the engineer's situation, to think with the engineer. To predict the properties he must understand them and this is usually done by scientifically studying the relation between properties and microstructure so as to provide a basis for explanation of the effects of processing, heat treatment, etc.

The *engineer* must design machines, structures and components to achieve a required function and to select and specify the material standards of workmanship and overall quality that must be attained. So he must be aware of properties of different materials relevant to him and of the potential problems which may arise. Overall a value judgement will be exercised by society on the enterprise and this is simply expressed in terms of cost. The alloys must be supplied and the structure made at the price that the customer will pay. Whether or not the structure is supplied at an acceptable cost is the job of all *technologists*.

The job of the *scientist* is easy to define – it may not be easy to do. I have found it helpful to think of the job of the technologist in terms of a diagram which illustrates his relationship to science and to people. This idea has been introduced in a forthcoming book (Davies, Sheahan & Banfield 1976) which I have enjoyed discussing with the authors, and is illustrated in figure 7. The scientist explains things because they are there. He develops a language of explanation by the systematic confrontation of nature with observation and with logic. In terms of the diagram he relates things and ideas but has not very much to do with people. The artist relates man with his idea of himself without the use of language. He relates people and ideas and has little to do with things. The trader brings things to people in an acceptable form and has little to do with ideas. The successful technologist is part scientist, part trader and part artist. The physical metallurgist relates science and technology on these definitions and this is what Rosenhain attempted to do. This Conference will examine further how this business of relating science and technology has been fruitful and will be more so.

I am very grateful to a number of people who have provided me with information; in particular Mrs N. Kirsner, Professor N. Greenwood, Professor R. W. Cahn, Dr E. D. Hondros and to my secretary Mrs P. Lee for so carefully editing the manuscript. I am very grateful to the staff of the library at the N.P.L., in particular Miss H. K. Carter and Miss J. H. Simmons, who have helped me diligently in my research for some rather obscure sources. I am grateful to Drs R. G. Baker, J. V. Dunworth and D. E. Miles for helpful comments on the typescript.

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APPENDIX 1. (*Chronology*)

date	event	age
1875	Born 24 August Berlin, only son and youngest child of Moritz and Frederike Rosenhain	0
1880	Family went to Australia, lived in Geelong	5
1888	Queen's College Melbourne founded	
1890	Visited Europe with parents	15
1891	Entered Queen's College, University of Melbourne; read Physics and Engineering	16
1894	Visited Europe with parents seeking cure for his lame sister Clara	
1896	Final Honours in Engineering	21
1897	Won 1852 Exhibition to go to Cambridge – St Johns.	22
	Worked with Sir Alfred Ewing	
1900	Ewing delivered Bakerian Lecture based on work with Rosenhain	25
	June – Took up post of scientific adviser to Chance Bros., Smethwick	
	Associated with Stephen Chalmers (died 1918) Clara's husband and Bruce Chalmers' father	
1900–6	Studied refractory materials and glass	25–31
1901	December – Married Louise Monash, daughter of Louis Monash and sister of General Sir John Monash	
	Joined Iron and Steel Institute	
1906–31	Appointed Superintendent, Department of Metallurgy and Metallurgical Chemistry, National Physical Laboratory	
1906	Andrew Carnegie Medal, Iron and Steel Institute	
1908	Institute of Metals founded – Rosenhain original member	
	Published *Glass manufacture* 1st edition, Constable	
1909	Awarded D.Sc. Melbourne	
	Commemorative Diploma, Brussels University	
1911	Commemorative Diploma, Turin University	
	Sir Julius Wernher donates £10000 for new Metallurgy Laboratory at the N.P.L.	
1911–18	Member of Council, Institute of Metals	
1912	Visited New York for 6th Congress International Association for Testing Materials	
1913	Commemorative Diploma, Ghent University	38
	Elected Fellow of Royal Society	
1914–18	Great War during which half of staff of Metallurgy Division called up	
1914	Published *An introduction to the study of physical metallurgy* 1st edition, Constable	39

	Visited Australia with British Association and returned in December	
1917	Ph.D. established at University of Oxford	
	Department of Scientific and Industrial Research established	
1919	*Glass manufacture* – 2nd edition published	44
	Introduction to Physical Metallurgy – 2nd edition published	
1921	R38 crashed – two staff of the N.P.L. killed	46
1923	Gave third Sorby Lecture. 'Present and future problems of metallurgy' (Rosenhain 1923*b*)	
	Lecture tour of United States	
	May Lecture Institute of Metals, 'The inner structure of alloys' (Rosenhain 1923*c*)	
1926	Autumn Lecture to Institute of Metals 'Modern metallurgy and ancient industries' Liege, Belgium (Rosenhain 1926)	
	Vice President International Association for Testing Materials	
1927	Joined Institution of Professional Civil Servants and helped establish a branch at the N.P.L.	
1928–30	President, Institute of Metals	53–55
1928	Presidential Address Institute of Metals	
1929	Fellow of the Institute of Metals	
1930	Bessemer Medal, Iron and Steel Institute	
1931	Scientific Civil Service established	
	June – Resigned from the N.P.L.; became private consultant	
1932	Honorary member Deutsche Gesellschaft für Metallkunde	
1934	Thomas Turner Gold Medal, University of Birmingham	58
	17 March died at Kingston Hill, Surrey	
	Introduction to physical metallurgy, 3rd edition (with J. L. Haughton) published	