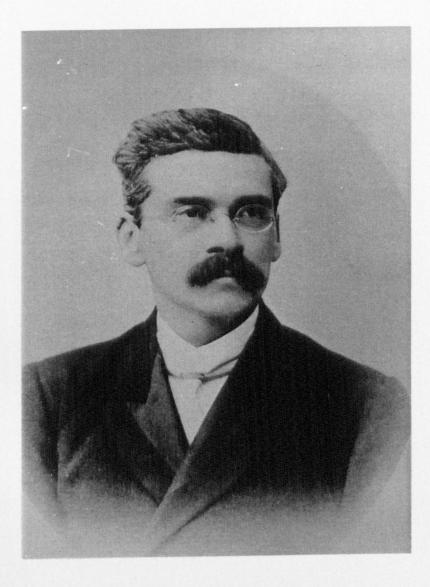# AN EVALUATION OF FERDINAND HURTER'S
CONTRIBUTION TO THE DEVELOPMENT OF
THE NINETEENTH CENTURY ALKALI INDUSTRY

CYRIL ARTHUR TOWNSEND

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FERDINAND HURTER

1844 - 1898

ABSTRACT

Ferdinand Hurter was an industrial chemist and one of the earliest chemical engineers. He was born in Switzerland in 1844 and, after obtaining his Doctorate in chemistry at Heidelberg University, he came to England in 1867. He joined Gaskell Deacon & Co, a major Leblanc alkali manufacturer in Widnes, the leading alkali town in Britain, as Works Chemist. During the next thirty years, the most successful period in the history of the Leblanc alkali industry, he devoted his considerable talents, both practical and theoretical, to developing and improving a wide range of processes. He was the author of a large number of publications and patents, and has been described as the first person to put the British chemical industry on a truly scientific basis.

When the British Leblanc alkali manufacturers amalgamated in 1890 to form the United Alkali Company, Hurter was appointed to the prestigious position of Chief Chemist, in which post he served until his early death in 1898. He was a leading figure in a number of learned societies, and also took an interest in technical education.

The accusation by certain historians that, in the 1890's, Hurter advised the United Alkali Company not to adopt the electrolytic process for manufacturing alkali, and thus caused the decline and eventual demise of the Leblanc industry, is fully investigated. It is concluded that the allegation was substantially unfounded.

The purpose of this thesis is to carry out an in-depth study of Hurter's professional career, so as to evaluate his influence on the development and prosperity of the British alkali industry. He made many major contributions to chemical manufacturing, and was one of the most outstanding and talented personalities in the chemical industry during the nineteenth century.

ACKNOWLEDGEMENTS

ABBREVIATIONS

The following abbreviations are used in the Text and References:

BP: British Patent

CRO: The Dickinson Archive, Cheshire Record Office, Chester

Dingler: Dingler's Polytechnisches Journal

ICI: Imperial Chemical Industries

JACS: Journal of the American Chemical Society

JIEC: Journal of Industrial & Engineering Chemistry

JSCI: Journal of the Society of Chemical Industry

LPS: Liverpool Physical Society

RPS: The Driffield Bequest, Royal Photographic Society, Bath

SCI: The Society of Chemical Industry

UAC: United Alkali Company

USP: United States Patent

CONTENTS

CHAPTER 1

INTRODUCTION & OBJECTIVES

Ferdinand Hurter was regarded by many of his contemporaries as one of the leading figures in the nineteenth century inorganic chemicals industry in Britain. It was claimed that he was a pioneer in the application of scientific methods to chemical manufacturing processes. Many of his published papers, which cover a wide range of subjects, are classic documents in the history of chemical manufacturing. Hurter was born in Switzerland in 1844. After receiving his education in the Gymnasium in Schaffhausen, he entered the textiles dyeing industry as an apprentice, aged sixteen. When he was nineteen he felt the need for a better scientific education, being dissatisfied with the rule-of-thumb procedures used in the industry, and went to study chemistry at Zurich Confederated Polytechnic School. In 1865 he went on to Heidelberg to work under Bunsen and Kirchoff and in 1866, just a year after arriving there, he was awarded the degrees of Doctor of Philosophy *summa cum laude* and Master of Liberal Arts. Hardie [1.1] was unable to discover the subject of Hurter's research for his doctorate, but the point is explained in a letter from the Heidelberg University archivist dated 1978:

> ... I can advise you that Ferdinand Hurter did not submit any dissertation at the Faculty of Philosophy of the University of Heidelberg. Preparation of a dissertation in the nineteenth century was not a condition of graduation [1.2].

On qualifying, Hurter was offered a professorship in chemistry, but he refused this and came to England in 1867, obtaining employment as Works Chemist with Gaskell Deacon & Co, Widnes, a major Leblanc chemical manufacturer. He remained with the firm until he joined the United Alkali Co (UAC) in 1890.

When Hurter arrived in England in 1867, the British Leblanc alkali industry was approaching the peak of its commercial success. It was producing a wide range of products, the most important of which were sodium carbonate and bleaching powder. From the beginnings of the Leblanc industry in the 1780s up to that time, however, there had been only limited technological innovation, the principal advances being increases in the sizes of production plants and the total

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quantities of chemicals produced. The opportunity existed to make major technological improvements to the processes and so improve the profitability of the industry.

No attempt has been made to write a detailed history of the alkali industry; the information which is provided is to assist in evaluating Hurter's work. Detailed accounts of those chemical processes with which he was involved are given in the contemporary literature; these are referenced throughout the text, or included in the Bibliography. Brief descriptions are provided where these are essential to the understanding of the matter under discussion.

Hurter's work cannot be evaluated in isolation from the changing state of the British alkali industry during the period which he spent in it. It will be shown that he made a large number of contributions which were of great value to both the technological development and the economic optimisation of a wide range of processes. His career began at the start of this period of greatest prosperity for the Leblanc alkali industry and ended when the fortunes of UAC (which incorporated most of the British Leblanc companies) were beginning to decline - a decline which was to continue until the amalgamation of the company into ICI in 1926.

Hurter's most prolific period of invention and technological research was during his employment by Gaskell Deacon & Co. His work, covering a wide range of subjects, often involved the application of basic theoretical principles of physics and chemistry both to laboratory scale experimentation and to the design and optimisation of full-scale chemical production plant.

The second part of his career, in the prestigious post of Chief Chemist of UAC, the largest chemical manufacturer in Britain, shows a different aspect of his character. He established UAC's Central Laboratory, one of the first research departments in the country devoted to chemical manufacturing. The appointment was a major watershed in Hurter's life, revealing managerial skills which he had hitherto neither exercised nor demonstrated. It is regrettable that he was in poor health for much of his time with UAC and, after a strenuous business visit to the USA, Hurter died suddenly in 1898, at the early age of fifty four, cutting short a promising and still-developing career.

The decision to carry out the research described in this thesis was influenced by comments made by the historian, Hardie:

> Hurter was one of the very first to apply rigorous physical chemical methods to an industrial process [1.3].

> The scientific career of Dr Hurter reflected more intimately than that of any other man the development and decline of the Leblanc system [1.4].

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These are confirmed by the remarks of Hurter's professional associate for many years, George Lunge. Lunge was accorded the honour of delivering the first Hurter Memorial Lecture in the year after Hurter's death. He reviewed Hurter's career in the British chemical industry and concluded:

> Hurter will always be looked upon as one of the pioneers in the application of mathematics [i.e physico-chemical principles] to problems of technological chemistry, and therefore in the establishment of a true scientific base for manufacturing chemistry [1.5].

Hurter was widely acknowledged as one of the first industrial chemists to apply the theoretical principles of both physics, e.g. fluid flow and heat transmission, and chemistry, e.g. thermodynamics and reaction kinetics, to chemical manufacturing processes. The principal purpose of his career was to improve these processes, so as to maximise the profits of the Leblanc alkali industry and prolong its life. The matters which he particularly addressed were:

Increasing the conversion efficiency of a chemical process, producing a greater yield of product per unit of raw materials. Improving its thermal efficiency, thus reducing fuel costs.

Increasing the purity, and hence the selling price, of products.

Reducing labour costs by introducing sophisticated process techniques.

Using better materials for constructing chemical plant, thus reducing maintenance costs.

Minimising the capital cost of new plant and equipment.

Hurter paid a great deal of attention to the economic viability of chemical processes. In the early days of the Leblanc industry this matter was not assigned a high priority, since there was little competition from other manufacturers. In the latter part of the nineteenth century, however, it became essential to maximise the profitability of a process, so as to be able to face up to competition. Hurter recognised the need for this early in his career and developed an expertise in the subject which would normally have been outside the remit of a laboratory-based chemist.

There has been only one major criticism of Hurter's work: that he played a significant part in the decline and eventual failure of the British Leblanc alkali industry by advising the Board of UAC not to

adopt Castner's electrolytic alkali process. This thesis examines the evidence for and against this allegation in depth and it is concluded that it was substantially without foundation.

It has also been suggested that, because he spent the whole of his working life in the Leblanc industry and was obviously thoroughly committed to it, Hurter was blinkered in his attitude to the ammonia-soda and electrolytic alkali processes. This will be shown not to be the case; he was very knowledgeable and open-minded about them. He invented a version of the ammonia-soda process and also a highly successful electrolytic process for manufacturing potassium chlorate.

In addition to his work in industrial chemistry, Hurter was enthusiastic about education and training in technology: embodying the communication of information in the broadest sense. His interests included the membership of learned societies, the publication of many learned papers and patents, contributions to books, the training of process managers and operatives, and lecturing.

In the course of evaluating Hurter's contributions to the British chemical industry, information on his personality has emerged from his personal letters, contributions to scientific meetings and comments made about him by contemporaries and by latter-day historians. It is felt that this should be included, since it has a bearing on his professional career. However, because it is not directly connected with his technical and scientific contributions to the chemical industry, it appears as Appendix X.

In 1871 Hurter became friendly with Vero Charles Driffield, Gaskell Deacon's Works Engineer, who had formerly been a professional photographer. Although he had left the profession, Driffield retained an interest in photography and tried to persuade Hurter to take up the hobby. Hurter felt that photography was not sufficiently scientific to interest him and he set out to apply physico-chemical principles (particularly reaction kinetics) to it. His publications on photographic science were numerous and of high quality; his research on the subject is still highly regarded today. He became as famous for his work on photography as he was for that on industrial chemistry. Many of his photographic experiments were carried out in a special darkroom which was constructed at UAC's Central Laboratory, although he does not appear to have suggested to the company that they might commercially exploit any of his photographic inventions. His photographic research has been fully studied and documented (see the Photographic Bibliography). It is not directly relevant to this thesis and is not dealt with in detail, although brief references to it are unavoidable. However, it is felt that some mention of this aspect of his life should be included to demonstrate the breadth of his abilities and interests.

In view of Hurter's undoubtedly outstanding abilities and reputation, it is surprising that a full account of his life in the British chemical industry has never been written; only brief references have been made to it by historians. It is believed that this thesis is the first document to produce a comprehensive and

objective evaluation of Hurter's professional career. It is based on his many published papers, patent specifications, reports and private papers, as well as comments on his professional abilities by his contemporaries and by latter-day historians.

Much of the thesis consists of a detailed account and in-depth analysis of the vast amount of scientific material which Hurter published during his career. The technical merit of this work and its value to the industry is assessed and evaluated. His interests were so numerous and wide ranging - from the complex reaction kinetics of the lead chamber sulphuric acid process to popular science lectures for the general public - that it has not been easy to ensure that everything he did has been included. It is believed that very little has been omitted, even if only a brief mention of some items has been possible.

Hurter's major achievements, such as his research into the manufacture of chlorine and sulphuric acid, his studies of gas-liquid absorption and electrolytic processes, and his work in establishing the UAC Central Laboratory, have been allocated chapters of their own. Other contributions to the development of the British alkali industry are described in Chapter 8, although some of these might properly have been awarded longer accounts in their own right.

The objectives of this thesis may now be now formally stated:

To appraise Hurter's professional abilities as an industrial chemist and to evaluate his contributions to the development of the British chemical industry, in particular the Leblanc alkali industry. His major scientific and technical achievements will be examined.

To study Hurter's career in the context of the technological and economic development of the British alkali industry during the latter part of the nineteenth century.

To consider how his work contributed to the prosperity of the Leblanc industry during its period of growth and, in the subsequent period of its decline, was instrumental in prolonging its life.

To investigate the allegation that Hurter's advice to the Board of UAC not to adopt the electrolytic alkali process was the cause of the demise of the Leblanc industry in Britain.

To review his abilities and interests in education and training.

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CHAPTER 2

BACKGROUND

The purpose of this chapter is to give an overview of the development of the British alkali industry during the nineteenth century, so as to provide a background to the study of Hurter's work.

The earliest of the three synthetic alkali processes (Leblanc, ammonia-soda and electrolytic) was patented by Leblanc in 1791 [2.1]. The Leblanc industry came into existence at the end of the eighteenth century to satisfy the requirements of the new industries of the Industrial Revolution for large quantities of alkali at an economic price. By 1820, alkali manufactured by the Leblanc process had virtually displaced that made from naturally occurring substances, but it was neither as pure nor as cheap as it might have been had a strongly competitive product existed [2.2].

During the greater part of the nineteenth century, the alkali industry was, to all intents and purposes, *the* British chemical industry. Despite this, up until the 1860s, only limited technological improvements were made to its processes; the principal advances were increases in the size of process plants and the overall scale of production. The period of greatest development and commercial success of the Leblanc industry was, arguably, from about 1860 to 1880. After that it gradually declined, to be replaced by the ammonia-soda and, later, the electrolytic alkali processes.

Until 1890 the industry consisted of a large number of independent companies, usually family concerns or small partnerships; there was minimal commercial co-operation between them. By 1890, however, so great were the problems of competition from other sources (mainly ammonia-soda), that UAC was formed as a defensive measure; virtually all the British Leblanc manufacturers were absorbed into the new organisation.

During the nineteenth century Leblanc's basic alkali process developed into the Leblanc System of chemical manufacturing processes. These included: the production of sodium carbonate (soda), sodium hydroxide (caustic soda), sodium bicarbonate, bleaching powder, chlorine, sodium and potassium perchlorates and sulphuric acid. The processes employed are well-described in contemporary text-books by Kingzett [2.3] and Lunge [2.4]; a flow diagram of the Leblanc System is shown in Appendix I and that of the sulphuric acid process in Appendix II.

The Leblanc process was not without its problems. Emissions of hydrogen chloride, sulphur dioxide and oxides of nitrogen caused atmospheric pollution, and sulphur-containing "alkali waste"

caused land pollution. The loss of sulphur, chlorine and oxides of nitrogen affected profitability. In time it became possible to recover these, converting the hydrogen chloride to chlorine; by the 1870s the latter had become one of the most important products of the industry. Despite these improvements, though, it was always recognised that an alkali process which did not possess the problems of the Leblanc process would be preferred.

In 1861 the Solvay brothers patented their version of the ammonia-soda process which, after overcoming some technical and financial problems, soon began to displace the Leblanc process. The cheaper and purer soda made by the Solvay process was its main attraction, but its chief disadvantage was that chlorine could not be easily manufactured from its calcium chloride waste. The Leblanc industry was able to survive for a number of years principally because of the commercial value of the chlorine (sold as bleaching powder) which it produced.

The competitive situation between the Leblanc and ammonia-soda processes changed substantially in the 1890s, following the invention of electrolytic alkali processes. These converted salt directly into sodium hydroxide and chlorine, thereby overcoming the disadvantages of both the Leblanc and ammonia-soda processes. Chlorine made by electrolysis was purer and cheaper than that made by the Leblanc industry, and caustic soda was a more profitable product than sodium carbonate. The electrolytic process never entirely replaced its predecessors, however, since sodium carbonate continued to be required for certain specialised applications. Additionally, there was an economic advantage in running the electrolytic process in tandem with ammonia-soda, so as to ensure a proper chlor-alkali balance.

During the 1880s, the period when the ammonia-soda process was becoming established and successful, the Leblanc alkali industry was suffering severe set-backs: a general trade depression; cheaper and purer ammonia-soda; foreign competition; restrictive legislation; and transport tariffs.

The competition between the Leblanc and ammonia-soda processes is demonstrated by comparing their production costs between 1872, when Brunner, Mond first produced ammonia-soda, and 1894 (Appendix III). In 1872, Brunner, Mond's costs were slightly lower than those of the Leblanc manufacturers [2.5], but the situation was soon to alter substantially in favour of ammonia-soda. Between 1880 and 1891 there was a steady increase in its market share of alkali, although the total output of soda from the two processes remained substantially constant (Appendix IV).

This competition resulted, by 1892, in a fall in the production costs of alkali made by both the Leblanc and ammonia-soda processes, but for different reasons. In the case of ammonia-soda, this was to be anticipated because of improvements to the process as it became established. To counter this, the Leblanc manufacturers were forced to make improvements to their processes, for which there had been little incentive prior to the introduction of ammonia-soda. By 1894, these improvements had halved the

production cost of soda manufactured by the Leblanc industry, so that ammonia-soda was in the lead by only a small margin.

Because of the fierce competition from ammonia-soda, much of the process plant in the Leblanc industry would probably have been redundant by the 1890s, had not it not been for the production of chlorine. Hydrochloric acid was the cheapest source of chlorine for making both bleaching powder and potassium chlorate. Only the Leblanc industry could make bleaching powder and it became one of its principal products. Demand for it rose steadily until 1880, then remained constant until 1890. Its commercial position was strengthened by the fact that a potential process for the electrochemical bleaching of paper had not proved successful.

One disadvantage was that the total production capacity of the bleaching powder plants began to exceed the demand for the product. This was due largely to improvements which had been made to them, many of which resulted from the research which Hurter had carried out (see Chapter 4). Individual manufacturers were forced to compete with each other, with the result that bleaching powder was sold at uneconomic prices (Appendix V). The same problem was experienced with soda.

Many of the Leblanc manufacturers attempted to meet the challenge from ammonia-soda by diversifying their products and by increased efficiency, but it soon became obvious that this strategy alone would not be sufficient. If the industry were to survive, concerted action was needed, probably in the form of a merger along the lines of the Salt Union [2.6].

From the beginnings of the Industrial Revolution there had been various forms of co-operation, for commercial protection, in many industries [2.7]. From as early as 1838 a number of informal agreements had been created between British alkali manufacturers. These were later followed by formal alliances, e.g. the Alkali Manufacturers' Association, the Bleaching Powder Association and the Widnes Traders Association.

In the late 1880s Sir Edward Sullivan, a major Leblanc manufacturer, used the Bleaching Powder Association as his forum to advocate formal amalgamation of the Leblanc companies, but failed to attract very much support. By 1889, its members realised that the time had come for the industry to be put on a sounder and more rational footing. Their companies needed to be amalgamated into a single undertaking, thus accomplishing economies of scale and countering the threat from ammonia-soda.

Of the manufacturers approached, some were enthusiastic, others hesitated but, by 1890, the time for action had come. A "strictly private" circular on the capitalization of the new company was sent to all potential vendors of individual firms [2.8]. A committee of experts from within the industry inspected and provided valuations of all the factories which might become part of UAC [2.9]. Sufficient capital was raised for the new company to be formed and it was incorporated on

1 November 1890, in the face of some opposition from the press, a national financial crisis and hostility from customers who feared that a monopoly was being created.

The formation of UAC is described in the many histories of the company [2.10] to [2.15]. A private publication by UAC describes how it viewed its position in both the British and world chemical markets [2.16]. Lindert and Trace provide a review of the British alkali industry which examines the complex economics of the chlor-alkali balance, before and after the formation of UAC [2.17]. Discussions in this thesis of the history of UAC are based on the above sources and on others which are individually referenced. They are intentionally brief, their purpose being to provide a background against which Hurter's contributions to the British chemical industry may be evaluated.

Forty firms comprised UAC, contributing forty eight factories (see Appendix VI). In addition to manufacturing the traditional products of the Leblanc system, the constituent companies were involved in many other activities. A wide range of "non-Leblanc" chemicals was being produced because many of the individual firms had, even before amalgamation, begun to diversify their manufacturing interests. In addition to chemical works, UAC acquired: railway track and rolling stock; a fleet of ships; brick and pipe works; stone and limestone quarries; sulphur mines; employees' housing; and freehold land [2.18].

Hurter's employer, Gaskell Deacon & Co, was one of the largest companies to become part of UAC. In 1890, production of its principal product, soda ash, was 15% of UAC's total production and it was the second largest producer of saltcake, soda ash and bleaching powder in Britain [2.19]. It is not surprising, therefore, that the directors of Gaskell Deacon had considerable influence in the newly-formed company. Holbrook Gaskell I was elected Honorary Vice-President (later President); his sons, Holbrook Gaskell II and James Gaskell, both became directors. Eustace Carey, Works Manager and partner at Gaskell Deacon, became Company Secretary and Hurter was appointed Chief Chemist.

UAC was more effective than the earlier trade associations had been in strengthening the Leblanc industry. Its size enabled it to compete with the ammonia-soda industry, as well as with foreign manufacturers. Virtually all the soda made in Britain by the Leblanc process was produced by UAC. In the early 1890s the company made great improvements to its profitability by reducing production costs, but it has never received full credit for this achievement. Although UAC was unable directly to overcome the threat from ammonia-soda, a commercial agreement was reached with Brunner, Mond & Co, the principal producer of ammonia-soda in Britain, and UAC made a modest start on manufacturing ammonia-soda itself.

In 1890 the position regarding the manufacture of alkali and chlorine was clear: soda ash was made by the ammonia-soda process, and caustic soda and chlorine by the Leblanc system. The possibility of a successful electrolytic alkali process being introduced was recognised, but it was felt that this was some years off and did not pose an immediate threat to the two chemical processes. This

prediction proved to be over-optimistic since, within a year or two, a number of promising electrolytic processes began to make their appearance.

Despite the technological and commercial efforts which went into forming UAC and ensuring a profitable future for the company, it suffered grave set-backs from which it never fully recovered. Its decision not to adopt the electrolytic alkali process in the 1890s undoubtedly contributed greatly to its decline, but this was not its only problem.

From its earliest days UAC was never in a strong position financially, for a number of reasons. These included: competition from the ammonia-soda and, later, the electrolytic alkali processes; competition from overseas manufacturers; the general trade recession during the 1890s; the rising cost of fuel; and the protectionist policy of the USA. Also, when UAC was formed, far too generous a price had been paid for some of the less profitable component companies with the result that, in subsequent years, there was never really sufficient capital available to improve and expand its operations.

A programme was put in place to rationalise the company's production pattern and this resulted in the closure of a number of inefficient production units: in some cases entire factories. Progress was slow, however, due to understandable opposition from their former owners, who were now directors of UAC. Some of the problems with these individuals were described by Sir Frederick Norman, a leading figure in the Leblanc industry:

> The formation of the U.A.Co. soon realised the fears that I had forecasted viz:- That you could not clamp a body of rivals together and then get them to sink their idiosyncrasies. Full soon under the pretence of policy lurked the disposition to find places for pals and pander to old parochialisms [2.20].

The formation of UAC lost the Leblanc manufacturers some of their former customers. One of the most important of these was William Lever, a major soap manufacturer. Fearing that the price of alkali would rise due to a monopoly, he erected his own soda ash and caustic soda plants and, for a time, there was a serious risk that he might also compete with UAC on the open market [2.21].

An editorial in the Chemical Trade Journal describes, at first hand, the position in the alkali industry in the early 1890s [2.22]. In some financial circles it was felt that, by 1894, the Leblanc industry had only a few months of life remaining, but the editor did not agree that the situation was as serious as that. The British producers of alkali, whether it was made by the Leblanc or ammonia-soda processes, were in a bad way in the world markets anyway, which was why UAC's new ammonia-soda works had proved disappointing commercially. UAC and Brunner, Mond had together produced more ammonia-soda than was required, reducing the market price "to a starvation rate". Also, because ammonia-soda had displaced soda made by the Leblanc process, salt-cake had become an unwanted low-value product and UAC was forced to react to the situation by reducing the amount which was produced. This action was, fortunately, facilitated by the fact that the quantity of saltcake which was

required eventually to manufacture bleaching powder had already been reduced to a third by the introduction of the highly efficient Deacon process. Even so, there was still little profit to be made in alkali manufacture and there was a constant battle to maintain an economic balance between the products of the Leblanc and ammonia-soda industries. A further set-back to UAC at that time was the substantial reduction in the demand for bleaching powder (Appendix V), mainly from the paper and textile industries [2.23].

Drastic measures were urgently needed if UAC were to survive and, hopefully, prosper. Plans to rationalise its manufacturing operations and to diversify into new product areas had been made soon after the company came into existence, but they were not implemented as speedily or as effectively as the situation demanded.

Diversification had not been in the minds of UAC's Board when the formation of the company was planned, although it was clear that some degree of rationalisation of the many and diverse manufacturing units would be required. At that time, the perceived reason for the amalgamation was the protection of the commercial interests of the firms which comprised UAC.

It was not intended that the Leblanc process be abandoned, although it was felt that some ammonia-soda might be manufactured. This point was referred to in the prospectus for the new company, since an ammonia-soda works - Mathieson's of Widnes - had been acquired at the time of formation [2.24]. One of the first and most important diversification developments was to expand the production of ammonia-soda. A large new ammonia-soda works commenced operations at Fleetwood in 1893 (Chapter 6) and from then on the company began to reduce its production of soda by the Leblanc process [2.25]. The minutes of the Board meeting of 12 December 1895 record that:

> It was resolved that the policy of the company is to reduce the manufacture of Leblanc soda ash in favour of ammonia-soda and that the ammonia-soda committee be instructed to report upon the cost of a new unit at Fleetwood [2.26].

During the 1890s and the early twentieth century, UAC developed a wide range of new products, making full use of the research facilities of Hurter's Central Laboratory (Chapter 9). Sadly, though, the company never really realised the potential which it possessed when it was formed. Had its foundation been more soundly based financially, and the efforts made during its early years to ensure its prosperity been more effective, it could have become one of the world's great chemical companies. Apart from a boost to its fortunes during World War I, UAC's progress during the early twentieth century was unremarkable, and it was this period which saw the decline and virtual demise of the Leblanc process. The company ended its life in 1926, when it became part of Imperial Chemical

Industries. It should perhaps be credited, though, with the fact that it had remained in business for thirty-six years without falling into receivership, or being taken over.

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CHAPTER 3

CHLORINE

The Deacon chlorine process was the first major project of Hurter's career and one of the most important. In carrying out this work, he moved away from the traditional role of a laboratory-based chemist into the fields of pilot plant and production scale development and chemical engineering design. This early experience probably stimulated his interest in the application of scientific research to the development and improvement of chemical processes, which was to become the main purpose of his career in the alkali industry.

Chlorine, whose main use was for making bleaching powder, had been manufactured from the early days of the Leblanc system. When Hurter entered the industry in 1867 it was growing in importance and, by the 1880s, was beginning to replace soda as the industry's principal product. During the second half of the nineteenth century, attempts were made by the Leblanc manufacturers to increase the output of chlorine and improve the efficiency of its production methods. Two major processes resulted: Weldon's in 1866 and Deacon's in 1868.

Until the invention of the electrolytic chlorine process in the 1890s, all the methods of manufacturing chlorine were based upon the oxidation of hydrochloric acid, differing only in the way in which this was done. In 1774 Scheele found that manganese dioxide was a suitable oxidising agent and it was used by all commercial processes for the next eighty years. A serious problem was that all the expensive manganese dioxide was converted to manganese sulphate and chloride, and could not be recovered. The value of the lost manganese dioxide was a considerable proportion of the cost of the chlorine which was produced; inventors of new chlorine processes concentrated their efforts on recovering it. Dunlop's process was patented in 1855 [3.1]; the percentage of manganese dioxide recovered was not high, but the fact that it was possible encouraged others. His process was, however, costly and had limited commercial application. Several inventions used oxidising agents other than manganese dioxide, but none achieved commercial success.

During the 1830s, the Leblanc manufacturers began to address the long-standing problem of the discharge of hydrogen chloride into the atmosphere, by dissolving it in water in absorption towers (see Chapter 7). At first, much of the hydrochloric acid produced in this way was run to waste, since there

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was more than enough to satisfy current demands from industry. Unfortunately, this acid effluent replaced the air pollution problem by one of river pollution. The situation improved when it was found that hydrochloric acid was a suitable raw material for chlorine manufacture. New processes based upon hydrochloric acid soon began to replace the earlier ones which started from salt and sulphuric acid. However, in 1862, only 25% of the available hydrogen chloride was utilised in this way [3.2].

The manganese dioxide chlorine process continued to improve and, by 1866, the amount of chlorine produced by it had risen substantially in response to increasing demand, particularly as a result of the growth of the textiles and paper industries, which used chlorine for bleaching.

The most commercially successful chlorine process at that time was invented by Weldon. Its novel feature was the recovery of manganese dioxide, but the entire process became known as Weldon's chlorine process [3.3, 3.4]. Although it recovered 95% of the manganese dioxide, only one-third of the available chlorine in the hydrochloric acid was converted to chlorine. This was, however, of high concentration and very suitable for bleaching powder manufacture (see Chapter 4).

Weldon's process is discussed here because it was one of the two chlorine processes of the late nineteenth century, the other being Deacon's. Although technically different, they were often operated together until well into the twentieth century, when the electrolytic alkali process displaced them.

In 1869, just after Deacon had patented his chlorine process, Weldon's was in operation at Gaskell Deacon's works and Hurter worked with Weldon on a study of its chemistry. Holbrook Gaskell II, Deacon's partner, described the work:

Weldon's process is doing very well ... Mr Weldon has read a paper before the British Association in which he shows that the Peroxide of Manganese is combined with the lime forming a Manganite of Calcium. Dr Hurter assisted him in his experiments to find out what the compound was... [3.5]

Hurter's involvement is surprising, since Deacon's process was expected to become a competitor of Weldon's, and Hurter was Deacon's employee. It is possible that Deacon may have contracted Hurter out to Weldon to assist him, because of Hurter's growing reputation as an investigative chemist.

Weldon attempted to improve his process by developing two new versions based on reactions involving magnesium compounds [3.6], [3.7]. These had a short commercial life and were unable to compete with the Deacon process. In 1883 Hurter presented a paper to the SCI, reviewing the current chlorine processes: Deacon, Weldon No. 1, Weldon No. 2 and Weldon-Pechiney [3.8]. Because of the similarity between Weldon No.2 and Weldon-Pechiney, coupled with trade secrecy, many chemists were unclear about the details of these. Hurter confused Weldon No. 2 with Weldon-Pechiney and, during the discussion of his paper, Weldon corrected him:

I agree with his [Hurter's] criticism of what he has called "Weldon No.2", but he is in error in supposing that the process to which he has referred by that name is the process which we are now endeavouring to realise at Salindres [ie the Weldon-Pechiney]. The error is not his but it nevertheless is an error, and hence his criticism of "Weldon No.2" does not apply to the process that my friend M.Pechiney will succeed in realising. That process, while it undoubtedly grew out of the magnesium manganite process [Weldon No.2] which Dr Hurter has referred to, differs therefrom very materially indeed.

Which Hurter acknowledged, but without apology:

... I am very much obliged to Mr Weldon for his compliments [on the Deacon process]. Of course, if the process I have discussed as Weldon's process No.2 is not the one which is about to be worked on a large scale at Salindres [the Weldon-Pechiney], my remarks fall to the ground ...

The advantages of a process which would be able to use air for the direct oxidation of hydrogen chloride to chlorine, instead of manganese dioxide, were obvious from an early date: several of these were invented between 1845 and 1866 [3.9]. Although none was commercially viable, the stage was set for the invention of the Deacon chlorine process, which in time was as successful as Weldon's. The fact that many of the earlier processes had tried copper salts as a catalyst for the oxidation is significant.

Deacon studied copper salts as catalysts for the oxidation of both hydrogen chloride and sulphur dioxide (for the manufacture of sulphuric acid) in 1867. The first patent for his chlorine process was obtained in 1868, after he had solved the problem of catalysing the reaction in a large flow of mixed gases at a temperature low enough to produce chlorine in economic quantities [3.10].

In a paper to the British Association in 1870, Deacon described a more sophisticated version of the process, in which the metallic oxides were replaced by a refractory material impregnated with copper sulphate [3.11]. Hurter published a comprehensive heat balance for the process [3.12]. This was a piece of classical chemical engineering which, although a routine procedure at the present time, was innovative then. A description of the earliest operational form of the Deacon process was given by Kingzett in 1877 [3.13].

Deacon's invention was a continuous gas-phase process, whereas earlier ones had been batch processes carried out in the liquid phase. The advantages of Deacon's process were that it was easier to obtain steady operating conditions, and hydrogen chloride could be used direct from the Leblanc saltcake furnaces. Deacon described the development of the process and the mechanism of the catalytic reaction in 1872 [3.14]. It is probable that Hurter carried out a good deal of the work described in it; its style suggests that he may have written some of it.

Deacon began work on his process soon after the Norwegians Waage and Guldberg revived Berthollet's law of mass action in 1864 [3.15]. He described his understanding of the reaction:

Hydrochloric acid gas and ... air [are passed] over heated compounds ... one of the elements of which must have the power of absorbing oxygen ... when heated and must possess, when subsequently treated with hydrochloric acid and heated, ... the power of decomposing such acid and of ... yielding chlorine ... [3.10].

It was not until 1928 that Beebe and Summers explained the mechanism of the first reaction: the conversion of copper sulphate to chloride [3.16].

Kingzett suggested that the mechanism of the second reaction: the catalytic oxidation of hydrogen chloride to chlorine, was that the copper chloride first reacted with oxygen to form chlorine and copper oxide, which reacted with hydrogen chloride to form copper chloride [3.13].

Hurter's interpretation was similar, but he assumed the existence of intermediate compounds: basic cuprous chloride, or cupric chloride. He was probably correct in the light of modern knowledge of catalytic mechanisms [3.17], [3.18], but the final explanation was not provided until the publication of the First Report of the Committee on Contact Catalysis, 1922 [3.17].

Deacon's paper to the Chemical Society reveals how his background (as an industrial engineer rather than a chemist) formed his ideas on the mechanism of catalysis [3.19]. He probably benefited from discussions with Hurter, who had the advantage of a good theoretical background, received from Bunsen. Deacon referred to "Bunsen's Gasometry", written in 1857, in which it is stated that:

... chemical affinity is the result of the attractive forces exerted by *all* the molecules within the sphere of chemical attraction, whether these molecules take part in the chemical action or not [3.20].

Deacon understood that the copper atoms in the catalyst affected the affinity between the hydrogen and chlorine atoms in the hydrogen chloride. The main conclusions of his paper, viz. that the surface of the catalyst was more important than its mass; the amount of copper chloride was not related to the amount of chlorine produced; and the molecule of the copper salt "is only a medium or fulcrum for the other forces", are generally acceptable in the light of present-day catalytic theory.

The Deacon process was one of the first examples of the use of catalysis on the production scale. The phenomenon of catalysis was not new in the mid-nineteenth century, but it was far from being fully understood. An account of early theories of catalysis is given by Partington [3.21].

It might be thought that Deacon's process, because of its technical elegance, was an instant success when it was introduced in 1868. This was far from being so: years of development were required to perfect it. Even Deacon, writing to a potential licensee of his process in 1870, expressed some reservations:

In reply to yours of yesterday, Mr Weldon's process is a bird in hand. My process, as far as the commercial manufacture of bleaching powder [is concerned], is not [3.22].

It was certainly Deacon's intention that his process would be more profitable than any other; in 1872 he wrote:

My problem has been how, readily, regularly, and continuously to make from a heated current of hydrochloric acid gas, mixed with air, the largest quantity of chlorine in the smallest space, in the shortest time, at the least cost [3.14].

Deacon died from typhoid in 1876 at the early age of fifty four and so did not live to see his ambition fully achieved [3.23]. Progress towards the final version of his process was slow; in 1877 Kingzett remarked:

... for some period [after 1870], it was doubtful whether or not it [the Deacon process] would rival or even displace the Weldon process. Further experience, however, discovered difficulties in the working of this beautiful method ... which lessen its applicability. Although several plants have been erected ... most of them have been since abandoned, and at the present time ... most of the chlorine is manufactured [by] the process of Mr Weldon [3.13].

Hurter's laboratory notebooks confirm that he carried out much of the development work on the process [3.24]. Between 1871 and 1872 this included: the mechanical design of the decomposing furnace; the amount of copper sulphate to be charged into it; the fuel requirements for heating the decomposer and super-heater; the compilation of tables showing the weight and volume of gases produced per ton of salt decomposed; the operation of gas compressors; the construction of gas analysis apparatus; the analysis of arsenic and antimony dust deposits in the decomposer; and the adverse effects of clay on the solubility of copper sulphate. The notebooks also contain a full history of the operation of a decomposer and a statistical study of the cracking of pipes leading from the decomposer to the condenser. Other improvements are described in patent specifications. Between 1870 and Deacon's death in 1876, a number of improvements were made to the decomposer [3.25] to [3.28] and to the catalyst [3.29] to [3.34].

In 1873 Hurter effected a major improvement to the thermal efficiency, and hence the profitability, of the process. The decomposer was provided with a double wall and the annular space so provided acted as a heat exchanger, in which the feed gas to the decomposer was pre-heated by heat from the exothermic oxidation reaction [3.35].

Initially there were problems in controlling the gas flow rate through the decomposer, so as to ensure a constant temperature in it. To facilitate this, an accurate method of measuring high gas flow rates was required. Because there was no suitable instrument available, Hurter himself developed an suitable anemometer of the pitot tube type [3.36], [3.37].

Until 1883, poisoning of the catalyst by impurities in "roaster gas", the hydrogen chloride produced in the high temperature furnaces of the second stage of saltcake manufacture, precluded its use for making chlorine. The principal impurity was arsenic from the pyrites used in sulphuric acid manufacture. As early as 1871 Deacon insisted that only pure hydrogen chloride should be used [3.26]. He also believed that sulphuric acid was a catalyst poison, but this was probably not so [3.32]. In 1875 he patented a process for removing sulphuric acid from "roaster gas", but this proved unsuccessful in plant trials [3.38].

Because of the problem with "roaster gas", only the purer "pan gas", from the first stage of saltcake manufacture, was used to make chlorine. Although this was only about half of the available hydrogen chloride, the Deacon process was widely operated in this way from about 1872 and was commercially successful. However, it was necessary also to run a Weldon plant to deal with the "roaster gas", which was first dissolved in water in absorption towers and then processed as "roaster acid".

The problem was solved in 1883 by the German, Hasenclever, who had devised numerous improvements to the Leblanc system [3.39]. He mixed "roaster acid" with hot concentrated sulphuric acid and blew air through it, producing pure hydrogen chloride which passed to the decomposer. The sulphuric acid was diluted by the water in the "roaster acid" and had to be reconcentrated. Although the cost of this operation had to be taken into account, the ability now to process "roaster acid" made the new process more profitable than the basic Deacon process. Furthermore, unconverted hydrogen chloride leaving the decomposer could be recycled.

If Hurter was so talented a chemist, why was it left to Hasenclever to devise a means of purifying "roaster acid? It seems likely that Hurter could have solved this relatively simple problem, had this been necessary. In 1883, however, there was no great need to use "roaster acid" because sufficient chlorine was being produced by the Deacon and Weldon processes to satisfy the demand in Britain for bleaching powder. Hasenclever was working in Germany, where the situation may well have been different. Later in the nineteenth century, the increased demand for chlorine resulted in the Deacon-Hasenclever process becoming widely employed in Britain. Details of the final version are given in a UAC internal report [3.40], an American manual of industrial chemistry [3.41] and an ICI internal report [3.32].

Even before the introduction of Hasenclever's improvement, Deacon's process had attracted considerable interest and he had begun issuing licences for it as early as 1871 [3.42] to [3.44]. After

the improvement was made, the process was able to compete on equal terms with Weldon's. Although it was superior to it in a number of ways, it never replaced it; both continued in use until well into the twentieth century. This was because Deacon's chlorine, although less contaminated with trace impurities than Weldon's, had a concentration of only 5% to 10%, compared to Weldon's 85% [3.45]. This dilute chlorine could not be processed in traditional bleaching powder chambers and it was necessary for Hurter to design a new type (see Chapter 4).

The final version of Deacon's process, which he never in fact lived to see, satisfied all his original aspirations:

The chlorine produced was cheaper than that made by his only rival, Weldon, whose process required two tons of salt to produce one ton of bleaching powder. Deacon's first process required thirty three cwt and the final Deacon-Hasenclever development only one ton [3.9].

It was a single-stage continuous process, having a low labour requirement.

The continuous process ensured that the quality and flow rate of the chlorine passing to the bleaching powder chambers remained constant.

In 1888, after an interval of about thirteen years, Hurter and Henry Wade Deacon introduced further improvements. The first of these was to dry the hydrogen chloride with sulphuric acid before it entered the decomposer [3.46]. Previously, drying had been effected by cooling the gas, but much hydrogen chloride was lost in the condensate. This valuable development greatly improved the overall efficiency of the process and it was widely implemented [3.32].

A process was proposed for recovering unreacted hydrogen chloride which left the decomposer, although this does not appear to have been developed further [3.47].

Hurter invented a process to produce hydrogen chloride for manufacturing Deacon chlorine by reacting salt with sulphuric acid in the liquid phase, rather than in the saltcake furnace [3.48]. It is probable that this came about because, as ammonia-soda began to replace Leblanc soda, the manufacture of saltcake decreased. It does not appear that the process was developed commercially, however.

The work which Hurter carried out on developing Deacon's process was the earliest major project of his career and one of the most important. He received full recognition for this from both his contemporaries and from historians.

At a meeting of the SCI in 1883, Weldon, speaking about the Deacon process, which was a serious rival to his own, generously remarked:

> As regards ... the Deacon process, no-one admires that process more than I do. I regard it as the most beautiful industrial chemical process ever invented, and no words I could use would adequately express my admiration for the genius and skill which, through years of patient labour, Dr Hurter has devoted to its realisation [3.8].

Ludwig Mond, at a meeting of the Society in 1888, remarked:

> ... Dr Hurter's classical research on the decomposition of hydrochloric acid by air by means of catalytic substances.

> ... the well-known [Deacon] process now in use, the beauty, simplicity and thoroughly scientific character of which, combined with commercial efficiency, will make the names of Henry Deacon and Ferdinand Hurter ever famous in the annals of industrial chemistry [3.49].

An obituary of Hurter records that:

> Dr Hurter was closely associated with the late Mr Henry Deacon in the chlorine process which bears his name. Though Dr Hurter was ever too modest a chemist to lay claim to any share of the merits of that process, yet all his friends knew perfectly well who was the originator of the many details which made a crude process into one which was at once practicable and profitable [3.50].

Hardie wrote in 1950:

> Probably historical justice would have been better served had the method been named the Deacon-Hurter process [3.20].

And Hardie and Pratt in 1966:

> The finally effective form of Deacon's process was the result of ten years of research by Ferdinand Hurter ... [3.51]

Deacon also acknowledged Hurter's work, but his remarks were somewhat understated, probably because he believed that, since Hurter was his paid employee, the work was simply part of his normal duties. Even this limited acknowledgment was more, perhaps, than would have been obtained from

other employers at that time. The first reference by Deacon to Hurter's work is in a paper to the British Association in 1870:

A regular series of experiments were then decided upon, and all were performed by Dr Hurter in our laboratory, and to this gentleman belongs much of the credit of the success attending the experiments hitherto made. I claim the discovery and the reasoning that led up to it ... but all subsequent progress has been the result of constant conference between Dr Hurter ... and myself and I am glad to have this opportunity of acknowledging the value of [his] assistance [3.52].

And in 1872, in his paper to the Chemical Society, he said:

... I would acknowledge the assistance throughout of our laboratory chief, Dr F Hurter ... [3.14]

Lunge commented:

Deacon has himself publicly recognised the important services rendered in the working-out of his process by Ferdinand Hurter [3.53]

Hurter's name does not appear on any of the patents granted to Deacon, but this was not unusual at that time; the patent would have been in Deacon's name as the principal inventor. After Deacon's death in 1876, his son Henry Wade Deacon took over the company's commercial affairs and Hurter assumed responsibility for the technical development of the process. His name appeared on all subsequent patents.

Hurter's interest in chlorine manufacture was not confined to the Deacon process. In a paper presented to the SCI in 1883, he discussed the thermodynamic considerations necessary for liberating chlorine from chlorine compounds. The value of this data for comparing the relative efficiencies of the various chlorine manufacturing processes was discussed. Regarding the ammonia-soda and Leblanc processes, he stated that :

... the low cost of the carrier of chlorine has enabled the ammonia-soda process to take the lead in the manufacture of soda, and the peculiarly loose combination of hydrogen and chlorine has given the Leblanc process command of the chlorine industry ....

... the only practical way of decomposing common salt on account of the very great affinity of the two elements, sodium and chlorine, is by a double decomposition ...[3.54].

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It is seen that, at the time of publication of the paper, Hurter believed that only a chemically-based process could be used for manufacturing soda. The situation did eventually change, as a result of the invention of the electrolytic alkali process.

In 1891 Hurter carried out research on two new processes for manufacturing chlorine. The first of these was based on the gas-phase oxidation of hydrogen chloride; it had been invented in 1889 by de Wilde and Reychler, consultants to UAC. Between 1890 and 1894, they assigned a number of their patents to that company [3.39]. Their process consisted of passing, alternately, currents of air and hydrogen chloride through a heated mixture of manganese chloride, magnesium chloride and magnesium sulphate in a decomposer [3.55], chlorine being formed. Each alternate operation was conducted at a different temperature, causing chlorine to be produced at different concentrations, making it necessary to use two different types of bleaching powder chamber. Also, thermal stresses caused deterioration of the equipment.

Hurter, now Chief Chemist of UAC, applied his chemical engineering expertise to these problems, devising an arrangement of two heaters in parallel, one running on air, the other on hydrogen chloride. Each heater was connected to a decomposer. During the first "run" the exit gases from the two decomposers were mixed together. The heaters and decomposers were then interchanged and a second "run" carried out. In this way both decomposers were maintained at the same temperature and the chlorine concentration in the combined product gas stream was constant [3.56], [3.57].

Although there is no evidence that this process was operated commercially, Hurter's sophisticated technique of interchanging reactors to ensure constant operating conditions - the "push-pull" system - was highly innovative. It is a procedure which is employed in present-day chemical processes, such as the steam reforming of hydrocarbons. It seems likely that this was the first example of its use and that Hurter may well have invented the technique.

In 1891 Hurter evaluated a process for making chlorine by oxidising hydrogen chloride with nitric acid, which had been patented by Davis [3.58] to [3.61]. He concluded that the process was difficult to carry out and could not compete with Deacon's. He returned to the subject in 1896, when he presented the results of his own research to the Liverpool meeting of the British Association, in a paper entitled "The manufacture of chlorine by means of nitric acid". He described experiments in which anhydrous hydrogen chloride was passed into a mixture of nitric and sulphuric acids. This was expected to yield chlorine, nitrogen dioxide and water; the nitrogen dioxide then being re-oxidised to nitric acid.

The journal, "Engineering", commented:

There is a certain fascination about such a continuous process, as the numerous attempts and patents testify .

Success in the laboratory has, however, as yet always been followed by failure in practical application. Large bulks of sulphuric acid are needed and have to be concentrated. The plant required for this purpose is very costly and it would, moreover ... be very difficult to find materials which will withstand the strong acids employed [3.62].

Quite apart from these process problems, Hurter's experiments did not proceed as expected. The product of the reaction was a colourless gas which was decomposed spontaneously into nitrogen dioxide and chlorine by the action of light or heat. Hurter decided that it was nitryl chloride. This substance had first been prepared by Muller as early as 1862 [3.63] and so its existence would certainly have been known to Hurter in 1896. Surprisingly, the authoritative work on inorganic chemistry by Mellor attributes the discovery of nitryl chloride to Hasenbach in 1905 and does not mention the earlier work [3.64]. Hurter had not, of course, discovered nitryl chloride, but simply a new method of making it.

In reporting the matter, Hardie made an important mistake. He claimed that:

[Hurter had]...obtained a new, light-sensitive gas...the new compound... [3.65]

There is also an implied criticism of Hurter for not publicising his "discovery" of a new gas. In fact, as Hurter well knew, it was not a new gas at all. He might have been expected to publicise the discovery of a new method of making nitryl chloride, but the fact that he did not do so gives an indication of his pragmatism. He did not proceed further with the matter because he decided that to manufacture chlorine by this route (the original purpose of his research) was difficult, expensive and would probably not be commercially viable [3.66], [3.67].

Why then did he present this paper, in view of its unpromising nature? The probable explanation is that it was customary for an eminent local scientist to contribute to a British Association meeting. Given the somewhat abstruse nature of Hurter's paper, it may have been that "politics" had an influence on its being accepted. Because the paper was a "second level" contribution, a full transcript of it was not published; the only record is a summary published in the JSCI [3.66].

*   *   *

Hurter's work on the Deacon chlorine process was one of the most successful projects of his professional career. Although his university education had been largely academic, including very little applied chemistry or chemical engineering, the work shows that he learned these subjects quickly, his notebooks demonstrating his abilities in full-scale process experimentation and development.

Chlorine, and its derivative bleaching powder, were, by the 1880s, becoming the principal products of the Leblanc industry, replacing soda, which was made more cheaply by the ammonia-soda process. Hurter's work on developing a successful process for manufacturing chlorine was a major technical achievement which helped prolong the life of the Leblanc industry.

His work on chlorine manufacture is an excellent demonstration of his ability to work with both pure and applied chemistry, to acquire practical production experience and to bring a sound theoretical approach to the development of a chemical process.

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CHAPTER 4

BLEACHING POWDER

Most of the chlorine made by the Deacon process was used for making bleaching powder. Because this chlorine was of a much lower concentration than that made by the Weldon process, the bleaching powder plant associated with this was unsuitable for Deacon chlorine. Hurter embarked upon a comprehensive programme of research with the objective of designing a suitable plant. A sophisticated and efficient piece of equipment resulted, which was in widespread use for many years. In the course of carrying out the research, Hurter produced data which was invaluable for optimising not only Deacon bleaching powder plants, but also those which were employed in association with Weldon chlorine plants, and so the whole bleaching powder industry benefited from his work.

Hurter's published work and his laboratory notebooks show that the project was almost entirely in his hands, from laboratory studies of the mechanism of absorption of chlorine in lime, through to the design and construction of full-scale production plant.

As the textile industry developed in the eighteenth century, new processes for bleaching cloth were invented. By 1756, the bleaching agents: alkali, milk and sunlight had been replaced by sulphuric acid, and this change had stimulated the development of the new lead chamber sulphuric acid process. In 1787 Berthollet proposed the use of chlorine water, which was made by passing chlorine gas into cold potassium hydroxide solution, for bleaching. This was expensive to produce, however, so in 1798 Macintosh invented "bleaching powder", made by absorbing chlorine in slaked lime. The process was a commercial success: the new bleach was easier to transport and safer to use than was sulphuric acid. It was, however, a crude material chemically and its formula was the subject of discussion among chemists for many years. Partington traces the debate from 1813 to 1935 [4.1], when it was finally decided that bleaching powder was substantially a mixture of calcium hypochlorite and basic calcium chloride. The strength of bleaching powder was expressed in terms of the percentage chlorine available for bleaching. Thus, bleaching powder which contained, for example, 37% available chlorine was termed 37% bleaching powder.

Until the electrolytic alkali process was invented at the end of the nineteenth century, chlorine (and hence bleaching powder) manufacture was exclusively the province of the Leblanc industry, since it was impracticable for it to be made by the ammonia-soda process [4.2].

The techniques of textile bleaching are described by Muspratt [4.3], and the manufacture of bleaching powder by the same author [4.4] and by Partington [4.5]. This process used high strength (85% to 95%) chlorine made by the Weldon and similar processes. A summary of it is as follows:

Slaked lime was made by sprinkling quicklime with water. The fine powder formed was cooled and sieved. It was spread in a thin layer on the floor of a lead box with an asphalt floor, which was about six feet high, fifteen feet wide and one hundred feet long. The lime was raked into furrows, the chamber sealed and chlorine admitted. The absorption of chlorine into the lime was monitored by observing the change in the colour of the chlorine. Absorption was rapid at first, decreased over a period of twelve to twenty four hours, then ceased. The solid mass was then raked over and the process repeated. Finally, unused chlorine was removed by blowing lime dust into the chamber and the bleaching powder was raked out.

This traditional bleaching powder plant could not be used with Deacon chlorine, because the latter was too dilute (5% to 10%), so Hurter and Deacon designed a new type of plant. It consisted of a number of chambers which contained stacks of shelves through which the chlorine flowed over layers of lime. This was a counter-current gas-solid contact operation, with the solid phase moving intermittently.

Although it was labour intensive, the process included two innovations: the lime was spread very thinly to ensure efficient absorption of the chlorine; and it was an early attempt at a continuous process, a considerable improvement over the traditional intermittent one. The Deacon bleaching powder process did not replace the traditional process, however. The Weldon and Deacon chlorine processes, with their associated bleaching powder plants, were often operated side by side.

Deacon patented the basic version of the process in 1870 [4.6], but Hurter's laboratory notebooks and published papers show that he carried out the experimental work on it. A later patent by Deacon describes how the process could be improved to make it fully counter-current, with both gas and solid phases in motion [4.7]. A thin layer of lime was maintained on inclined shelves set one above the other in a vertical chamber. The shelves sloped in opposite directions, so that the powder slid across them and progressed downwards through the chamber. The idea does not, however, appear to have been adopted, since a Brunner Mond report of 1904 [4.8], describes the plant arrangement as it was in Deacon's original patent.

Hurter's research on bleaching powder had the following objectives:

To design a plant to manufacture bleaching powder from dilute (Deacon) chlorine.

To optimise the operation of this plant with respect to bleaching powder strength and production output.

To make improvements to the traditional bleaching powder process.

He decided that fundamental research was needed into the theory and practice of bleaching powder manufacture. The existing process was primitive and its mechanism had never been properly understood. His work was published in 1877 in three papers which gave practical recommendations for designing and improving production plants [4.9] to [4.11]. A summary of his research follows; the original papers provide a great deal of detail of his experimental results and of the mathematical formulae which he derived from them.

His experimental work began with a study in the laboratory of the physics of the flow of a gas through a layer of solid particles, using air and fine sand; he found that the gas velocity was proportional to its pressure and inversely proportional to the depth of the layer. Using this data, he investigated the effects of the following factors on the absorption of dilute (Deacon) chlorine in slaked lime: contact time; chlorine concentration; depth of the lime layer; the adverse effects of contaminants in the chlorine: carbon dioxide, hydrogen chloride and water vapour; and temperature.

His study of the detrimental effects of contaminants in the chlorine on the formation of bleaching powder showed that carbon dioxide and water vapour presented the greatest problem, hydrogen chloride less so. Because carbon dioxide was difficult to remove from the chlorine, it was important to exclude it from it in the first place.

He studied the effects of carbon dioxide in the full-scale process and found that, provided the chlorine entering the bleaching powder chambers was well above 5%, carbon dioxide was not a problem. Below 5%, it converted the lime to calcium carbonate, reducing the absorption efficiency and making it difficult to produce high strength bleaching powder.

The carbon dioxide entered the chlorine at the chlorine plant, where the process gases passed through a heater and a decomposition oven, both of which were heated by products of combustion. These contained carbon dioxide which could leak into the chlorine. Typical carbon dioxide concentrations in the gas throughout the process were: 5% at the entrance to the heater, 19% at the inlet to the decomposition oven and 38% at its outlet. It was therefore important to check the chlorine plant regularly for leakages and this was done indirectly, by monitoring the efficiency of the bleaching powder plant. An acceptable bleaching powder strength was 37%; below this, the gas tightness of the chlorine plant required attention.

Hydrogen chloride adversely affected the absorption of chlorine in lime, but could be easily removed from the chlorine by dissolution in water.

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Water vapour was more serious. At a content of 3% in the chlorine, an impenetrable layer was formed on the surface of the lime, inhibiting chlorine absorption. Higher concentrations of water vapour could reduce the lime bed to mud. Water also encouraged the formation of calcium chlorate in the bleaching powder. It was therefore essential to dry the chlorine before it reached the bleaching powder chambers; this was done in sulphuric acid drying towers. Hurter concluded that, if water and carbon dioxide were excluded from the chlorine, then high quality bleaching powder could be made from Deacon chlorine; this had been confirmed in practice.

He examined the effects of temperature on bleaching powder manufacture. It was easier to produce high quality bleaching powder in winter than in summer, but on very cold days chlorine absorption could slow down and even stop. He had assumed that the absorption rate rose with temperature, as in many other chemical reactions, so higher temperatures were advantageous. This was not always so: above a certain temperature, the bleaching powder could become overheated, changing from a fine powder to a crumbly mass. To prevent this, the heat had to be dissipated. Riddel had patented a rotating cooler for this purpose [4.12].

Figures for the heat generated during the absorption were used to specify the cooling requirements for the chambers. It was found that concentrated (Weldon) chlorine could not be used in Deacon chambers, due to overheating; it had to be diluted with air and dried before it could be processed in a Deacon plant. This was vital information at factories where both Weldon and Deacon chlorine and bleaching powder plants were in operation.

Hurter was aware of the problems of scaling up laboratory experiments when designing production plant, but he believed that his laboratory results were of value nonetheless. He therefore proceeded to study the absorption of chlorine in lime, especially the relationship between absorption rate and time, on a full-scale production plant. An understanding of this was essential to the efficient operation of a plant, since it affected the time required to manufacture a given quantity of product, which in turn was related to the cost of labour.

In the traditional process, the lime was turned over by hand when the calcium hydroxide contained about 37% chlorine. In the Deacon plant, because the chlorine was dilute, a larger surface area and a smaller depth of lime were required and the labour cost could be unacceptably high. It was therefore necessary to determine the optimum depth of the lime layer. Hurter found that the ability of a bed of lime which was covered with bleaching powder to absorb chlorine was only about half that of fresh lime. At the beginning of the process absorption was very fast then, as the gas penetrated deeper, it slowly fell to zero as the lime became saturated. Thus the bed of lime could be regarded as a number of thin layers which absorbed chlorine to differing degrees, producing acceptable bleaching powder ranging through to unreacted calcium hydroxide. For practical purposes the absorption was considered

as proceeding in three stages: the first, during which the absorption rate fell from 100% to 50%; the second, during which it was approximately constant; and the third, when it gradually fell to zero.

Although the laboratory work had shown that, in theory, the depth of the lime layer would not affect the rate of absorption of chlorine, it appeared that, to optimise the economics of the process, the bleaching powder chambers should be designed so that the whole depth of the lime was penetrated by chlorine, and all of this was absorbed, within an acceptable time. To satisfy the first requirement, the depth of the lime layer had to be calculated and, for the second, its total surface area. The most concentrated chlorine came into contact with completed bleaching powder and the most depleted chlorine with fresh lime. Thus an average concentration of half that of the fresh chlorine was presented to the lime overall. In traditional bleaching powder plants, three to four days were required to complete the absorption and other operations, so Hurter assumed a batch time of ninety six hours when designing the chambers for dilute chlorine. His theory predicted an optimum depth of lime bed as 1.6 cm, which was confirmed in practice.

Finally, he calculated the depth to which chlorine of a given concentration would need to penetrate lime to produce 36% bleaching powder. He also showed that, if the lime layer was raked up into ridges, the absorption time was more than halved, due to the greater surface area of lime which was exposed to the gas.

After completing his experiments in the laboratory and on the plant, Hurter adopted a theoretical approach to derive formulae which related chlorine flow rate, partial pressure and time. Obtaining good agreement between this theoretical data and his experimental results, he proceeded to the design of a full-scale production plant.

He used his formulae to calculate the dimensions of a plant for use with dilute (Deacon) chlorine which would produce twenty five tons of bleaching powder per week. He reported that six years' experience in operating this plant had shown good agreement between the theoretical prediction of its performance and the results obtained in practice.

He had found that, when the Deacon chlorine plant was working well, with a high chlorine pressure, as much as forty tons per week of bleaching powder could be produced. If, however, it was running badly, when the chlorine concentration might fall as low as 5%, the absorption time rose considerably and the output fell to sixteen tons per week. Below 5%, chlorine absorption was found to be negligible. This proved how essential it was to eliminate leakages of air into the process equipment, which diluted the chlorine.

In 1892, when he was at UAC, Hurter returned to the subject of bleaching powder manufacture when he patented a modification to the Deacon chlorine plant [4.13]. The purpose of this was to improve the operation of the associated bleaching powder plant, by controlling the temperature of the chlorine passing from the sulphuric acid drying tower to the bleaching powder chambers. Contact with

the acid usually raised the temperature of the chlorine above ambient, except in very cold weather, when it could become too cold to be absorbed efficiently by the lime. The modification kept the gas temperature constant, greatly improving the efficiency of the plant.

The gas was cooled by refrigerating the sulphuric acid in the drying tower and also, if necessary, cooling the gas in a heat exchanger using refrigerated brine. If the gas had to be heated (in cold weather), the heat of solution of water in sulphuric acid was employed, also the heat exchanger could be fed with hot water.

The chemical engineering reasoning behind this invention was novel, sophisticated and years ahead of its time. The concept of controlling an operating parameter (in this case, temperature) by varying the conditions in secondary equipment (the drying tower) was innovative. Also the use of refrigeration for process temperature control, although not unusual today, was uncommon in the 1890s.

*   *   *

Hurter's detailed and painstaking research on bleaching powder production had an entirely practical objective. Deacon's chlorine process suffered from one disadvantage: the chlorine produced by it was too dilute to enable it to be used for making bleaching powder in traditional plants. Instead of using the commonly employed empirical type of approach to develop a plant suitable for dilute chlorine, Hurter, characteristically, went fully into the theory and practice of the subject. As a result, the plant which he designed was sophisticated and highly efficient. His research also produced data for use in improving traditional plants and so the whole bleaching powder industry benefited from his work.

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CHAPTER 5

SULPHURIC ACID

From the earliest days of the Industrial Revolution, sulphuric acid was one of the most important chemicals used. Lord Beaconsfield was well aware of its immense industrial importance when he wrote:

> There is no better barometer to show the state of an industrial nation than the figure representing the consumption of sulphuric acid per head of the population [5.1].

Justus von Liebig was of the same opinion:

> It is no exaggeration to say that we may fairly judge of the commercial prosperity of a country from the amount of sulphuric acid it consumes [5.2].

The manufacture of sulphuric acid was the vital preliminary stage, in fact could be regarded as the first stage, of the Leblanc system. The process was also important in its own right for providing sulphuric acid for many other applications: the manufacture of fertilisers, explosives, dyestuffs and other chemicals. Lunge gives a comprehensive description of the methods of manufacture and uses of sulphuric acid [5.3]. One of the best accounts of the history of sulphuric acid manufacture is by Dickinson [5.4].

This chapter is concerned mainly with Hurter's work on the lead chamber sulphuric acid process. During the nineteenth century this was the only feasible industrial process for manufacturing the acid, until the contact process came into commercial use in 1898. When Hurter arrived in Britain, the lead chamber process had been in use for almost a century and a number of improvements had already been made to it. Because of its industrial and economic importance, research on its development continued until well into the twentieth century. Hurter made a number of valuable contributions to this.

Full descriptions of the lead chamber process are given in many textbooks but, to assist in understanding Hurter's work, a summary follows. A flow diagram is given in Appendix II.

Sulphur, iron pyrites (iron sulphide), or gas-works spent oxide, were roasted in a current of air to produce sulphur dioxide. The sulphur dioxide passed from the sulphur burner, up the Glover tower, countercurrent to a stream of sulphuric acid from the Gay-Lussac tower. This acid contained the oxides of nitrogen which had left the lead chamber. In the Glover tower, the Gay Lussac acid was heated by the hot sulphur dioxide, the oxides of nitrogen were released from it and the gas mixture passed into the lead chamber. In the chamber, excess air from the sulphur burners reacted with sulphur dioxide to form sulphur trioxide, the reaction being "assisted" by a mixture of nitric oxide, nitrogen dioxide and nitrogen trioxide. The oxides of nitrogen were obtained in the first instance from sodium nitrate (nitre); thereafter, most of it was re-cycled. Reaction between the sulphur trioxide and water or steam produced sulphuric acid, which flowed from the bottom of the chamber. The oxides of nitrogen, nitrogen from the air and unreacted sulphur dioxide passed to the bottom of the Gay Lussac tower, where the oxides of nitrogen and sulphur dioxide were recovered by dissolution in cold sulphuric acid. The nitrogen and oxygen (with some oxides of nitrogen) were vented to atmosphere. The acid from the Gay Lussac tower was then recycled to the top of the Glover tower. Not all the oxides of nitrogen could be recovered and losses from the system were replaced by adding small amounts to the sulphur dioxide just before it entered the chamber.

The term "nitre" was used to describe a variety of compounds by nineteenth century chemists: sodium nitrate, potassium nitrate, or the mixture of oxides of nitrogen used in the lead chamber process. In this thesis it means sodium nitrate.

The reactions in the lead chamber plant were complex; the subject exercised the attention of chemists for over 100 years. The position as understood in the latter part of the nineteenth century was given by Lunge [5.5] and by Hurter [5.6]. The (probably) final opinion on the matter was published by Stivenius-Nielsen in 1943 [5.7].

The economic optimisation of any process is of vital importance and, in the nineteenth century, nowhere was this more true than in the case of sulphuric acid manufacture. It was the key to the economic viability not only of the Leblanc system but also of many other chemical processes. Many improvements were made to the lead chamber process over the years, the principal ones being due to Clement and Desormes (1793), Holker (1810), Gay-Lussac (1827) and Glover (1859) [5.8].

A major factor in the profitability of the lead chamber process was the provision of oxides of nitrogen. This was a major cost on the process due both to their actual loss from the system and to the need to remove them from the final product.

The oxides of nitrogen were lost from the system as "mechanical" losses (dissolved in the product acid and the vent gases) and as "chemical" losses (irreversible chemical decomposition, mainly in the Glover tower, by reduction to lower oxides and nitrogen). Air pollution by oxides of nitrogen emitted from the plant's vent stack was a serious problem.

In the second half of the nineteenth century, improving the economics of the lead chamber process, particularly by minimising the losses of oxides of nitrogen, became almost an obsession with sulphuric acid plant managers. The description given below of a massive correspondence in the technical literature about the function of the Glover tower confirms this.

Hurter was fully involved with operating and improving the lead chamber process from his earliest days with Gaskell Deacon. He carried out research on a number of matters, including: the denitration of acid in the Glover tower and the reactions associated with this; and a study of the thermodynamics and reaction kinetics of the whole process. His earliest work on the Glover tower was carried out in 1869. He recalled in 1896:

> I am proud to say that the first Glover tower introduced in Lancashire started work on the 29th December 1868 under my superintendence, the second on the 21st May 1869, and the third in July 1870, in the works of Gaskell Deacon and Co., where I was then chemist [5.9].

Before discussing the Glover tower, however, it is necessary to consider the first major improvement to the lead chamber process, the Gay-Lussac tower. Its function was to absorb in sulphuric acid the oxides of nitrogen which were contained in the vent gases from the process, so as to prevent their loss into the atmosphere. The acid was then diluted and heated with steam, when the oxides of nitrogen were recovered from it and recycled to the chambers. The system was first tried out in France in 1827 by Gay-Lussac, who found that it reduced the consumption of sodium nitrate by two-thirds. The invention was, however, largely ignored in Britain at the time, because sodium nitrate was so cheap that it was not worth going to the trouble and expense of installing the tower.

The Glover tower was invented in 1859 by John Glover. It offered substantial benefits to the lead chamber process:

> It recovered oxides of nitrogen from the product acid and re-cycled them for re-use by transfer into the sulphur dioxide leaving the burners.

> It concentrated the acid from the chambers and from the Gay-Lussac tower. This acid was then re-cycled to the Gay-Lussac tower for recovering oxides of nitrogen.

> It generated steam for the chambers and cooled the burner gases.

Once the Glover tower became established, the Gay-Lussac tower was gradually introduced in partnership with it, producing the lead chamber plant in its final form (see Appendix II). A typical plant

consisted of a number of Gay-Lussac and Glover towers in combination with a series of lead chambers; this was known as a "with towers" plant. The adoption of these improvements was neither widespread nor immediate, however; "without towers" plants continued in operation for some years.

In 1896, at the fifth Annual Meeting of the SCI, John Glover was presented with a medal in recognition of his invention. Hurter was not present at the meeting, but he submitted a written contribution in which he described the background to the invention of the Glover tower and the benefits which it had conferred upon the lead chamber process, particularly the improvement of its profitability [5.9].

Hurter had not always been favourably disposed towards the Glover tower, however. Between 1874 and 1879, a long series of papers appeared in Dinglers Polytechnisches Journal and in Chemical News. The purpose of the correspondence was to resolve, by soundly-based scientific discussion, the question as to whether the Glover tower did, in fact, reduce the "chemical" losses of oxides of nitrogen from the process. The other advantages of the Glover tower were not in dispute.

There was a major difference of opinion between Hurter and Lunge about the function of the Glover tower, which the correspondence did not resolve. Hurter believed that the Glover tower did not de-nitrate (i.e. recover the oxides of nitrogen from) the acid passing through it to any significant extent; Lunge believed that it did. The other participants either supported, or disagreed with, these views. Agreement was not obtained at that time; in fact the matter was not resolved until several years later. The correspondence is fully documented in references [5.10] to [5.36].

The massive correspondence, which consisted of lengthy dissertations and shorter letters, contained over 50,000 words, of which 14,000 were by Hurter and 15,000 by Lunge (a university professor with industrial experience), with shorter contributions from Vorster and McTear (both alkali manufacturers) and Davis (one of H M Alkali Inspectors). Hurter's longest paper was of 7,500 words.

Vorster, Hurter, McTear and Davis carried out their investigations both in the laboratory and on full-scale production plants. Lunge's work was done on a glass pilot plant, which he constructed himself. Hurter's comprehensive research was particularly detailed and of his customary high standard; he reported an enormous amount of experimental data which he obtained on Gaskell Deacon's sulphuric acid plants.

The researchers encountered difficulties in obtaining an accurate material balance of oxides of nitrogen throughout the process. There was doubt as to the accuracy and reliability of the methods used for determining the concentration of oxides of nitrogen in sulphuric acid and in the vent gases and for measuring the vent gas mass flow rates. Work had to be carried out to resolve all these matters, so that the principal objective of the research could be properly pursued.

The final conclusion of the correspondence - which was more subjective than objective - was that, whilst many sulphuric acid manufacturers were convinced that the Glover tower recovered oxides of

nitrogen to an extent which benefited the economics of the process, this had not been established by the research described in it.

The purpose of discussing the Glover tower correspondence is not primarily to examine its technical subject matter: the function and efficiency of the Glover tower. Its value lies in providing information about the attitudes and inter-relationships of nineteenth century industrial chemists; about Hurter's character and professional abilities; and about his contribution to the understanding of the lead chamber process. It shows that chemists from different backgrounds: industry, government service and university were willing to devote a great deal of time and trouble to carrying out extensive programmes of research, both in the laboratory and on process plant, and writing lengthy papers for publication. It is debatable whether the subject of the correspondence was of sufficient importance to justify such efforts. It is most unlikely that, in the modern-day chemical industry, such a programme of work would be permitted to proceed without proper economic justification. Nonetheless, the participants deserve credit for the large amount of work which was done and the meticulous way in which it was carried out.

The tenacity of Lunge and Hurter in persisting with their opinions, only being diverted from them when convincing counter arguments were advanced, is perhaps due to their Germanic origins and education. During the course of the correspondence they appeared to exhibit some antagonism towards each other, but Hurter was willing to capitulate (though never apologetically) when a point was proved against him. They were, however, surprisingly generous to each other at other times. It appears that they regarded their differences purely as matters of scientific fact - dispassionately and objectively - with no personal feelings involved. Hurter and Lunge had earlier collaborated amicably, in 1884, on the production of the Alkali-Makers' Pocketbook (Chapter 12), but were to differ again, in 1893, on the subject of absorption tower design (Chapter 7).

The correspondence illustrates the emerging desire among industrial chemists in the late nineteenth century for a proper scientific understanding of chemical processes. This is confirmed repeatedly in the technical literature of the time, particularly the Journal of the Society of Chemical Industry, which was introduced in 1882 (see Chapter 12).

Eventually, the function of the Glover tower became properly understood and Hurter was finally convinced of its efficiency in recovering oxides of nitrogen [5.37]. A written contribution to the SCI meeting in 1896 confirms that he had revised his earlier opinions [5.9]. He explained his understanding of the principal purpose of the Glover tower: this was to recover oxides of nitrogen, but he emphasised that this was not done entirely in the Glover tower itself. Some oxides *were* recovered in the Glover tower, but most were recovered in the Gay Lussac tower by dissolution in concentrated acid which had been produced in the Glover tower.

It is to Hurter's credit that he was willing to acknowledge the final outcome of the matter in this way, but it is clear that he did not do so without first being firmly convinced that it had been properly and scientifically proved.

Hurter was responsible for an impressive study of the complex theories of the reactions in the lead chamber process. In 1882 he published the results of his work in a series of three papers entitled "The dynamic theory of the manufacture of sulphuric acid" [5.38] to [5.40]. The research described is a good example of his practice of applying a theoretical approach to the study of a production process, with the object of producing practical recommendations for improving it. These papers were probably the most theoretical and mathematically complex of all Hurter's publications. He applied the new science of reaction kinetics (known then as chemical dynamics), a subject in which he had shown an interest in a number of other areas.

Although the lead chamber process had at that time been in use for about a hundred years, the reactions involved in it were far from being properly understood. There had been a great deal of debate on the subject, but for much of the time progress had been hindered by there having been no means available for writing down either chemical formulae or reactions. Furthermore, the study of reaction kinetics was very much in its infancy.

The "primary" reactions of the lead chamber process were: the oxidation of sulphur to sulphur dioxide and then to sulphur trioxide; and reacting the trioxide with water to produce sulphuric acid. The reaction was "facilitated" by oxides of nitrogen. There were also several "secondary" reactions, which were often used to explain the loss of oxides of nitrogen from the system. Hurter reported upon only the primary ones on this occasion.

Because the objective of his research was primarily practical, the experimental work was carried out on full-scale lead chamber plants in normal operation. The relationship between the dimensions of the chambers and the temperature within them was found to depend upon the way in which a number of chambers was combined in sets. The data could be used to devise the most efficient arrangement of the chambers.

Hurter applied a principle of reaction kinetics which referred to the velocity, or rate, of chemical change. Previous research in this field had been confined to simple reactions involving only two substances. Harcourt had claimed that:

> When, in any chemical system, a change occurs whereby one substance only is caused to decrease in amount, the other substances being kept constant, then the amounts of the substance which vanish in equal times form a geometric progression.

This explanation was too simple even for the primary reactions of the lead chamber process, so Hurter proposed the following, which he had derived from the dynamic theory of gases, as advanced by Kronig, Clausius and Maxwell:

> The rate of chemical change depends upon, and is proportional to, the facility with which groups of molecules favourable to the particular change can form in the system in which the change occurs.

Hurter carried out a mathematical analysis of this principle as it applied to the primary reactions of the process, and then proceeded to practical trials on operational production plant. He chose to carry out the research on lead chamber plants of the "without towers" type because, in this arrangement, all the oxides of nitrogen which entered the process were consumed and none were recycled, thereby giving an exact measure of the amount used. He obtained figures for chamber volume, average annual nitre consumption and the strength and quantity of acid produced from five different sulphuric acid works. He determined the percentage conversion of sulphur dioxide to sulphur trioxide (and hence the amount of acid produced) in successive chambers of a series. This was related to the heat of reaction and therefore to the temperature. The amounts of acid produced and the heat of reaction evolved in each chamber were found to decrease geometrically. He also obtained relationships between the chamber temperatures and the amount of acid made in each chamber and between chamber volume and the concentration of oxygen and sulphur dioxide in the gas phase.

He concluded that the work done by a set of chambers depended upon the quantity of nitre used and upon the strength of the acid produced, and that this decreased geometrically, as did the chamber temperatures. The temperature of the first chamber depended on its fraction of the total volume of all the chambers (i.e. was related to the number of chambers connected to it). Finally, he deduced the composition of the gases which would give the optimum product yield with a minimum of chamber space; he found this to be that which gave about 7.95% oxygen in the vent gas.

The stated purpose of Hurter's research was to provide a proper understanding of the operating conditions of the lead chamber process, thus enabling improvements to be made to it. He seemed also to be using the work as an intellectual exercise to demonstrate his abilities in physical chemistry and mathematics. It shows that he was familiar with the work of at least some of the leading workers in the emerging disciplines of thermodynamics and reaction kinetics, although it is a pity that he included no literature references. He concluded his paper with the following words:

> The application of a principle of chemical dynamics has given us in a short time, and with greater precision, results which it has otherwise taken a century's experience to arrive at.

None of Hurter's three papers produced any comment or discussion in the literature at the time of their publication. It may be that, because of their highly theoretical nature, they caused Hurter to lose some of his credibility as a practical industrial chemist. The lack of interest by Hurter's contemporaries was probably because they were, in the main, practising industrial chemists, with perhaps limited theoretical knowledge. They may therefore have found little in his work which was of practical value and also might not have understood much of it. A comment made after his death bears this out:

> This paper, whilst able and exhaustive, has been generally considered beyond the domain of practical, working conditions. It is replete with characteristic mathematical considerations of a manufacturing process .... [5.41]

The research did not go entirely unacknowledged, however. At the fifth annual meeting of the SCI, in 1886, the President, Edmund Muspratt, said:

> Hurter's paper on the dynamic theory of the manufacture of sulphuric acid seems finally to have placed the operation on a thoroughly scientific footing [5.42].

In the same year as he published his papers on sulphuric acid manufacture, Hurter delivered a lecture to the Widnes Science and Art Classes, entitled "The manufacture of sulphuric acid and recovery of nitrogen compounds". He also published it privately as a monograph [5.6].

The nature of the lecture's audience suggests that it should have been of an elementary nature. Closer examination reveals that this was far from being the case. The lengthy document is a treatise of great erudition and scholarship. Hurter claimed that its purpose was to:

> ...try to convey...the scientific principles upon which the working of those appliances [used for the manufacture of sulphuric acid] depend.... I shall speak very little about the apparatus, no more than is necessary for a clear understanding of the rest; but I will as clearly as I can explain the purely scientific part of our subject.

It is clear that the principal purpose of the lecture was not to instruct his audience on the complex theories of the lead chamber process. It was written so that he could have the last word on the matter of the function of the Glover tower and to give him the opportunity to publish a simplified version of his work on the theory of the sulphuric acid process. He had realised that his three papers on the subject were too complicated to provide practical guidelines for use on a production plant, and that they had not been well-received in the industry for that reason.

He explained in detail the operations of the various components of the process plant and described improvements which had been made to them. Regarding the Glover tower, he took the opportunity to express his opinion, now without fear of contradiction, about the loss of oxides of nitrogen there:

> Opinions differ. I held at one time that a great portion [of the oxides of nitrogen] was lost in the Glover tower. Professor Lunge, on the other hand, maintains that none is lost in the Glover. Mr McTear thinks that it is partly lost in the Glover and partly in the chambers. I have recently made some more experiments on denitration and I find again that the decomposition of nitrososulphuric acid yields ... nitric oxide and ... nitrogen. But these experiments require repetition before valid conclusions can be drawn ... but there is no doubt that a reaction is going on somewhere in the system which reduces the nitrogen compounds to nitrogen or nitrous oxide - both of them valueless ....

His next comment shows that, however firmly established his opinions appeared to be on any matter, he was always receptive to further investigation of it. He was obviously leaving the door open for future research on the subject:

> But however wrong, so long as such views promote further inquiry, they are valuable, and we cannot look at a subject from too many sides.

Hurter concluded his lecture by returning to theoretical matters. He described the complex chemical reactions which took place in the lead chamber process and, in so doing, made a valuable contribution to the knowledge of the subject.

Hurter's simplified explanation of the lead chamber process given in his lecture was more favourably received in the sulphuric acid industry than were his original papers. Carey, President of the SCI, confirmed this in 1907 [5.43].

The lead chamber process was, for over a century, the only viable industrial process for manufacturing sulphuric acid. Its disadvantages were numerous and were early recognised: the conversion of sulphur dioxide to sulphur trioxide was slow; the lead chambers were difficult to construct, operate and maintain; the process was unsatisfactory economically; its chemistry was obscure; and its day-to-day workings were unpredictable. A simpler and more direct process was obviously desirable. Between 1831 and 1898 there were numerous attempts to invent a process for making sulphur trioxide (and thence sulphuric acid) directly from sulphur, but few were successful. Finally, in 1898, the contact process, which used platinised asbestos and later vanadium pentoxide as catalysts for the oxidation of sulphur dioxide to trioxide, came into general use.

Hurter had some involvement with an alternative to the lead chamber process. In 1871 Deacon was granted a patent which described the use of the catalytic convertor which he used for making chlorine (Chapter 3) to react sulphur dioxide with air and water to produce sulphuric acid [5.44]. He used the same catalyst: porous brick impregnated with copper sulphate. A heated mixture of sulphur dioxide, air and steam, was passed through the converter. Sulphuric acid condensed out of the exit gas. Although this invention was in Deacon's name, Hurter's laboratory note book shows that he carried out the experimental work on it [5.45]. There is, however, no record that the invention was exploited commercially, probably because Deacon was fully occupied with developing his chlorine process.

*   *   *

The lead chamber sulphuric acid process was inefficient, using crudely constructed plant which was difficult to operate and maintain. The nature of its reactions was not fully understood for many years. Sulphuric acid was vital to British industry during the nineteenth century and the optimisation of its manufacturing process was of the utmost importance. Gaskell Deacon were major manufacturers of sulphuric acid and Hurter was fully involved with this work. He made a number of significant contributions to the development and improvement of the process, including studies of the thermodynamics and reaction kinetics of the process and the operation of the Glover tower.

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CHAPTER 6

AMMONIA-SODA

The allegations by certain historians that Hurter played a part in the decline of the Leblanc alkali industry are fully discussed elsewhere in this thesis. Hardie implies that, because Hurter had spent much of his life working on the Leblanc process, he was prejudiced in its favour and was unwilling, even subconsciously, to admit that any other alkali process, such as ammonia-soda or electrolysis, might be better [6.1].

A study of Hurter's involvement with the ammonia-soda process shows that Hardie's implication is unfounded. Hurter was interested enough in it to work enthusiastically on his own version. Furthermore, during his period with UAC, the company operated ammonia-soda plants and as Chief Chemist he became involved with these.

From the 1870s the ammonia-soda process was the chief competitor of the Leblanc process for manufacturing sodium carbonate and, by the 1890s, had displaced it from its position as the principal process for manufacturing this in Britain. This competition gave the Leblanc industry the incentive to seek new products in the late nineteenth and early twentieth centuries.

Although the Leblanc process had been one of the most significant developments in chemical manufacturing, it possessed serious disadvantages: the sodium carbonate made by it was of poor quality, hydrogen chloride was a waste product which caused serious atmospheric and water pollution, and alkali waste (calcium sulphide) was a land pollutant.

Because of this, some alternative means of making alkali from salt was sought and the ammonia-soda process resulted. It made purer sodium carbonate and had fewer waste disposal problems than the Leblanc process. The ammonia-soda process is described in many publications (see Bibliography); a brief summary of it is given here, to assist in the discussion of Hurter's involvement with it.

Carbon dioxide is passed into a salt solution containing ammonia. Ammonium carbonate is first formed, which is then converted to ammonium bicarbonate. This reacts with sodium chloride to form sodium bicarbonate and ammonium chloride. The sodium bicarbonate separates out as a solid which is thermally decomposed to produce sodium carbonate.

The reaction was discovered by Fresnel in 1810 and several production processes were attempted between 1836 and 1858 [6.2 to 6.4]. None of these was technically satisfactory enough to be developed commercially.

As early as 1853 Henry Deacon developed a version of the ammonia-soda process, but it was not a commercial success and work upon it was discontinued. In 1873 Deacon wrote to Mond:

> I began to put up the apparatus for the ammonia-soda process at Widnes in October 1853, but this was not working until the summer of 1855, the month of July or August. I carried on for nine months producing at the end of this period about 2,000 kilos of carbonate per week [6.5].

Hurter first became involved with ammonia-soda at Gaskell Deacon in 1885, when he interviewed an employee who had been at the works when it first opened. His report on the interview gives details of the plant and process [6.6], as does a paper by Mond [6.4].

A manuscript by John Brunner confirms that Deacon was operating an ammonia-soda process *before* Brunner and Mond introduced Solvay's version into Britain.

> Mond came to me one evening to say that he had heard of Ernest Solvay's success in the use of the ammonia-soda process for converting salt into bicarbonate of soda, a process which both of us knew had been worked by Mr Henry Deacon of Gaskell Deacon & Co at Widnes [6.7].

In 1861 the Belgian chemist Solvay patented the special item of equipment - the Solvay absorption tower - for his version of the ammonia-soda process. His purpose was to dispose profitably of unwanted ammoniacal liquor from gas manufacture [6.8]. His invention was introduced into Britain by Brunner and Mond in 1874 under licence. Their enterprise became a commercial success due, to a large extent, to the ability of local chemical plant constructors to fabricate the cast-iron absorption towers which were used in the first stage of the process.

Brunner and Mond did not introduce the Solvay process without considerable opposition from the Leblanc manufacturers. A W Tangye, Manager and Director of Brunner Mond, wrote in 1900:

> In 1874, Mr Mond's chief opponent was Dr F Hurter, the "Arch Chemist" of the various Widnes chemical firms. His influence disappointed Mr Mond's hopes of introducing the Solvay process and taking over the Winnington property.

On the success and funding of Brunner, Mond & Co in 1880, I gathered from Dr Mond himself how Dr F Hurter had opposed him. Dr Hurter was considered the highest authority on the Leblanc process at Widnes etc, and Dr Mond was not able to get any of the big chemical owners - Gaskell Deacon, and Muspratts, as Chairman. He was very glad to get Mr J Crosfield [6.9].

Tangye believed that Hurter's influence in the Leblanc industry was important enough seriously to impede Mond's attempts to introduce the ammonia-soda process. Certainly, there was opposition from the Leblanc manufacturers, due to the threat from ammonia-soda to them, but the extent of Hurter's influence was probably exaggerated by Tangye. The set-back to Mond's plans was only temporary, in any case. Tangye's reports have been found to be confused and unduly subjective. They should therefore be viewed with caution.

It might be thought that, by the 1880s, the Leblanc manufacturers, realising that they had a serious rival in ammonia-soda, would have sought to diversify their production activities in other directions. This was partly true, but they also fought back, attempting to devise their own ammonia-soda process which would be different enough from Solvay's not to infringe his patent. Hurter was fully involved in the work.

The alternative process which was most favoured was to react ammonia and carbon dioxide with sodium sulphate. Weldon patented an experimental version in 1883 [6.10]. Compared to the Solvay process, which Mond described as "this remarkably simple and beautiful process" [6.4], this process was quite the reverse. It started in the same way as did the Leblanc process: sulphuric acid and salt reacted together to produce sodium sulphate (saltcake) and hydrogen chloride. Thus Leblanc's saltcake stage, which was a messy high temperature furnace operation, still had to be undertaken and also had to be preceded by sulphuric acid manufacture.

After purification, the sodium sulphate was reacted with ammonia and carbon dioxide to produce sodium bicarbonate and ammonium sulphate. The sodium bicarbonate was calcined to produce sodium carbonate. The ammonia was obtained either from gas-works ammoniacal liquor, or was recovered from the ammonium sulphate by reacting it with lime. When this was done, however, undesirable calcium sulphate was produced as a waste product. As an alternative, the ammonium sulphate could be sold as a fertiliser.

It is difficult to identify the advantages of this process over those of Leblanc or Solvay, and it seems unlikely that it would have been be able to compete with Solvay commercially. It may be that, because the sulphuric acid and saltcake processes were so well-established, the Leblanc manufacturers felt that they had a sound base from which to design a new process. This point was explained, if somewhat optimistically, by Weldon:

This invention consists in the manufacture of soda in such wise as to combine the advantages of...the Leblanc process with those of...the ammonia process.

The ammonia process yields soda very much more cheaply than the Leblanc process, but the Leblanc process yields not only soda but also hydrochloric acid, whereas the ammonia process yields soda only.

The ammonia process is based on the decomposition of chloride of sodium by bicarbonate of ammonia...If sodium sulphate could be decomposed by bicarbonate of ammonia, the production of soda from sodium sulphate would be immensely cheapened, while hydrochloric acid would at the same time be obtained from the sodium chloride commenced with, just as in the Leblanc process [6.10].

The principal advantage of the new process over that of Solvay was the fact that it produced hydrochloric acid which could be used to manufacture bleaching powder, a product essential to the prosperity of the Leblanc industry. Weldon might also have mentioned another advantage of the alternative process: it did not possess the problem of alkali waste.

Hurter developed his own version of an alternative ammonia-soda process, based on sodium sulphate, between 1883 and 1886 [6.11] to [6.15]. His patent states that:

... the reactions are effected in a manner similar to and in apparatus similar to that now employed....in the production of carbonate of soda from chloride of sodium by the employment of ammonia in conjunction with carbonic acid [6.11].

The important difference was that the sodium sulphate was reacted with ammonia and carbon dioxide in a series of tanks operating under pressure, whereas Solvay employed an absorption tower. Hurter's process produced less waste products than did Leblanc's, but a major problem, which he worked upon for a number of years, was the recovery and recycling of the ammonia without the need to react it with lime. Lime produced calcium sulphate which was a worthless waste product; furthermore, its removal caused the loss from the system of the sulphate ion which originated from the sulphuric acid.

The extensive research programme of the German chemist Blattner, published in 1885, demonstrates the widespread interest in processes for making soda from sodium sulphate [6.16].

In all the patent specifications for Hurter's ammonia-soda processes, details of the process equipment show that they were intended for full scale production. A semi-technical plant was constructed and operated at Gaskell Deacon's works at Widnes, but there is no record of a version having been operated commercially [6.17]. Tangye, writing in 1913, said:

In 1881, Dr Hurter realised he had made a mistake in condemning the Solvay process. He attempted to combine an ammonia-soda process with Leblanc .... Hurter put up an expensive plant at Gaskell Deacon, which failed utterly on [the following] points:

1. The difficulty of re-distilling ammonium sulphate (deposition of scale).

2. The habit of sodium sulphate wanting to crystallise with 10 molecules of water.

3. It wasted the sulphuric acid molecule as calcium sulphate.

However, this threat brought Mond back to considering the recovery of the wasted chlorine from the Solvay process.

Dr Hurter later spent quite a lot of capital attempting to make an ammonia-soda version work and failed. Dr Mond spoke of his contempt of Ferdinand Hurter to me more than once [6.18].

Tangye was a loyal employee and admirer of Mond; these comments and those quoted earlier indicate his dislike of Hurter. Hardie believed that Tangye was in error:

As far as [ICI's] records go, Hurter and Carey's experiments never passed the semi-technical stage and were only at that scale for a short time [6.19].

The conclusion is that Hurter pursued his research responsibly and that there was no unnecessary expenditure of capital. He did not proceed to full-scale production because he had demonstrated that this would not be profitable. Why then did he attempt to develop a process which he must have known possessed serious disadvantages compared to Solvay's? It seems likely that he was instructed to do so by Gaskell Deacon & Co, perhaps because the latter wished to test the competitiveness of an alternative ammonia-soda process, to commercially embarrass Mond, or to dispose of a surplus of saltcake or sulphuric acid. Whatever the reason, even if Hurter did not initiate the project, he carried out the work with his customary efficiency and dedication.

The interest of the Leblanc manufacturers in devising their own ammonia-soda process certainly annoyed Mond, even if he had no difficulty in dealing with any potential commercial threat from it. He believed (probably encouraged by Tangye) that Hurter was the principal driving force behind this interest although it seems unlikely that this was, in fact, the case.

This may be an appropriate point to examine the relationship between Hurter and Mond. They shared somewhat similar back-grounds: they were both German-speaking immigrants who had studied under

Bunsen at Heidelberg, although not at the same time. This may explain in part why Hurter followed Mond to Widnes when he left university.

Hardie suggests that, despite these common factors, there always was some incompatibility between the two men [6.20], but Roberts describes Mond and Hurter as "friendly rivals" [6.21]. Tangye recorded that Mond had a poor opinion of Hurter, "Mond spoke of his contempt of Hurter", but his evidence is subjective. Mond was known to have a volatile personality and his opinion of Hurter may well have varied from one occasion to the next.

Hardie further suggests that, when Mond left the Leblanc industry in 1873 to begin manufacturing ammonia-soda, he may have asked Hurter to join him [6.20]. If such an offer was made, Hurter certainly rejected it, but this may have been due more to his close involvement with the Leblanc industry rather than to any antipathy towards Mond. No other evidence of this offer has been found.

The main disadvantage of the ammonia-soda process was that it could not easily produce chlorine. In this respect the work of Hurter and others on an alternative ammonia-soda process did produce one interesting result. It stimulated Mond into looking into the possibility of making chlorine from the calcium and ammonium chloride which were the waste products of the Solvay process. He soon produced two experimental processes [6.22], but they were not particularly successful on a production scale. He did operate one of them for a time but more, perhaps, to annoy the Leblanc manufacturers than as a serious commercial proposition. Mond became obsessed with recovering this "lost" chlorine from the Solvay process and this obsession was to influence his decision not to adopt the Castner electrolytic process (see Chapter 11). Typically, he left the work of developing a suitable recovery process to others and moved on to other interests.

In the 1870s and 1880s Brunner, Mond & Co, operating the Solvay process under licence, were the largest ammonia-soda manufacturers in Britain. Surprisingly, in 1873, Solvay also granted a licence to a much smaller company [6.23], but this had limited commercial success because the royalty which Solvay required from that company was higher than that paid by Brunner Mond. Several companies operated "non-Solvay" ammonia-soda processes [6.24], but most of these were less efficient than Solvay's and their life was short. The firms were eventually either bought out by Brunner, Mond or became part of UAC. Independent ammonia-soda manufacturing did not infringe Solvay's patent, since this covered only the primary absorption tower; the ammonia-soda reaction was unable to be patented *per se*.

In his position as Chief Chemist of UAC, Hurter had considerable involvement with the manufacture of ammonia-soda. Neil Mathieson, an independent ammonia-soda manufacturer, became

part of the new organisation and continued manufacturing ammonia-soda until 1898. The fact that one of their component companies was an ammonia-soda producer was taken advantage of by the Board of UAC. The prospectus of the newly formed company stated:

> As this Company ... will possess plant for the manufacture of carbonate of soda by the ammonia process, it will be open to the Directors to ... extend the ammonia-soda process ... [6.29].

UAC began seriously to consider changing over entirely from the Leblanc to the ammonia-soda process. As a first step they entered into an agreement with Brunner, Mond, whereby they would be permitted to make a maximum of 15,000 tons of ammonia-soda per year, and Brunner, Mond no more than 165,000 tons of ammonia-soda, 3,600 tons of chlorine and no sodium hydroxide.

UAC began constructing a completely new ammonia-soda plant at Fleetwood. This opened in 1893 and was a great commercial success, quadrupling its initial annual production over the next thirty years [6.25]. Reports issued by Central Laboratory show that Hurter's staff were fully involved with the development of the Fleetwood plant, under his direction.

Referring to UAC's ammonia-soda activities, Lunge wrote:

> The increase in European opposition had been allowed for by the United Alkali Company and the growth of the ammonia-soda process was to a certain extent met by the erection of a plant at Fleetwood to meet the new competition and to replace with ammonia ash material hitherto produced by the Leblanc process [6.26].

In the long term, UAC's move into ammonia-soda benefited its prosperity and delayed the eventual demise of the company (see Chapter 2).

Hurter's work on an alternative ammonia-soda process had been suspended in 1886, probably because the investigation into its feasibility had served its purpose. At UAC, he became involved with the matter again in 1893, when he was associated with two patents [6.27], [6.28]. These were in his name, but the specifications state that they had been communicated to him by the Norwegian chemist, Johannes Omholt. It was probably intended that UAC might purchase the patent from Omholt, since a patent in Hurter's name automatically became the property of UAC.

As with Hurter's earlier inventions [6.11] to [6.16], the objective was to recover ammonia from the ammonium sulphate formed in the ammonia-soda/sulphate process. The ammonium sulphate was reacted with tri-basic calcium phosphate to form ammonia and sulphuric acid. The ammonia was recycled or recovered. A mixture of mono-basic calcium phosphate and calcium sulphate was produced which was intended to be used as an agricultural fertiliser, thus utilising the previously valueless

calcium sulphate waste. As with the work of 1886, though, there is no evidence that this process was ever developed to the production stage.

*   *   *

Although Hurter was primarily a Leblanc chemist, it cannot be claimed that his involvement with, and interest in, the ammonia-soda process was insignificant. He worked on the development of an alternative ammonia-soda process with his customary energy and efficiency. The fact that it did not prove to be worthwhile was, perhaps, to be expected, but the possibility had at least been properly investigated.

As Chief Chemist of UAC, he was fully involved with the production of ammonia-soda as the company proceeded with its plans to introduce it as a replacement for the Leblanc process.

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CHAPTER 7

THE ABSORPTION OF GASES BY LIQUIDS

One of Hurter's important research projects was a study of the methods used for absorbing gases in liquids. He was the first person to carry out a proper scientific evaluation of this operation, which had been employed in the chemical industry for many years. His objective was to provide guidelines for selecting the best method of carrying out a particular gas absorption operation, and for properly designing the necessary apparatus. There was a wide range of applications for gas absorption processes in the chemical industry, involving both simple dissolution and chemical reaction.

Early gas absorption processes were simple - the gas was bubbled through the liquid from an open-ended or perforated pipe. The efficiency of contact between the gas and the liquid was very low. One of the first examples of more sophisticated absorption equipment was the coke-filled tower which was developed by Gay-Lussac in France in 1827 for use in the lead chamber sulphuric acid process (see Chapter 5). At about the same time, the first gas absorption tower in the Leblanc industry was developed by Gossage for the absorption of hydrogen chloride gas in water.

Hurter's research was carried out between 1885 and 1893. Details of his experimental work are found in his laboratory notebooks [7.1], [7.2] and a full report is given in papers presented at meetings of the Society of Chemical Industry (SCI) [7.3] to [7.6]. These long and detailed papers ably demonstrate his practice of first considering the theory of a process; conducting a meticulous programme of experimental work in the laboratory; deriving further theoretical concepts from the results; carrying out trials on process plant and equipment; and performing process cost studies. The final result was the production of soundly-based guidelines for use in the chemical engineering design and optimisation of full-scale process plant.

When Hurter carried out his research, little or no guidance existed on the design of gas absorption equipment. That which he provided was based on sound physico-chemical principles. In adopting such an approach, Hurter was ahead of his time - a time in which chemical plant design was still largely based on empiricism. The methodology which he employed was of a standard which would be acceptable to chemical engineers a century later.

Hurter expressed the opinion that, although there was a good deal of published guidance available on the structural aspects of the design of a chemical plant, there was very little on its chemical engineering design. This had to be based largely on practical experience:

> ... we are left entirely to our fancy [and] to that second sight which is gradually and unconsciously acquired. But with the termination of the career of many a successful man terminates also his whole experience ... [7.7].

He believed that it was essential to preserve this experience in written form. Generally applicable design data, derived from sound theoretical considerations and, where appropriate, the results of properly conducted experimental work, should be made available to all interested parties.

A latter-day historian, Broad, commented:

> The brilliant Hurter was quick to apply the ... principles of physical chemistry to the proper design of ... absorption systems [7.8].

Hurter presented the results of his research in meticulous detail, so that others could obtain the maximum benefit from it. He was honest enough to admit that his researches had not entirely succeeded (he may have meant, had not lived up to his high expectations), but he hoped his results would encourage others. In this it is felt that he was being unduly modest, as this account will demonstrate.

He gave examples of different types of gas-liquid absorption operations: the saturation of a liquid with part or all of a single gas or gas mixture; the complete removal of one component of a gas mixture; the removal of a volatile component from a liquid by stripping it out with a gas; and heterogeneous gas-liquid reactions.

The basic principle of any gas absorption process was that the gas must be brought into intimate contact with the liquid, with the use of the least amount of energy. It was necessary to be able to measure the mechanical efficiency of the contact, so as to compare the efficiencies of different methods. Mass transfer between the gas and liquid phases depended on the extent of the contact surface, the number of molecules present per unit time (proportional to the gas pressure) and the time of contact. Hurter proposed a "unit of contact", which combined these factors:

> One unit of contact is the number of gas molecules which would impinge in unit time upon unit area of the gas-liquid surface if the gas exerted unit pressure [7.7].

The same unit of contact would be obtained by using a small contact area for a long time, or a large area for a short time. The efficiency of an item of absorption equipment was measured by the number of contact units which it provided for a given expenditure of work.

Hurter compared the efficiency of three common methods of gas-liquid contact: bubbles of gas passing upwards through a liquid; liquid droplets falling through a column of gas; and counter-current gas-liquid flow over an extended surface (e.g. a packed tower). The third method was in common use in the chemical industry, where it was widely recognised, but without scientific proof, that it was vastly superior to the other two.

Hurter described his study of the rising bubble method, in which small bubbles of gas passed upwards through a column of liquid. He calculated the energy required to overcome the head of liquid, over a range of operating pressures, and found that, although the method had a high energy requirement and was therefore inefficient, the equipment was simple to construct. It could have a useful application in special circumstances, e.g. where a fast chemical reaction was involved [7.3]. This emphasised that it was not sufficient to consider only the mechanical aspects of an absorption operation; to disregard the value of the rising bubble method where a chemical reaction was involved could lead to the rejection of a useful procedure.

He emphasised that, for most gas absorption operations, maximum efficiency was obtained when counter-current flow was employed. This was not possible where gas bubbles were rising through a liquid, or liquid droplets were falling through a gas column, and so it was unlikely that high absorption efficiencies would be achievable in these cases. Although Hurter's work on rising gas bubbles was carried out only in the laboratory, it had a practical value in that it assisted him in developing techniques which he could use to study other absorption systems. From the results of these studies he would then be able to proceed to his principal objective - the design and optimisation of full-scale process plant. One of his laboratory notebooks contains tables giving the power requirements, and pressure-volume diagrams, for two types of air compressor. He produced this data himself experimentally, demonstrating his abilities as a practical engineer [7.1].

A number of gas absorption processes involved a chemical reaction between the gas and liquid phases and Hurter's work on this was especially valuable, since there had been very little understanding of this subject before he studied it. He emphasised that it was essential to consider both the chemical reaction rate and the physics of the absorption.

He quoted as examples two processes which were widely employed; both involved gas bubbles passing upwards through a column of liquid. The first was the bubbling of carbon dioxide gas through a sodium hydroxide solution to produce sodium carbonate; the second was the regeneration of manganese dioxide in the Weldon chlorine process, in which air was bubbled through a suspension of

manganese monoxide in a solution of calcium chloride (see Chapter 3). Using theoretical calculations and practical experiments Hurter derived relationships between the height of the liquid column and the efficiency of absorption.

The method of contacting a gas with a liquid in which fine droplets of the liquid fell through the gas, had never been widely employed. Its low efficiency had been recognised, even if not scientifically proven, at an early date. Although Hurter felt that a study of it was scarcely worth carrying out, he decided to do so for the sake of completeness. His approach to the subject was semi-quantitative, and he was obliged to make some assumptions, but his results were adequate enough to prove his proposition.

He confirmed the low efficiency of the falling droplet method and attributed this to the high velocity of descent of the droplets through the gas; this resulted in short contact times between gas and liquid. He measured the work required to spray a liquid down through a gas in a tower. This was much greater than that required to blow gas bubbles through a column of liquid, because of the additional energy to raise the liquid to the top of the tower and to form the spray. This confirmed the unacceptability of the falling droplet method. Another, practical, drawback of this method was the difficulty of constructing a fine orifice for forming the droplets. Hurter was able to devise a successful means of doing this, by directing a fine stream of liquid on to a second stream of the same liquid.

Hurter's interest in the fine droplet method probably stemmed from his work on the operation of spraying water into the lead chambers of a sulphuric acid plant (see Chapter 5).

Although Hurter had proved in the laboratory that the falling droplet method was of low efficiency and possessed some practical difficulties, the opportunity arose for him to confirm his findings on a full-scale production plant. The Gay Lussac tower of a sulphuric acid plant had been emptied of its coke packing, and Hurter carried out an experiment in which a fine spray of acid droplets fell through a gaseous mixture of oxides of nitrogen. He found that there was no detectable change in the composition of the gas; the absorption of oxides of nitrogen by the acid was negligible; and the spray nozzles were prone to blockage. These experiments confirmed all his earlier conclusions about the falling droplet method.

Of considerably greater practical value to the chemical industry was Hurter's research on gas absorption in towers. He studied both the packed tower, e.g. the well-established and effective method of gas absorption - the coke tower, and the more sophisticated plate tower. He had already shown that the low efficiency of the falling droplet method was due primarily to the short time of contact between the gas and liquid. He believed that if this could be increased by passing the liquid over an extended surface, the liquid would form a thin film having a high surface area and the rate of liquid descent

would be greatly decreased. As a result, the efficiency of contact between the liquid and the gas would be greatly increased.

A common type of packed tower was one which contained lumps of coke. This material was cheap, chemically inert and its rough surface provided a high surface area for gas-liquid contact. It was not easy to obtain the actual flow rate of a liquid over the surface of a lump of coke, but experimental determinations were attempted. From these, an approximate value for the total surface area available for contact had been obtained. The assumption was made that a lump of coke was an impervious sphere; in practice the porosity of the coke provided a greater surface area than this.

It was found that a coke tower retained a large quantity of liquid for a considerable time. This was an advantage because it evened out fluctuations in liquid supply pressures, keeping the exit gas analysis constant.

Hurter also studied the distribution pattern of a liquid in a coke tower, from both single- and multi-jet distributors. This work was one of his most important contributions to chemical engineering; his results were still in use as late as 1952 [7.9].

One criticism of the coke tower was that the gas rose more easily through the larger voids in the coke bed, where there was also less liquid for contact in relation to the gas space than in the smaller ones. Hurter did not see this as a problem; he believed that the irregularities would average out, quoting the analogy of an electric circuit which had a number of current paths of differing resistances. He concluded that the coke tower gave a large amount of gas-liquid contact for a small expenditure of energy at a low cost. This confirmed the prevailing, but empirically based, opinion that it was one of the best gas absorption systems in use at that time.

Hurter commented on some alternative designs of counter-current absorption tower, including the Lunge-Rohrmann plate tower [7.10], [7.11]. He gave a long and detailed discourse on this, describing a comprehensive series of experiments and calculations which he had made on a plate tower on a sulphuric acid plant. He decided that there were a number of specialised applications where it was better than the coke tower, e.g. the processing of liquids containing suspended solids and as a reaction vessel to replace the lead chamber of a sulphuric acid plant. He believed that a plate tower could sometimes profitably be employed in series with a coke tower, but probably not in substitution for it.

Hurter derived a means of measuring and comparing the efficiencies of different absorption systems. The first component of this was the concept of contact ratio, which he defined as the amount of contact surface available per unit volume of gas per unit time. The second component was the work required to provide the same amount of gas-liquid contact.

When he considered the three types of gas absorption, he found that the work required to force gas bubbles up a column of liquid was ten times that required to lift liquid to the top of a coke tower. He

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had already shown that the falling droplet method used far more energy than rising bubbles, because of the energy required to form the spray of droplets. Combining "contact ratio" and "work required" for each of the three systems, he was able to prove that the packed (coke) tower was by far the most efficient of them. He advised that, if there were special circumstances where it was essential that the rising bubble method was used, then the cost of doing this could be greatly reduced by increasing the efficiency of the gas compressor used for producing the bubbles. This was the first time that the three methods of gas-liquid absorption had been properly compared on a quantitative basis.

Because of the widespread use and importance of gas absorption processes, Hurter's pragmatic approach to the study of them was well-received when he presented his papers. There was, at each presentation, a lively and informed discussion, which was very much in contrast to the response to his work on the theory of the sulphuric acid process (see Chapter 5). There was general agreement that the research was particularly valuable in clarifying matters which, although they had been recognised empirically for a long time, had never before been evaluated on a proper scientific basis. It was agreed that his work on packed towers would be of particular value when optimising the lead chamber sulphuric acid process.

In addition to the discussions, there was a detailed written response from Lunge, which contained some constructive and appreciative comments:

> ... it is a pity that *all* manufacturing operations cannot be evaluated with such mathematical scrutiny as Hurter, probably above all living chemical technologists, is able to bestow upon them. Hurter is always careful to compare his theoretical results with actual practice [7.12].

He did not, however, entirely accept Hurter's criticism of the falling droplet method and felt that further research might make this more efficient and applicable under certain circumstances. He went on to discuss his own sophisticated invention - the plate tower. This had better liquid distribution and gas dispersion than the coke tower and had numerous practical applications. It had received some criticism by Hurter, although he had praised its technical elegance. Hurter replied that his opinion on the subject was far from being finalised. He had not intended to criticise the plate tower; on the contrary, he had nothing but praise for it.

He concluded, honestly:

> I do not flatter myself that my work is perfect; on the contrary, I expect that such a beginning in establishing scientific principles of chemical engineering must and will necessarily be subject to corrections [7.13]

During the course of his research on the fundamental aspects of gas absorption, Hurter presented a stand-alone paper on the special case of the absorption of hydrogen chloride in water (known commonly but erroneously as "condensation") [7.6]. This had been of great importance for many years, being employed in virtually every Leblanc works. The paper is an excellent demonstration of Hurter's competence in the field of theoretical physical chemistry, coupled with his ability to produce guidance of practical value on a production plant. Because it deals specifically with one process, it is considered separately here, but it also provides information on Hurter's treatment of the theoretical and practical aspects of gas absorption in general.

In the early years of the Leblanc process, waste hydrogen chloride gas from the saltcake furnaces was simply allowed to escape into the atmosphere, causing serious air pollution. Factories on the continent were the first to address the problem by absorbing the gas in water in stoneware receivers (Woulfe's bottles). These were, however, because use of their mode of construction, unable to be scaled up so as to handle large volumes of gas. The problem was solved with the invention of what was probably the first absorption tower, by Gossage in 1829 [7.14] to [7.16].

Hurter had been well acquainted with hydrochloric acid condensation for many years, but admitted ignorance of its theory if, indeed, one existed at that time. If a proper theoretical explanation of the mechanism of the process could be derived, the process could be improved by determining the maximum concentration of hydrochloric acid which could be produced from a gas of known composition, together with the conditions required for achieving this. There was very little experimental data available on the absorption of hydrogen chloride in water, and Hurter produced an empirically based analysis of the mechanism, making use of the work of Hammerl, Bertholet, and Roscoe and Dittmar. Because their results were too theoretical for practical use, he developed and augmented them with experiments which he carried out both in the laboratory and on full-scale process equipment. He produced data on: the amount of heat produced during the absorption of hydrogen chloride in water; the consequent rise in temperature; and the effects of temperature and pressure on the absorption.

He was also able to provide an explanation of the commonly encountered phenomenon: that when hydrogen chloride gas escaped into damp air, almost half the water vapour in the air was precipitated as a fog:

> Any gas saturated with water vapour, but containing hydrogen chloride, the partial pressure of which is at least that given in the table, cannot hold more water vapour than that corresponding to the partial pressure given in the table under the heading "maximum tension [ie pressure below atmospheric] of water vapour" [7.17].

Combining his experimental work on heat evolution and on the concentration of solutions, Hurter produced tables for practical use when calculating the parameters relevant to the cooling of mixtures of hydrogen chloride and water vapour: the amount of heat evolved; the amount of hydrogen chloride absorbed; and the concentration of aqueous solutions of hydrogen chloride produced over a range of temperatures and pressures.

He then proceeded from theoretical considerations and laboratory experiments to full-scale trials using a forty feet high coke tower. He obtained an energy balance for the plant from which he concluded that, for maximum efficiency of absorption, effective cooling must be employed. It was essential that this was done in the absorption tower itself, since it was there that the greatest amount of heat was generated. Cooling the gas before it entered the tower, which was commonly practised, removed only a small proportion of the total heat and was scarcely worth doing.

He confirmed that the most important factors which influenced the efficiency of the process were: the time of contact between the liquid and the gas, and the surface area of the tower packing.

Hurter claimed that the practical value to the industry of his research on the "condensation" of hydrochloric acid was considerable, making it possible to optimise one of the most widely employed processes in the Leblanc industry.

In concluding his presentation, Hurter emphasised that the subject of hydrochloric acid condensation was not simple and that no-one had hitherto attempted to study it thoroughly, from both a theoretical and practical point of view. Hydrochloric acid was no longer the valueless product of forty years earlier; it was essential to the future prosperity of the industry. For maximum profitability, it should be recovered at as high a concentration as possible, with the expenditure of the least amount of energy.

He believed that his research would be of value in achieving this objective. If his tables were not quite perfect theoretically, their purpose was to abolish the need for complex mathematical calculations. No doubt in time his data would be improved upon, but he believed strongly that someone had had to make a beginning:

> I have now shown how by means of Roscoe and Dittmar's, Hammerl's and Bertholet's researches many problems connected with the cooling and condensation of gases containing hydrochloric acid can be solved arithmetically... The key to good condensation is not only to be found in the previous cooling of the gas, but also in the cooling of the absorbing liquid [7.18].

Probably because of the wide interest in hydrochloric acid condensation in the Leblanc industry, there was an enthusiastic response to Hurter's research. Herman referred to Hurter's "masterly paper" when describing his own invention of a hydrogen chloride absorption tower made from glass [7.19]. At an

SCI meeting there was a lengthy and informed discussion of Hurter's paper, the general consensus of opinion being favourable:

Dr Hurter had given a most valuable contribution to forming a theory of condensation of hydrochloric acid, and it seemed ... that he had not left much for anybody else to add [7.20].

At a later date Lunge endorsed this view:

We must be exceedingly thankful to Hurter for his erudite and difficult investigations [7.21].

* * *

Hurter's practice of thoroughly investigating a subject, to provide scientific proof rather than unsupported opinion, is well-demonstrated in his work on gas-liquid absorption processes. He produced practical guidelines which enabled them be operated efficiently and profitably. The work is of particular value in demonstrating his abilities in the relatively new discipline of chemical engineering. In the twentieth century, when the subject had reached a stage of mature development, his research (particularly on the distribution of liquids in packed towers) was still in use. He placed particular emphasis upon the fact that, in addition to studying the mechanism of an absorption process, it was vital to carry out cost calculations so as to optimise its economic viability.

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CHAPTER 8

PROJECTS CARRIED OUT AT GASKELL DEACON

Hurter's principal research projects are described in Chapters 3 to 7. It would, however, be wrong to exclude a number of other valuable contributions to the development of the chemical industry, some of which could well have merited chapters in their own right. They are described here so that full credit can be given to him for them, and the breadth and quality of his work can be fully appreciated. They were carried out by him whilst he was employed by Gaskell Deacon. This was his most prolific period for practical development work; his later activities, at UAC, are described elsewhere in this thesis.

From his earliest days at Gaskell Deacon, Hurter realised how empirical were many of the processes being carried out there. This strengthened his resolve to place the chemical processes of the time on a more scientific footing; in particular, by looking into ways in which reaction kinetics and thermodynamics could be applied to them.

In 1870 Hurter presented a paper at a meeting of the British Association on the relatively new subject of reaction kinetics [8.1]. His purpose was to address the question:

> A chemist may know that zinc and sulphuric acid react, but he would ask, "What time does the force [of affinity] need in order to complete this work [i. e. the reaction rate]?"

He developed, from first principles, a formula expressing the relationship between the work done in a chemical reaction and the time required to carry it out. He used this to calculate the rate of reaction between sulphuric acid and zinc, choosing this example because the reaction took a reasonable time to proceed to completion and was therefore easy to study. He derived a formula relating the amount of unreacted sulphuric acid in the reaction mixture to the time elapsed from the start of the reaction. He found this to be a simple logarithmic relationship: a first order rate equation. The formula was widely applicable and he verified it for the important industrial process, the recovery of manganese dioxide in the Weldon process. Trials on a production plant gave good agreement between theory and practice.

Hurter's rate equation was a substantial step forward in understanding the *practical* application of chemical kinetics, which was always his principal objective. Prior to his work on the subject, when

carrying out the design of a process plant full-scale trials had been required to discover how long a reaction would require to go to completion.

Hindsight shows that Hurter was not entirely correct in his understanding of the link between reaction rate and chemical affinity. He was right to identify the importance of knowing the rate of a reaction, but the affinity between molecules is not a guide to this. Thermodynamics predicts the final position of the equilibrium of a reaction, but gives no clue as to how rapidly equilibrium is attained. Although his work was based on a false premise, Hurter should receive credit for the innovation and value of studying the kinetics of a reaction in the laboratory and then applying the results to the design of a production plant. He established what is now a standard procedure in process plant design and he may be regarded as a pioneer in this respect.

Hurter's early interest in reaction kinetics may be in part responsible for his interest in photographic science. Much of his photographic research was based upon studies of the kinetics of photo-chemical reactions; his objective was to place the subject of photography on a proper scientific basis. In 1878 he stated in a paper on photography that:

> It is a general law of chemical kinetics that the increase of the products of a reaction tend to diminish the speed with which the reaction proceeds [8.2].

His interest in reaction kinetics continued throughout his life; one of his laboratory notebooks [8.3] contains calculations on the subject and in 1882 he published his research on the kinetics of the sulphuric acid process (see Chapter 5).

In 1877 Hurter published a paper on gas-solid reactions [8.4], in which he addressed a problem which had been identified with certain exothermic gas-solid processes, e.g. the Hargreaves-Robinson salt-cake process [8.5] to [8.11]. These suffered from inconsistent reaction rates, which were caused by lack of uniformity of particle size and temperature within the bed of solid material. This had led to some potentially successful processes being abandoned. When the solid phase was of uniform particle size, a steady gas flow resulted and a more even reaction rate was obtained. This had been achieved by forming the solid reactant into uniform-sized lumps, rather than using it in random sizes.

The problem of fluctuating temperatures in the solid bed was more difficult to resolve and this was the principal purpose of Hurter's research. In the Hargreaves-Robinson process, it had been found that the reaction proceeded more uniformly if the gas passed downwards through the bed of solid. Downward gas flow through the exothermic reactants gave improved gas mixing and heat transfer, resulting in a more constant reaction rate. Hargreaves had noticed differences depending upon the

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direction of gas flow through the reactor, but had considered the matter only qualitatively. He had concluded that with upward gas flow in a large diameter cylindrical vessel, the chimney effect accelerated the gas stream in the hotter parts of the vessel. Thus, locally, the exothermic reaction rate rose and the temperature increased still more, with the result that a small initial temperature difference escalated to a dangerously high one. With downward gas flow, localised over-heating was reduced because the upward convection of the gas, which was caused by the heat of reaction, controlled the flow rate of the feed gas to the reaction site and slowed down the reaction.

Whilst Hurter did not disagree entirely with Hargreaves' findings, he did not feel that his explanation was correct under all circumstances. He investigated, both in the laboratory and on a production plant, the conditions under which gas could flow upwards without adverse effects, analyzing his results mathematically.

As he had done in his research on bleaching powder manufacture, he studied the physics of gas flow through beds of coarse-grained solids over a wide range of conditions. He derived an impressive range of formulae which related gas velocity to temperature, for both open chimneys and vertical cylindrical reactors. Various grades of solid filling were tested.

The principal conclusion of Hurter's research was that the irregular rate of a gas-solid reaction was primarily due to varying temperatures within large reaction vessels. The solution was to heat the reactor evenly, and this was best achieved by passing the gas downwards. Furthermore, the solid material in the reactor needed to be of uniform particle size.

This valuable research project produced practical guidelines for use in process plant design. Hurter, with his sound knowledge of theoretical chemistry and mathematics, provided a scientific solution to a problem where, hitherto, only a qualitative explanation had been available.

He returned to the subject of uniform particle size in 1882 when, during a discussion on smoke abatement, he emphasised that, for the efficient combustion of a solid fuel, it should be of uniform size and, furthermore, that the velocity of the combustion air should be related to the size of the pieces [8.12].

Before the ammonia-soda process was introduced, Leblanc manufacturers had little incentive to manufacture high purity soda. The crude black ash which they produced contained only 20%-30% of sodium carbonate, as well as many trace impurities. Purer material was available on request, but it was much more expensive. The ammonia-soda process produced high purity soda and Hurter was involved in the efforts made by the Leblanc industry to counter this competition.

The poor colour of the soda which was manufactured by the Leblanc process was its greatest drawback. Hurter attributed this to iron compounds: a total iron concentration as low as 0.01% could cause serious discoloration [8.13]. He proved that sodium ferrocyanide was the principal discolouring

agent and that the source of iron compounds was the sulphuric acid used in the Leblanc process. Having discovered the reason for the discoloration, Hurter appreciated the need to quantify the problem. He developed methods for determining ferrocyanides in alkaline solution [8.14] and devised a number of successful processes for their removal, which he patented between 1879 and 1882 [8.15] to [8.18].

Although sodium carbonate was for many years the principal product of the Leblanc industry, in the late nineteenth century sodium bicarbonate also occupied a reasonable position in the market, particularly after the introduction of the ammonia-soda process.

Hurter's improvements to an existing process for manufacturing sodium bicarbonate were patented in this country in 1881 [8.19] and in the USA in 1883 [8.20]. He developed a variation of the process starting from anhydrous sodium carbonate, the principal product of the ammonia-soda process; he patented this in Britain in 1882 [8.21] and in the USA in 1883 [8.22].

The standardisation of methods for sampling and testing industrial chemicals was a matter which was of great concern in the nineteenth century chemical industry and one which engendered much discussion. Hurter's involvement with the subject took him from the alkali works laboratory into the broader arena of general chemical manufacturing.

Standard methods of analysis were already in place in the German chemical industry; many of these had been compiled by Lunge [8.23]. The German Society for the Protection of the Chemical Industry had asked Lunge to bring about an international agreement on sampling and testing, but the response to a questionnaire circulated by him in Europe had been poor and the matter had been shelved.

Hurter reported the results of the questionnaire to the SCI [8.24], beginning by asking:

Is it possible and is it desirable to have international standard methods of sampling and of analysis for the purpose of valuation of chemical products? .

One person who responded to the questionnaire doubted whether the English would introduce the metric system. Hurter did not see this as a problem.

I see no difficulty in that. If the results are expressed in a centesimal system, they are perfectly independent of the particular system of weights and measures which are in use. Percentages do not depend on cubic feet and pounds any more than upon litres and grammes.

The French would have to give up their Gay-Lussac degrees for bleaching powder.

He agreed with one comment received by Lunge: that it would be difficult to introduce standard methods for *sampling* chemical products, due to the wide variety of localities, accessibility, sampling personnel and packages.

Hurter recognised that a standardisation scheme was essential and suggested that the SCI was the appropriate body to set it up in Britain. Regarding the standardisation of analytical methods, Hurter foresaw many difficulties, particularly the wide range of units used. Also, the use in some instances of different atomic weights had led to confusion. He wrote a letter to a meeting of the SCI in January 1884, saying:

> After the various Sections of the Society have decided that the subject ought to be dealt, and can be dealt with, it should be brought forward by some competent person at the next Annual General Meeting. A representative committee should be appointed which should be placed in communication with the scientific bodies of the country, indicated in your circular letter of invitation. A joint committee thus formed should consider the question. No precepts nor directions should be given, and the committee should be free to take such steps as would conduce a satisfactory solution of the problem [8.25].

The meeting recommended that a committee be set up representing chemical manufacturers, consumers and analysts to determine the circumstances under which national or international agreement on sampling and testing was desirable.

In April 1884 Tate described the difficulties of sampling industrial chemicals [8.26]. Further meetings took place, but Hurter did not attend them. Finally, the Manchester Section of the SCI was given the task of drawing together the findings of the other Sections and in June 1884 it produced a set of weak recommendations [8.27]. There the matter ended, with a compromise solution which was far from what had been intended by the originators of the matter. The subject was not referred to in the Journal of the society again during Hurter's lifetime and was still unresolved as late as 1916 [8.28].

A major factor affecting the economic viability of chemical processes has always been the cost of heat, and the recovery and utilisation of waste heat is of the utmost importance. Hurter showed an early interest in the subject when he improved the thermal efficiency of the Deacon chlorine process by recovering the heat of reaction of the oxidation of hydrogen chloride to chlorine (see Chapter 3).

He carried out a comprehensive study on the utilisation of waste process heat and described his research at a meeting of the SCI, commencing his presentation with the words:

As an example, he studied the economics of utilising the waste heat from the flue gases of a black ash revolver, for evaporating a liquid. The first type of heat exchanger which he examined was a simple jacketed pan; the second type was known as Perkin's Pipes: a system of coils situated in the flue, through which liquid circulated by convection.

He found that neither system was an efficient heat exchanger. The jacketed pan had only a small surface available for heat transfer and, by its nature, it could not easily be improved. Perkin's apparatus had low heat transfer efficiency, principally because the liquid velocity through it was low. A considerable improvement could be obtained by a proper, theoretically based, design procedure. Hurter derived scientifically-based formulae and tables to enable this to be done and also provided heat transfer and cost calculations. He ended his presentation optimistically, prophesying that the tube-type evaporator would, in time, become successful. This proved to be true since, by the end of the century, efficient evaporators were in use in a number of industries. The comprehensive data provided by Hurter would have been invaluable in developing them.

Hurter's pioneering work on heat transfer was some of the earliest which was carried out. He used heat transfer calculations widely when studying not only the processes of the Leblanc system, but also others which were of more general application throughout the chemical industry. Heat transfer computations are routine procedure in chemical process design and optimisation today, but their use was unusual and innovative a century ago. By the early 1890's, Hurter was designing heat transfer equipment to a standard which would not have been out of place many decades later.

Hurter's facility with the more academic aspects of the thermal efficiency of processes was ably demonstrated in a long and learned paper which he presented as Chairman of the Liverpool Section of the SCI [8.30]. Based on a detailed consideration of their thermodynamics and thermo-chemistry, he compared the thermal efficiencies of the three alkali processes: Leblanc, ammonia-soda and electrolytic. He analyzed the energy requirements for an alkali process into four components, demonstrating how these varied for each individual process. These were: (a) the heat required to raise the reactants to the temperature at which the reaction proceeds; (b) the energy required to perform the reaction of splitting sodium chloride into sodium and chlorine; (c) the heat lost in cooling the reaction products to ambient temperature; (d) general heat losses.

In the three alkali processes, (b) is the same for all of them, but (a), (c) and (d) are different; it is these differences which dictate the thermal efficiency of each process. He concluded that the Leblanc process (including sulphuric acid manufacture) involved a number of high temperature stages, therefore

(a), (c) and (d) were high. It was therefore inefficient compared to ammonia-soda and electrolysis, which operated at 100C and ambient temperature respectively. In making the comparison, he emphasized that he had not overlooked the fact that the electrolytic alkali process did, indirectly, include a high temperature stage: the generation of electricity by burning a fossil fuel.

Hurter clearly recognised that the electrolytic alkali process possessed the advantage over the other two processes on the basis of heat requirements, but pointed out that it was seriously disadvantaged at that time by the high cost of electrical power. It would not be able to compete with them until this cost was reduced. Hurter's views on the technical and economic viability of electrolytic alkali processes are discussed in detail in Chapter 10.

These are just two examples, one practical and one theoretical, of Hurter's interest in thermo-chemistry and thermodynamics. They show that he never lost sight of his practical objective of improving the efficiencies of chemical production processes. Other examples can be found in his laboratory notebooks [8.31]. When he was a student at Heidelberg, Hurter had the good fortune to study under Kirchoff, a pioneer in thermodynamics. This could have been the reason for his life-long interest in the subject, an interest which may have led to his producing the draft of a book on thermo-chemistry and thermodynamics (see Chapter 12).

In 1884 Carey and his employer, Holbrook Gaskell, patented an invention for manufacturing sodium sulphite by reacting crystalline monohydrated sodium carbonate with steam and sulphur dioxide [8.32], [8.33]. Details of this had been communicated to them by Hurter "at present residing in Paris, France". This statement is a mystery, since it has not been possible to discover what Hurter was doing in France at that time. As far as can be ascertained, Gaskell Deacon had no close connections with that country. One possible explanation is that Hurter was writing from an accommodation address in Paris for the purpose of obtaining patents for the process in both England and France. An alternative explanation is that, because the words "partly communicated" were used in the patent, it had been prepared jointly with an anonymous Frenchman.

Hurter's talents extended further than laboratory chemistry and chemical plant design; he also exhibited a gift as a practical inventor. In addition to his extensive chemical research at Gaskell Deacon, he was responsible for developing a number of devices for carrying out process measurements, which could be used generally throughout the chemical industry.

At meetings of the SCI he demonstrated slide rules which enabled semi-skilled process operatives to carry out relatively complex computations [8.34] to [8.36]. The first of these was used for correcting gas volumes for temperature and pressure when carrying out technical gas analyses [8.34]. The second was used in conjunction with a calorimetric method which Hurter had invented for measuring furnace

temperatures. The paper in which it was described also contained a comprehensive review of the existing devices for measuring high temperatures, some of which had been improved by Hurter [8.35], [8.36].

He also designed two special anemometers; these are described in Chapters 3 and 7.

<p align="center">*   *   *</p>

The projects described in this chapter demonstrate Hurter's wide ranging abilities in a variety of disciplines: reaction kinetics, fluid dynamics, chemical analysis, process plant design, thermochemistry, thermodynamics and mechanical invention.

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CHAPTER 9

HURTER AS CHIEF CHEMIST OF UAC

A major milestone in Hurter's career was his appointment as Chief Chemist of UAC. His first and most important task was to set up their Central Laboratory. Before UAC was formed, research and analytical work had been carried out, to a greater or lesser extent, by the individual companies which comprised it. All these probably had some laboratory facilities for testing raw materials, controlling processes and for quality control of products. Work might also be carried out on developing new products; seeking potential uses for them; improving the quality of established ones and optimising the efficiency and profitability of the various manufacturing processes.

In the early part of the nineteenth century, British industrialists had tended to be unreceptive to the concept of properly organised process research and development, relying much more on *ad hoc* and empirical approaches to this. By the second half of the century, a number of leading figures in the chemical industry had received their university education at continental universities (e.g. the Muspratts at Giessen and Hurter and Mond at Heidelberg) and the strong research tradition which existed at those places began to be seen in Britain [9.1].

The Board of UAC broke new ground in realising that the company's future prosperity depended upon a centralised, high quality laboratory organisation. This should be capable of carrying out systematic research to discover and develop new processes, as well as routine and specialised analytical work. They allocated the highest priority to the implementation of its provision [9.2].

The work could, of course, have continued to be done in the laboratories of the individual firms which comprised UAC. This would have been far less efficient, however, since the laboratory facilities in these companies varied from very good to virtually non-existent. There would, moreover, have been the difficulty of providing centralised control and co-ordination of the work.

The choice of the most appropriate person to head the new department was of utmost importance. The Board's decision to offer Hurter the post, at the age of forty seven, was to herald the second phase of his professional career.

UAC was formed on 1 November 1890, and the directors lost no time in confirming Hurter's appointment. The Board Minutes for 19 December 1890 record:

It was unanimously agreed that Dr Ferdinand Hurter of Widnes be appointed head of the laboratory department for the term of five years and that his salary be £1000 per annum, subject to the condition that any discovery he may make which is the subject of a patent shall be at once assigned to the Company [9.3].

The latter condition was soon extended to include all employees of the company [9.4].

A salary of £1,000 per annum was substantial for those times and it reflected the Board's high opinion of Hurter's abilities. It was increased to £1500 per annum from 1 January 1893 [9.5]. This compared well with other senior management salaries in the organisation. For example, J R Wylde, full-time Managing Director of the UAC St Helens District had a salary of £1250 per annum, plus a profit conditional bonus of £250 [9.6].

For all his abilities, Hurter seemed a little unsure about accepting the offer made by the Board. He wrote to George Lunge:

I was tolerably pleased with my position [at Gaskell Deacon] and could look forward with confidence, [but] now I must serve unknown masters [9.7].

From the tone of this letter, it seems unlikely that Hurter had carried out any lobbying for the post; indeed, he appears somewhat surprised at being approached by the Board of UAC. Why then was this offer made to him?

There is little doubt that, among all the companies comprising UAC, there was no person better qualified for the position than Hurter. The reputation which he had built up in the Leblanc industry over the preceding twenty three years spoke for itself. Also, he probably benefited from the patronage of a number of prominent people in the newly formed company. Several of the partners in Gaskell Deacon became directors of UAC and they would have possessed a good deal of influence with the Board, since Gaskell Deacon was one of the largest of the companies which comprised UAC [9.8].

Hurter would also undoubtedly have had the support of Edmund Muspratt, Honorary President of UAC at the time of its formation and one of the leading figures in the alkali industry [9.9], [9.10]. Edmund had known Hurter for about twenty years. They were founder members of the SCI and they both served as Chairman of the Liverpool Section. Hurter supported Muspratt in his election campaign of 1885, when he stood as parliamentary candidate for Widnes. Muspratt would have possessed great influence with the directors of UAC and this would have greatly assisted Hurter's appointment.

Despite his misgivings, Hurter accepted the Board's offer and lost no time in embarking with his customary thoroughness and enthusiasm on the exciting challenge which had been offered him - to establish the first laboratory organisation of its kind in Britain. Recognising that he had been given a virtually free hand and the full support of the Board, he made the most of his opportunity. The first task

which he set himself was personally to survey all the laboratories in the component companies of UAC, to see if he could select one in which the new department could be set up. On 19 February 1891 he reported to the Board that all the existing laboratories were too small for the work which he expected to be carried out. He described his understanding of what this would entail:

I conceive of the duties of this laboratory to be:

Original research in general, to promote the interests of the Company.

Investigations of processes offered to the Company for sale.

Investigations of all patents referring to the alkali and allied trades, whether offered to the Company or not.

Investigations into all inventions made by servants of the Company for the purpose of verifying results, and obtaining suggestions for the framing of both provisional and final [patent] specifications.

Analytical work, in connection with short methods for routine testing in the works, in connection with complaints made by customers, to check the results of the works chemists, and to investigate differences between them and public analysts [9.11].

He did not envisage Central Laboratory as being simply a research department, but rather a centralised laboratory facility having a variety of functions. In fact, the fundamental research element did not prove to be large during the period of Hurter's management. The facilities which he specified for the new department were, by the standards of the chemical industry at the time, ambitious and lavish, but they were certainly justified, in view of the brief given to him by the Board of UAC. He stated his requirements in detail:

A library for patents and journals.
Consulting room and private laboratory for the head of department.
General laboratory.
Weighing room.
Physical instrument room for electrical and optical experiments.
Gas analysis room.
Constant temperature cellar.
Store room for chemical and apparatus.
Cloakroom, lavatory and closet.

A new site was obtained for the complex, between the Widnes Baths and Gaskell Deacon's works - as close as possible to Hurter's erstwhile employers! Hurter remarked in his report:

I know of no space so central as this.

He requested that:

I [may] be allowed to visit the various works to find out what specifications, journals, etc they have .... so that I may complete....a good patent library embracing chiefly the alkali trade. The library of Gaskell Deacon forms a good beginning.... We should then be in a position to make our own patent searches.

He insisted that all the laboratory staff should have scientific training, with at least one of them having experience of alkali manufacture. There should be a deputy to himself, who should be a qualified chemist.

Hurter's report to the Board of UAC reveals a great deal about him. He had only been in post for two months, but he had quickly demonstrated the efficiency with which he was capable of assessing a task and commencing work on it. His energy and innovative abilities soon produced his specification for the new department. This embodied concepts which were well ahead of those obtaining generally in the British chemical industry at that time. Particularly impressive is his insistence on a comprehensive library, which would afford good data retrieval facilities. This is the essential pre-requisite of a laboratory organisation today, but it would have been an innovation then.

The new department, formally known as Central Laboratory, was quickly completed and came into use in 1892. It was one of the first of its kind in Europe and was as well-equipped as any university laboratory of the period [9.12]. It was unusual in that it was a laboratory which was established by an industrial concern for the purpose of carrying out technical research, at a time when academically based research workers (in Britain at any rate) had little interest in industrial matters. There were a number of industrial laboratories in the USA which had a similar function to the Central Laboratory [9.13].

A comprehensive picture of the work of the laboratory can be obtained by an examination of its original report books. Thirteen of these, covering the period from 1891 to 1927, are preserved in the Cheshire Record Office [9.14], [9.15]. They give details of every project carried out. The six which cover the time when Hurter was in charge have been studied in detail. The number of projects carried out over this period averaged forty four per annum.

The reports show that Hurter was not just the administrative head of the department; he carried out a good deal of experimental work himself, using his own special laboratory which he had included

in his original specification for the department. Interestingly, during the first year of operation of the laboratory, Hurter signed all reports himself, even those which were obviously prepared by someone else. Later he permitted the authors of reports to sign their own work. The report books reveal the wide-ranging nature of the work, from routine analyses of samples of coal, to full engineering and economic appraisals of new chemical processes. It fell into the following categories:

Routine and specialised analysis.

The evaluation of new processes, either originated within UAC or offered to the company by independent inventors (sometimes only as an imprecise idea, upon which very little practical work had been carried out) for development following its purchase, or in connection with the granting of a licence. Every invention was carefully appraised, with experimental work being carried out if this was deemed necessary. The report books demonstrate the degree of care which was taken when assessing the potential of a proposal.

The examination of patent specifications with a view to UAC's adopting them under licence, and the preparation of patent applications for inventions made within the company.

The invention of new processes and their development from the laboratory to a full-scale production plant.

The optimisation of existing processes operating within UAC.

A study of the 297 reports produced by Central Laboratory whilst Hurter was Chief Chemist (January 1891 to May 1898) reveals that there was very little work done which could be described as pro-active original research, either pure or applied. The great majority of the projects carried out were reactive, despite the fact that Original Research was the first objective which Hurter had originally specified for the new department. There was only a small number of original inventions, e.g. Hurter's chlorate process and the successful Raschen cyanide process.

This absence of pro-active work from the laboratory's programme is at first sight puzzling, since Hurter had virtually a free hand to direct its activities as he wished. He could perhaps in hindsight be criticised for this failure to carry out such work, but the following explanation for this is suggested.

In setting up the centralised laboratory organisation, Hurter based its activities on what was already being done in those individual works laboratories which it replaced: the type of work upon which he himself had been engaged throughout his period with Gaskell Deacon. Valuable as this was

to the prosperity of the Leblanc industry, it was, in the main, reactive in nature. Laudable as the decision of the Board of UAC was to set up a centralised laboratory organisation, it does not appear to have occurred to them to insist that original research should be an essential part of its activities.

Because Hurter's appointment was novel for the times, he was probably regarded as Chief Analyst of UAC (a recognised position in the industry) as much as he was Head of Research. As a result, he continued carrying out the type of work to which he had been accustomed. This should not, of course, be under-valued, but the department would have been more valuable to UAC had its activities included a higher content of original research. One example is the subject of electrolytic alkali processes, upon which Hurter possessed a great deal of knowledge and experience, much of which he obtained from studying other people's processes (Chapter 10) but, surprisingly, he never produced one himself.

In contrast to a large industrial organisation today, there was no representation at Board level of research interests: the position of Research or Technical Director did not exist. The head of a modern industrial research facility would probably be a Board member. His duties would include advising the Board on long-term development strategy, for as much as ten years into the future. At the time of Hurter's appointment this would have been inconceivable and well outside his sphere of responsibility. Although obviously highly regarded, Hurter was still only a salaried employee (a strictly defined position in the nineteenth century) and would have not been able, or have expected, to have his opinions heard at Board level, unless he was required to report on a specific subject.

Another reason why the capability of the staff of Central Laboratory to devise and develop new processes was not more fully exploited was the generally unsatisfactory financial state of UAC throughout the 1890s. The resources to develop new ideas up to full production scale were limited; as a result many were not proceeded with.

In his history of the British alkali industry, Warren exhibits an equivocal attitude towards Hurter's performance as head of Central Laboratory. Whilst praising his abilities:

Hurter [was] a chemist of high ability...

He suggests that Hurter had not run the department as successfully as he might have done:

Hurter was devoted to his work [as head of Central Laboratory] but was hampered by two factors: an apparent built-in antagonism to .... Ludwig Mond, and perhaps too great a deference to UAC's rather timid management [9.16].

These remarks are unjust to Hurter. When Hurter was Chief Chemist of UAC, Mond was no longer closely involved with the alkali industry. More importantly, he had never had any connection with

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UAC, or been closely associated with Hurter's work. It is difficult to understand, therefore, why any feelings of antagonism by Hurter towards Mond (if indeed there were any) could have affected his management of Central Laboratory.

If Hurter did, in fact, show some deference towards UAC's senior management, this was not unusual, since this would have been implicit in his status in the company in those days. The question as to whether UAC's management was timid, conservative, or simply cautious in an alien economic environment, is discussed fully in Chapter 11.

When developing a new process at Central Laboratory, the benefits of proceeding to the design of the full-scale production plant via a pilot plant stage were soon appreciated. Many of the pilot-scale and full-scale plant trials were carried out at the adjacent Gaskell Deacon works, sometimes using plant and equipment already available there. New processes which were investigated included the manufacture of acetic acid, acetone and carbon tetrachloride. Existing processes, such as bleaching powder manufacture, were systematically evaluated, with a view to improving them.

In 1896 Hurter gave an account of the work of Central Laboratory to a special sub-committee of the London County Council [9.17].

A few examples have been selected to demonstrate the quality and type of work done and to show how a discovery in the laboratory was developed up to full-scale production. They are fully described in the laboratory report books [9.14], [9.15]. Certain major projects, e.g. Hurter's work on the ammonia-soda process, and his long-standing research on the theory of electrolysis and on electrolytic processes are discussed elsewhere in this thesis.

The manufacture of acetic acid

This involved the assessment of Von der Linde's process for manufacturing acetic acid. Calcium acetate was reacted with sulphuric acid, the acetic acid formed being distilled off under vacuum. The use of vacuum resulted in a lower operating temperature, and so an impure form of calcium acetate could be used. After rigorous evaluation and some commercial negotiations, the process was purchased by UAC and a full-scale plant built. With the incorporation of a number of modifications, the plant operated as a commercial success until 1930.

The Raschen sodium cyanide process

The cyanide process for the extraction of gold had been discovered in 1886 and required a massive supply of sodium or potassium cyanide. This presented UAC with an attractive commercial

opportunity, which they lost no time in exploiting. After a great deal of research, Hurter's department produced the highly successful Raschen process for manufacturing cyanide from carbon disulphide and ammonia, both of which were readily and cheaply obtainable as by-products from coal gas manufacture.

In 1894 a pilot plant was erected at Gaskell Deacon's works. Following the success of this, a small scale production plant, capable of manufacturing 1.5 tons of cyanide per week, was constructed [9.18]. The lessons learnt on these plants were so encouraging that, in 1897, the Board sanctioned capital expenditure of £7,000 to erect a full-scale plant at Runcorn [9.19]. This was a commercial success and, despite being destroyed by fire in 1901, was speedily rebuilt and remained in production until 1908.

Related to the above research was the development of a process for converting an alkali sulphocyanide to the corresponding ferrocyanide.

The manufacture of acetone

Hurter's last patent application, which was accepted nine months after his death, described an improvement to an existing process for manufacturing acetone, in which acetic acid vapour was passed over an alkaline earth carbonate at high temperature. Hurter's improvement was a demonstration of his practical ingenuity, making use of his early work on the Deacon chlorine process. He proposed that the carbonate be deposited within the pores of fragments of pumice, brick, etc by impregnating the latter with a solution of the alkaline earth acetate. After drying, the impregnated porous support was heated, when the acetate decomposed to form the alkaline earth carbonate. This was used to decompose the acetic acid vapour to acetone. The concept was described in the patent, which also includes instructions for constructing and operating the plant. A full-scale plant was built, appropriately, where Hurter had begun his professional career and had spent most of his working life. Had he lived to see it, Hurter would certainly have been proud of his achievement. By 1902 acetone was one of UAC's major products; production rose from 3,000 tons per annum in that year to a maximum of 4,000 tons in 1911.

The original staff of eight at Central Laboratory increased slightly during Hurter's period there. A photograph taken in 1897 shows eleven members, including the clerk and the storekeeper/handyman [9.20], [9.21]. It might be suggested that a staff of only eight was unimpressive by present day standards, for a major laboratory facility. It should not be forgotten, though, that Hurter's principal achievement was in the original conception and establishment of the department. Even in its early years his modest cadre of chemists produced an impressive amount of high quality work, much of it directly

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applicable to UAC's manufacturing operations. The records of Central Laboratory show that it was a well-managed and efficient organisation; the laboratory buildings and facilities, as conceived by Hurter, were well ahead of their time.

From its modest beginnings, Central Laboratory developed into a major scientific and technical facility which was of great value to UAC and to its successor, ICI. By the mid-twentieth century, it had evolved into the research department of ICI General Chemicals Division, with a staff of some 700. It was then re-named the Hurter Laboratory, in honour of its founder.

Hurter's establishment of Central Laboratory received praise from both contemporary scientists and industrialists and latter day historians. Their approval of it seems to have been based more on the fact that it was one of the first *centralised laboratory organisations*, rather than a *research facility*. There was one dissenter, however. Professor R S Hutton, writing in 1964, remarked rather unfairly that:

Another example of industry's backwardness in appreciating the value of science graduates was that of the United Alkali Company, although it employed 12,000 workers, only started its research laboratory under Hurter in 1892 [9.22].

He implies, wrongly, that UAC had been in existence for a long time before creating Central Laboratory. In actual fact, the decision to do this was taken only a few weeks after the company was formed: in December 1890. Twelve months later it had been designed, constructed and was fully operational.

The last of Hurter's reports was dated 2 March 1898, just three days before he died. It is a copy of an untypically angry letter which he wrote to UAC's Chief Engineer, E J Duff, who was in the USA working on the layout of the production plant at UAC's new factory there. Hurter had recently returned to Britain, leaving Duff in charge of the project and the latter had agreed, against Hurter's wishes, to an unsatisfactory and unworkable plant layout. Hurter wrote:

You ... are not sufficiently acquainted with the process to be able to take liberties with my plans.... It is a fault that there is no one with you who understands what we really want for the process [9.23].

The directors of UAC maintained their high regard for Hurter throughout the period of his employment with the company. After his death on 5 March 1898, on a day when he had earlier been at work in the laboratory, they recorded:

The Board desires to record their deep sense of the great loss that this company has sustained in the death of Dr Hurter, their high appreciation of his character, his great attainments and his exceptional ability.... [9.24].

*   *   *

Hurter's appointment as Chief Chemist of UAC was a watershed in his career. He was set the task of establishing and managing the UAC Central Laboratory and he rose to the challenge with energy and enthusiasm. For the first time he was able to demonstrate his managerial and organisational abilities. He formed an organisation which was not only widely admired at the time, but eventually evolved into one of the leading industrial research establishments in the world.

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CHAPTER 10

ELECTROLYTIC PROCESSES

The allegation by certain historians that Hurter played a major part in the failure of the Leblanc alkali industry in the 1890s, by advising the Board of UAC not to adopt Castner's electrolytic alkali process, is examined in Chapter 11. He was accused of not being properly informed about electrolytic processes and of ignoring their commercial potential out of misplaced loyalty to the Leblanc system. This was far from being the case. His characteristic enthusiasm for new scientific matters sustained his interest in this new branch of industrial chemistry from its early days in the 1880s, when he was employed by Gaskell Deacon, through his period as Chief Chemist of UAC, until his death in 1898.

His work on the subject was both wide-ranging and detailed. It included laboratory experimentation on electrolytic cells, theoretical papers on electrolysis and assessments of new electrolytic processes based on their reaction kinetics, thermodynamics and economics. From 1885 up to the year of his death, he produced numerous publications and patent specifications on electrolysis and invented a highly successful electrolytic process for manufacturing potassium chlorate.

The earliest example of an electrolytic process which would have interested Hurter was that of Faraday who, in 1834, electrolysed salt solution to produce chlorine and sodium hydroxide. A patent based on Faraday's discovery was taken out by Watt in 1851, for manufacturing alkali metals, copper, silver, gold and hypochlorites and chlorates [10.1]. This invention was well ahead of its time, but it was unable to be developed industrially for some thirty years.

Hurter was one of the first workers in the Leblanc industry to carry out research on electrolytic processes. His first patent on the subject, which was produced jointly with Elmore, an electrical engineer, was for a process for purifying alkaline sulphides [10.2]. It was related to research which he had carried out to achieve the same objective by chemical means.

Hurter's involvement with an electrolytic process invented by Hermite provides valuable information about his opinions on the feasibility of such processes, at an early stage in their history. In 1887 he criticised [10.3] a paper written by Cross and Bevan [10.4] about Hermite's electrolytic bleaching process. This process [10.5], [10.6] produced sodium hypochlorite, which was proposed as a replacement for bleaching powder in the event that the ammonia-soda process caused the demise of the

Leblanc industry. Cross and Bevan claimed that the bleaching efficiency of Hermite's sodium hypochlorite solution was much greater than that of a solution made from bleaching powder.

Hurter had been carrying out research on electrolytic processes for some time and had become quite knowledgeable about the new technology. He stated that:

Some years ago ... I made a series of experiments on the electrolysis of common salt, which led me to the general conclusion that for the production of articles of low price [e.g. alkali] electrolysis as a manufacturing operation was impracticable. Even for such an article as chlorate of soda, the electrolytic preparation of which presents no difficulty, I found the process too expensive, the efficiency of the current being extremely low [10.7].

He expressed some reservations about Cross and Bevan's claims and, because they did not agree with his experience, carried out an investigation of the primary and secondary reactions of the Hermite process, designing the appropriate apparatus for doing this. His results disagreed with theirs and he accused them of not carrying out their research properly. Had they done so, they would have found Hermite's process to be far less efficient than it appeared. He remarked, somewhat unkindly:

We are told that the proprietors of the patent-right are fitting up a research laboratory. Let us hope they will have no cause to regret having acquired the patent-right first and fitted the laboratory last [10.8].

In a paper which they presented in the following year, Cross and Bevan refuted these criticisms of their work [10.9]. During the discussion of their paper, Hurter pointed out that their results were wrong, partly because they had overlooked one essential detail: the provision of a stirrer in the reactor, which increased the conversion efficiency. He implied that they should have investigated Hermite's process more thoroughly before publishing their paper, as he would most certainly have done. He concluded, rather testily:

With these remarks I close a discussion to which I cannot devote any further time and attention [10.10].

The Hermite process is of little interest scientifically, but Hurter's involvement with it provides the following valuable information:

This was the first time that Hurter stated that it was unlikely that an electrolytic process for manufacturing alkali could become economically viable.

Even in the early days of electrolytic processes he possessed considerable knowledge and experience of them.

Before commenting on research carried out by others, he was always ready to carry out detailed and thorough experiments of his own, rather than to give an unsubstantiated opinion.

He had little regard for individuals who published scientific work without carrying it out properly and thoroughly.

During the 1880s there was growing interest in an electrolytic process for manufacturing alkali from salt. By the early 1890s it was apparent that such a process would become available before long and that it could threaten the Leblanc and ammonia-soda industries. UAC recognised this from the first days of the company's existence; it was fortunate therefore that it was able to procure the services of Hurter as its Chief Chemist, giving them access to his extensive knowledge of electrolytic processes. This was acknowledged as being superior to that of any other person in the alkali industry at the time, and it was of inestimable value in assisting the company in deciding whether an electrolytic alkali process could be a viable possibility.

Hurter had expressed a keen interest in an electrolytic alkali process as early as 1888, despite his many years of association with the Leblanc process. When Lunge delivered the first Hurter Memorial Lecture, the year after Hurter's death, he remarked:

It is characteristic of Hurter that, when he had once recognised the importance of electro-chemistry, he threw himself into that domain with as much ability as energy, and made contributions even to the theoretical side, although his situation as chemist to a private firm prevented him from publishing the results of his work as far as manufacturing interests were concerned.

The zeal which Hurter latterly developed in the cause of electrochemistry is one of the cases in which he overcame a certain tendency to conservatism in technological questions [10.12].

Surprisingly, Hurter never invented an electrolytic alkali process himself, the probable reason being simply that he was never instructed by his employers to do so.

An important and much-quoted contribution by Hurter on the subject of industrial electrolysis was his Chairman's address to the Society of Chemical industry in 1888 [10.11]. This long and comprehensive paper described a quantitative study of the efficiency and profitability of electrolytic processes. In particular, he compared the electrolytic alkali process with Leblanc and ammonia-soda.

He reviewed the development of electrolytic processes, from Cook's invention of a porous diaphragm in 1851 up to that of Marx in 1887. He remarked, somewhat sarcastically:

> ... from the year 1851 to 1887 we have thus this vast progress, that while William Cook describes a method of producing a porous diaphragm 11 feet by 3 feet of biscuit or earthenware [i.e. an unglazed ceramic slab], the later inventors simply content themselves by mentioning a porous diaphragm [10.13].

Hurter suggested that an electrolytic alkali process would be potentially superior to the chemical ones, because the electrical energy could be applied directly to bring about the conversion of salt to alkali, and this could be done at ambient temperature. Although he did not develop these points further, it is clear that he believed that the electrolytic conversion of salt to alkali was a particularly efficient means of utilising energy to produce chemical change, despite the fact that it was necessary to burn a fossil fuel to generate electrical energy for the process. By comparison, the Leblanc process included a number of high temperature stages which were of low efficiency. These resulted in a high total energy requirement which adversely affected the overall efficiency of the conversion of salt to alkali.

He pointed out that the electrolytic process possessed certain disadvantages: the separation of the products was difficult, and its overall economic efficiency would be lower than Leblanc or ammonia-soda, because of the high cost of generating electricity at that time.

Despite the innovative nature of Hurter's paper, it did possess some weaknesses, but it is felt that no great blame could be attached to him for these. In the absence of reliable experimental data on the relatively new electrolytic alkali process, he was forced to make assumptions about the efficiency of the electrolytic cell. He assumed that the cell voltage was twelve volts (whereas three to four volts would have been more correct) and that its resistance was 0.1 ohm (one hundred times the correct figure). As a result he obtained too high a value for the energy required by the process. Furthermore, the technology for producing large quantities of electrical energy cheaply was still in its infancy. On the basis of the cost of electricity and on the high energy requirements of the process, Hurter concluded that alkali manufactured by electrolysis would be unable to compete successfully with that made by the chemical-based alkali processes. At a later date, further research produced much lower values for the energy required by the cell and, at the same time, the cost of electricity fell substantially. By 1895 Hurter was in a position to change his opinion on the matter and he did so with confidence.

He did believe that there were some electrolytic processes which could be commercially profitable (e.g. the extraction of metals and the manufacture of chlorates). He pointed out, to his credit, that if the economics of the electrolytic alkali process could be improved it would be superior to Leblanc or ammonia-soda. He concluded:

The supply of energy for decomposing sodium chloride is theoretically easiest to procure by the electrolytic method, but in practice is more expensive by that method than by the ordinary methods [Leblanc or ammonia-soda].

The separation of the products of the reactions from each other is practically easy and complete in the Leblanc process and ammonia-soda process, but offers almost insurmountable difficulties in the electrolytic process.

These considerations led me to the conclusion that, for the production of articles of low price, electrolysis as a manufacturing operation was impracticable [10.14].

These remarks are significant because they show that Hurter believed that the electrolytic alkali process was more efficient thermodynamically than Leblanc, but the cost of the energy required and the difficulty of separating the products rendered it non-viable at that time.

Importantly, the third statement was quoted by Reader as being the basis of Hurter's advice to the Board of UAC not to adopt the Castner process (see Chapter 11). Reader appears to have overlooked the fact that the advice was given seven years after the statement was made, by which time the situation (and Hurter's opinions) had altered considerably.

Hurter's paper was well-received; there was no dissent from his views and it was evident that he was held in high esteem. E K Muspratt, a person highly regarded in the alkali industry, said that he doubted whether, in the whole Society, there was another man who could read a paper which combined, so thoroughly, scientific with practical knowledge. He was very interested in a process for manufacturing chlorates but, after hearing Hurter, he felt that this possibility was far distant.

In reply Hurter repeated his earlier view that, if there were any chance for an electrolytic process, it would be for the manufacture of chlorates because of their high market value, which would justify the use of the more expensive electrolytic process. The amount of electricity which would be required was so large, however, that such a process would be unlikely to be technically viable at that time.

One of Hurter's principal duties as Chief Chemist of UAC was to evaluate potential electrolytic alkali processes submitted by other inventors, with a view to their adoption by the company. He carried out any necessary experimental work objectively and thoroughly and, where a process showed promise, was open-minded enough to say so; he never condemned one out of hand [10.15]. Accusations that he was ill-informed and prejudiced against electrolytic alkali processes were unfounded [10.16, 10.17]. As Hardie pointed out in 1950:

... by the mid-1890s Hurter was better equipped than any other industrial scientist in the country to pass judgement on an electrolytic process [10.18].

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Hurter's reports were sent directly to the Board, but not all the directors were sufficiently conversant with the subject of electrolysis to be able to understand them fully. Therefore, in 1892, he produced written guidance for their private instruction which explained electrolysis and electrolytic processes in layman's language [10.19].

In 1891 Hurter criticised Cross and Bevan's report on a new electrolytic alkali process invented by Greenwood, because they had failed properly to verify the latter's claim that he had invented a porous diaphragm cell of low resistance. Just as he had done in his earlier dispute with them [10.3], Hurter supported his arguments with a detailed programme of experiments on a Greenwood cell which he had constructed. He recognised that Greenwood's process was promising, but his study of it led him to the following conclusions:

Electrolysis cannot be efficiently used for the manufacture of caustic soda, since the current efficiency is too low at the point where a solution has been obtained from which caustic soda could with advantage be manufactured in the solid state.

With the present prices for caustic soda and bleach, it is however, likely that a paper works will again attempt the preparation of these chemicals. No doubt the failure of the Hermite process ... will retard the application a little, but the process of Greenwood's holds out a double profit. Hermite produced chlorine only, Greenwood proposes making caustic soda at the same time.

As an operation for the manufacture and sale of solid caustic soda and bleaching powder, I still think electrolysis hopeless. As a means of producing a caustic solution (brine) and gaseous chlorine in a paper works for our own consumption, the process will probably make some noise, but will not succeed unless prices of caustic and bleach are maintained at too high a price [10.20].

Also in 1891, Hurter gave a public demonstration of the electrolysis of brine in a cell invented by Le Sueur [10.21]. He evaluated the process, including both the electrolysis plant and electrical generating apparatus, and concluded that it showed promise as regards its electrolytic functions, but that it could not compete economically with Leblanc. Furthermore, the total capital cost of the electrolysis plant and its associated electricity generating apparatus was almost four times that of a Leblanc plant having the same output of alkali. He concluded that, whilst Le Sueur's process was not commercially viable at that time, he certainly would not dismiss the possibility of a successful electrolytic alkali process of that type being developed in the future:

It is thus certain that by the Le Sueur process, 1 ton of 70% caustic soda and 16 cwt of free chlorine will cost, exclusive of interest, depreciation and general expenses, nearly £14.10.6d, which exceeds the cost of the same articles by the Leblanc/Deacon-Hasenclever processes by over £3.

It must be remembered that the costs of the electrolytic processes are estimates based on the inventor's figures, certain to be found too low, whilst the costs of the Leblanc/Deacon-Hasenclever are well-known.

The Le Sueur process is useless as a process for manufacturing caustic soda or chlorine for sale, and the electrolytic method generally is not at present likely to compete with the Leblanc process. It is, of course, capable of improvement as shewn by the theoretical costs given and it is necessary to watch carefully these improvements [10.22].

Hurter visited the Electrolytic Caustic Soda and Chlorine Trust in 1893 to inspect their electrolytic alkali plant. In a scathing report to the directors of UAC he commented that the carbon anode cell was "very roughly constructed" and the production rate was very low; it was therefore quite unacceptable as a commercial proposition [10.23].

At about the same time Hurter studied three new electrolytic processes invented by Vautin, Heitmann and Castner, all of which possessed the common feature of employing the new mercury cells. He reported to the Board:

The above processes all have mercury in common. Vautin has offered us his process. In face of Castner's it may safely be declined. Heitmann's and Castner's appear to be identical, but seeing that Heitmann's patent specification has only just been accepted, it appears to me that his patent will be invalid [10.24].

Because he felt that Castner's process showed promise, Hurter produced a separate report to the Board about it. The matter is of such importance that it is discussed in detail in Chapter 11.

In the course of carrying out his evaluations of mercury cells, Hurter identified a possible toxic risk to process operators from the inhalation of mercury vapour. The matter arose again two years later when Castner published the prospectus of the newly formed Castner Kellner Alkali Co in the Liverpool Daily Post [10.25]. The next day a letter to the newspaper expressed concern about the emission of mercury vapour into the atmosphere and the deposition of mercury-containing waste on the land [10.26]. It is perhaps surprising that these matters, which are of such concern today, should have exercised the minds of the general public a century ago. Hurter's concern was more understandable, since he would have been more knowledgeable on the subject. Castner responded:

Quicksilver is volatile at low temperature but this property is of no moment in the process as the metallic surface is always kept wet, and the metal is never removed from the electrolytic cells - there is absolutely no danger of mercury poisoning [10.27].

Even after UAC decided not to adopt Castner's process, Hurter continued evaluating others. His assessment of that of Hargreaves and Bird, in 1895, is discussed here at some length because it

provides information on the attitudes of the Leblanc industry to electrolytic alkali processes at that time. These opinions altered and became better informed as the processes were developed and improved.

Hurter was severely criticised by Hargreaves, a relatively unknown chemist, for his earlier opinion that an electrolytic alkali process could not be economically viable. Hargreaves made the quite unfounded allegations that Hurter's knowledge of electrolytic alkali manufacture was deficient and out of date and that he was unreasonably prejudiced in favour of the Leblanc system.

Hurter visited Hargreaves and Bird's factory on behalf of UAC to inspect their process. His report was unavoidably brief, due to Hargreaves' lack of co-operation with him, but from the small amount of information which he was able to obtain, he was not convinced that the new process justified its inventors' claims:

I asked no questions, seeing that there was some reluctance in explaining me the nature of the diaphragms. All that follows is simply a statement by Mr James Hargreaves.

I hold the Castner cell to be still much superior .... Ingenious as the cell of Mr Hargreaves is, it is not in my opinion, a proved success [10.28].

There had been no love lost between Hargreaves and Hurter for some years. During the discussion of a paper which Hurter presented to the SCI in 1895 [10.29], Hargreaves criticised that paper and an earlier one by Hurter to the Liverpool Physical Society in 1893 [10.30]. Later in 1895, in a convincing presentation to the Liverpool Polytechnic Society [10.31], he attacked Hurter's earlier opinions about the disadvantages of an electrolytic alkali process, saying that it had been "demonstrated" mathematically [by Hurter, in particular] that it was impossible to get more favourable results. Referring to what he believed were Hurter's unreliable assumptions about the operating conditions in an electrolytic cell, Hargreaves remarked:

Granted the truth of the hypothesis, the practical manufacture of alkali and chlorine by electrolysis is doomed to inevitable failure. ... But the mathematical mill only grinds out what you put into it. ... If you put fictions in you must not expect to get facts out [10.32].

He pointed out that Hurter's over-estimate of the cell voltage and resistance had produced a gross over-estimate of power requirements. Hargreaves and Bird had found that:

Tons of salt have been decomposed with a potential difference between the terminals varying from 3 to 4 volts [10.33].

Hurter had claimed that a current of 1000 amperes flowing continuously through a cell for three years would produce only one ton of hydrogen. Hargreaves reminded him, somewhat sarcastically, that at the same time £20,700 worth of bleach and soda would also be produced - a fact which Hurter had either overlooked or chosen to omit.

Hargreaves claimed that electrical power could be supplied at half the cost quoted by Hurter and that the products of the process could be sold for twice the price given by him. Commenting on the by-then famous assertion in Hurter's 1888 paper that:

> The separation of the products from each other is practically easy and complete in the Leblanc, and in the ammonia-soda processes; but offers almost *insurmountable difficulties* in the electrolytic process [10.34].

Hargreaves remarked:

> The worst of the matter is that, when the difficulties *are* surmounted, the business is so simple, that those who threw wet blankets over the workers while surmounting the difficulties, now say: "There is nothing in it but what any fool could do" [10.35].

He described the construction and operation of his full-scale plant, in which the products had been successfully separated.

Hargreaves' attack on those whom he regarded as being opponents of electrolysis (including Hurter) is quoted in full to demonstrate the attitude towards electrolytic processes of some conservative elements in the Leblanc and ammonia-soda industries at the time:

> There seems to be a kind of partisanship on the question of commercial electrolysis. The methods of the election agent and the special pleader are resorted to as if the results depended on the votes of a crowd or the verdict of a jury. A scientific fact *is* a fact whether one believes it or not. Contradiction or argument cannot affect it in the least degree. It is rather suggestive that all the militant opponents of electrolysis are interested in arresting its progress. As an instance, Dr Hurter has "accepted a brief" against its application to the alkali manufacture. He has conducted his case with a degree of eloquence, skill and ability which all admire. The manner in which he has marshalled his witnesses, and the logical acuteness with which he has supported their evidence are models which the legal advocate (who, above all other things seeks to gain the cause of his client) may well copy with advantage. But unfortunately scientific facts have an awkward way of asserting *themselves*, whereby the evidence of honest but mistaken witnesses is confounded, and the arts of the special pleader set at defiance [10.36].

He concluded:

> ... the patience spent [by himself and Bird] in accomplishing this work [viz. solving the problems of electrolytic processes], is a mere trifle compared to what has been devoted to proving that *it cannot be done* [10.37].

There was a small amount of truth in Hargreaves' assertion that there was some degree of "opposition to electrolysis", due to partisanship and to the desire to arrest progress, by the Leblanc industry. Of far greater importance was the fact that UAC simply was unable to finance the adoption of the electrolytic alkali process at that time (see Chapter 11). He was unfair to Hurter because, in criticising the opinions which Hurter had expressed years before, in 1888, he used 1895 data on the efficiency of cells and the cost of electrical power. Hurter's reports on his assessment of Castner's process show clearly that, by 1895, he had updated his knowledge of electrolytic alkali processes and was far more favourably disposed towards them than he had been seven years earlier.

An important factor in reducing the cost of alkali made by electrolysis was that, by the mid-1890s, the cost of electrical power had fallen substantially, for two reasons. The first was the improvements which had been made to electricity generating plant, especially the introduction of the steam turbine for driving the generators, a development which Hurter could not have been expected to predict since it was outside his discipline. The second reason was the increase in size and output, and consequently the power requirements, of electrolytic cells, resulting in a decrease in the unit cost of power. Another important reason for the improved profitability of the electrolytic alkali process was the increased efficiency of the cells.

Hargreaves described his electrolytic alkali process in a paper presented to the Society of Chemical Industry [10.38]. During the discussion, Hurter repeated his earlier reservations as to whether the Hargreaves-Bird cell was as efficient as Castner's but, surprisingly, did not otherwise react to Hargreaves' attack upon him. He confined himself to a sarcastic response:

> No one who knew Mr Hargreaves could do otherwise than acknowledge him a genius; and the cell he had devised was as ingenious as any he had seen. Whether it was the best or not was, of course, another question [10.39].

At the UAC Central Laboratory a great deal of research was carried out on electrolysis and this was not confined solely to alkali manufacture.

One of Hurter's most learned papers: "On Electrolysis", was presented to the Liverpool Physical Society in 1893 [10.30]. It concerned the use of electrolysis to convert salt into sodium hydroxide and it addressed, in particular, the problem of the formation of secondary products. The research described

demonstrates Hurter's ability to combine both theoretical and empirical approaches to a problem, supported by meticulous experimental work.

Hurter explained that Faraday's law only applied to processes in which a single electrolyte was present. Hittorf, Buff and others had shown that, when there were two or more electrolytes, Faraday's law still applied regarding the sum of the chemical effects which the current produced in the cell, but any individual effect was only a fraction of that which the current would have produced had there been only one electrolyte.

Hurter determined the electrical conductivities of a range of mixtures of sodium hydroxide and chloride, showing how the percentage of the current employed in decomposing the sodium chloride (the current efficiency) fell as sodium hydroxide was produced. His work confirmed his long-held view that secondary products seriously impaired the efficiency of an electrolytic cell, causing it to decrease during the electrolysis. His work was principally a theoretical exercise to offer reasons for these problems, but he proposed no solutions. In time they were solved by improvements in cell design.

In 1894, Hurter patented a process for making alloys of sodium or potassium with lead by electrolysing molten sodium or potassium hydroxide. The cathode was molten lead (where the alloy was produced) and the anode was iron, which was purposely non-reactive [10.40].

In the same year there were three patents [10.41] to [10.43] and one in 1896 [10.44], all referring to the ingenious idea of combining the electrolytic and Leblanc alkali processes. Zinc blende (zinc sulphide) was thermally decomposed to produce sulphur dioxide and zinc oxide. The sulphur dioxide was converted to sulphuric acid which was used to make Leblanc saltcake. The zinc oxide reacted with hydrochloric acid from the Leblanc process to form zinc chloride which was electrolysed to produce metallic zinc and chlorine. In much the same way as the partial ammonia-soda process (Chapter 6) was devised by the Leblanc manufacturers to compete with Solvay, so this combination of the Leblanc and electrolytic processes was intended to be one means of maintaining the profitability of the Leblanc system. There is, however, no record of the process having been developed commercially.

An 1895 patent described a process for removing lead from bismuth by electrolysis [10.45]. The bismuth was the anode of a cell, the cathode was usually carbon and the electrolyte nitric acid. During the electrolysis, the anode dissolved. The lead remained in solution; the bismuth was deposited at the cathode and was processed to produce the pure metal.

Also in 1895, Hurter published a paper entitled "Notes on the efficiency of electrolytic apparatus" [10.46]. In assessing an electrolytic process, certain electrical information was essential: the current efficiency, the e.m.f. of polarisation and the cell resistance. The difficulties of precalculating or determining these factors were described.

In 1897 he presented the last paper of his life, appropriately, perhaps, on the subject of electrolysis. It was to a meeting of the SCI and was entitled: "The efficiency of an electrolytic cell"

[10.47], a follow-up to that of 1895 [10.46]. The necessary factors for understanding the conditions of electrolysis of mixed electrolytes had been obtained. The experimental methods and mathematical deductions used were characteristically precise and comprehensive.

The most commercially successful outcome of Hurter's research on electrolysis was his process for manufacturing potassium chlorate by electrolysing potassium chloride, which was patented in 1893 [10.48, 10.49]. This fulfilled the prophecy which he had made five years earlier:

> If there is any chance of an electrolytic process at all, it will be for the manufacture of chlorates [10.50].

Having carried out the basic laboratory development work on the process, Hurter requested the Board of UAC to finance the erection of a pilot plant [10.51], [10.52]. By now he was clearly optimistic that electrolytic manufacturing processes would become commercially successful in the near future:

> It is quite clear that many articles may be produced by electrolysis on a large scale cheaper than by any other means [10.51].

His proposal that potassium chlorate was a suitable product for a new electrolytic process had been carefully thought out. The chemistry of the chlorate process was relatively simple and well-known, but the secret of Hurter's invention was the design of the cell, which permitted the products of electrolysis of a chloride solution to interact within it.

The money was granted, the pilot plant built and staff trained in its operation. It produced data which was used for designing a full-scale plant and improving the energy efficiency of the process. So successful was Hurter's invention that UAC patented it in the United States [10.53], as a preliminary to constructing a plant there [10.54].

Hurter visited the USA in search of a suitable site for the factory in October 1897, six months before his death, when he was in failing health. The stress of the visit may have contributed to his death, but he must have gained satisfaction from the fact that one of his inventions was soon to be developed commercially. The last report which he wrote, just three days before his death, was on the subject of electrolysis: a letter to UAC's Chief Engineer, E J Duff, about to the proposed layout of the process plant [10.55].

The plant was completed after Hurter's death and was a major commercial success, remaining in production until 1928. It manufactured potassium chlorate at first and later also produced sodium chlorate and perchlorates.

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Hurter has been accused of not being properly informed on electrolysis and electrolytic processes, and of having very little interest in them. This was far from being the case. When the subject of electrolysis was still in its infancy, he was carrying out research on electrochemical theory and cell reactions. He was far better-informed on the subject than most of his contemporaries.

In 1888, he believed that an electrolytic alkali process was unlikely to be successful, chiefly because of its economics. We may, perhaps, have some sympathy with him. At that time, few people challenged his opinions, because they were far less well-informed than he was. He had the courage to make assumptions on cell efficiency and the cost of electrical power which, in the event, proved to be over-pessimistic. By the mid-1890s, his opinion had changed as he learned more about electrolytic alkali processes and, when he evaluated Castner's process, he believed that such a process would in time become successful, although he was still typically and properly cautious.

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CHAPTER 11

UAC AND THE ELECTROLYTIC ALKALI PROCESS

In 1894 the British Leblanc alkali industry (which was, to all intents and purposes, UAC) was not in a healthy state; competition from ammonia-soda was serious and profits were unsatisfactory. Firm decisions were needed to ensure UAC's prosperity in the coming years.

As a result of their investigations into electrolytic alkali processes, UAC were aware that chlorine which was produced by that process would eventually be a serious competitor of that made by their Deacon and Weldon processes. At the beginning of the 1890s, however, they did not see this as a serious threat because of the high capital cost of electrolysis plant and electrical generating equipment, and the cost of supplying electricity.

By the mid-1890s the situation was changing and UAC became interested in the manufacture of alkali which was made by the electrolysis of brine, as a possible life-saver for the company. They considered a number of potential processes, Castner's being the one which they evaluated most thoroughly, carrying out lengthy and in-depth negotiations with its inventor. In the event, they decided not to adopt the process and the company continued to decline. Undoubtedly their rejection of an electrolytic alkali process was a major factor in this decline, but there were other reasons too, which are discussed below. It would have been possible, of course, for UAC to have developed their own electrolytic alkali process, but they elected not to do so, preferring to select a suitable one which had been invented outside the company.

Some historians have suggested that Hurter's advice to the UAC Board not to adopt the Castner process was the chief reason for the future failure of the company. This was not so. Hurter's advice covered only some of the factors which the Board had to take into account. A detailed analysis of the matter shows that it was largely matters which were outside Hurter's terms of reference which produced the Board's final decision.

The discovery of the mercury cathode by the American chemist, Hamilton Young Castner, had an important influence on early electrolytic processes. Castner came to Britain in 1886, at the age of twenty eight, with a chemical process which he had invented for making metallic sodium from caustic soda by fusing it with carbon. The Webster Crown Metal Co, Birmingham, was interested in his

invention because they required metallic sodium for the production of aluminium. After evaluating Castner's process, a new company, the Aluminium Company Ltd, Oldbury, was set up to exploit it, with Castner as Managing Director.

Following an unprecedented fall in the price of aluminium, Castner changed over to manufacturing sodium metal. He invented a process based on the electrolysis of molten sodium hydroxide, but found that the only sodium hydroxide available for use in it was not pure enough. He therefore decided to make his own sodium hydroxide by electrolysing brine and for this purpose he invented the rocking mercury cell [11.1], [11.2].

All the original correspondence which refers to the negotiations between UAC and the Aluminium Company regarding Castner's process is at the Cheshire Record Office (File No DIC/X10/442); the following account is based in the main on an analysis of this. Hurter's reports from the Central Laboratory Report Book, and the UAC Board minutes, have also been used; these are referenced individually.

Because of their interest in an electrolytic alkali process, UAC approached the Aluminium Company in 1893. This was done upon the advice of Hurter who, as a result of his studies of electrolytic alkali processes, believed that Castner's showed promise and that UAC should consider adopting it. The Aluminium Company were interested in the proposal because they were not large enough themselves to exploit the invention properly.

Negotiations soon commenced between the two companies and lasted about eight months, during which time both technical and commercial activity on the matter was intense. A great deal of reliance was placed upon the considerable expertise of electrolytic processes which Hurter had acquired during the preceding ten years. It certainly cannot be claimed that UAC, principally represented by its Chairman, John Brock, who was advised by Hurter, did not thoroughly and fully assess both the commercial and technical potential of Castner's invention.

Hurter's part in the negotiations was to carry out a technical and economic appraisal of Castner's process. There were a number of other factors which affected the decision of the Board of UAC not to adopt Castner's process, but it should be stressed that these were outside Hurter's terms of reference and influence.

Castner initially submitted a description of his process to UAC. This document is undated, but must have been issued after his rocking cell patent of May 1893 and before November of that year. Hurter studied it thoroughly and sent his preliminary comments to Brock on 24 November 1893, followed by a full report on 27 November 1893. The calculations for the full report are in the form of manuscript notes [11.3]. The report itself included estimates of the size of cell, mercury usage and energy requirements. Hurter cautiously concluded :

The mercury processes are very interesting and of all the electrical processes the most promising, but I do not think that anybody will get very rich by means of them.

Undoubtedly the solution of the problem of electrolytic soda and chlorine is approaching nearer, but the enormous outlay of capital and the long time which such a slow process will require to work out, will for many years yet, leave the ordinary processes master of the situation. But it is necessary to move with the times.

Nevertheless I should not buy a process unless for tonnage royalty. There is no telling what tomorrow will bring [11.4].

On 7 December 1893 Brock, Hurter and Muspratt (Vice-President of UAC) visited the Aluminium Company to inspect an experimental Castner cell. From data which the Aluminium Company provided, Hurter produced a comparison of Castner's estimated costs of his process with actual UAC figures for the Leblanc and Deacon-Hasenclever processes. In his comprehensive report of the visit, he concluded that:

... under the most favourable conditions, the electrolytic process according to Castner will not produce caustic soda and bleach any cheaper than the Leblanc process.

Nevertheless, I think that we cannot afford to overlook this process. It ought to be known to us before it is generally known even to the Aluminium Co. If we could offer them to undertake a trial, I think that trial ought to be made ... to find the difficulties of the process and wear and tear on the cells, carbons, mercury etc [11.5].

Before the end of 1893, UAC proposed that an experimental plant be erected at one of its own works, but the Aluminium Company would not agree to this, wishing to keep such a plant on their own premises.

In January 1894, Hurter produced some further cost estimates which seemed favourable towards Castner. In the same month the Aluminium Company gave full details of the patent position and proposed that a fifty cell pilot plant be constructed at their factory at Oldbury, with UAC paying £7,000 towards its cost. The patent rights would be sold to UAC for £100,000 and, in addition, a royalty would be payable of 10s. per ton of salt decomposed.

Hurter advised Brock that such terms were exorbitant and UAC counter-proposed that the royalty be one-tenth of the saving achieved over the cost of manufacture by the Leblanc route. This resulted in the Aluminium Company's asking for details of UAC's Leblanc production costs - which were refused.

The minutes of UAC's Board meeting of 19 January 1894 record:

> The Chairman [J Brock] reported the negotiations with the Aluminium Company and read the correspondence with Mr G A Balfour [Chairman of the Aluminium Company] [11.6].

The Board decided that a stalemate existed and, at the next meeting, on 16 February 1894, it resolved to terminate negotiations:

> The Chairman reported the purport of the negotiations with the Aluminium Company which had taken place since the last meeting. After discussion it was resolved that the negotiations with the Aluminium Company as they have proceeded do not in the opinion of this Board afford hope of an agreement being made to the advantage of this company and that therefore the negotiations be not further proceeded with on the present basis [11.6].

Somewhat surprisingly, in June 1894, UAC requested that negotiations be re-opened. The Aluminium Company agreed and suggested a second visit to Oldbury to inspect a new small-scale production plant which Castner had built. Before the visit they provided UAC with some revised cost estimates in which the cost per ton of mixed produce (caustic soda and bleaching powder) was quoted as £2.9.4, compared to £4.0.10 previously. However, the costs of packaging the product, and of plant maintenance, were omitted from these new estimates. Furthermore, large reductions had been made to the costs of labour and fuel. Hurter considered these new figures in detail, comparing them, item by item, with the previous ones. He was quick to discover their weaknesses and reported:

> In this last estimate Mr Castner has gone too far. His cost per KWH is ridiculously low. Whilst he admitted at Oldbury a cost of one halfpenny per KWH, he has now lowered the price to 1/10 of a penny. I should say we shall be pleased to see the plant at work, but we must have a chance of verifying the statements before any fixed terms can be accepted.
>
> Castner's cell is, in my opinion, still the best [11.7].

Hurter and Brock paid their second visit to Oldbury on 1 August 1894, when the caustic soda production from fourteen cells was measured over a period of three hours. The cell design had been improved since the earlier visit, particularly with regard to improving the integrity of the joints so as to reduce leakages of mercury. Hurter's report of the visit described the plant, recorded conversations with representatives of the Aluminium Company and examined again, in minute detail, Castner's reduced cost estimates. Castner believed that his lower estimates were justified, in view of guarantees from the makers of the boilers, steam engines and dynamos regarding the efficiencies of their respective plants,

and of new information which he had obtained on fuel-to-power conversion efficiencies. Operating experience had shown that labour and maintenance costs were lower than had originally been estimated, and also that the carbon electrodes lasted longer, thus incurring a lower replacement cost per ton of product. Hurter accepted that Castner's figures for the cost of maintenance had been properly obtained, but emphasised that they had been based upon trials carried out on new plant. He asked:

Can the plant work day and night all year round without losing the very high efficiency upon which the Aluminium Company have had to rely for their results? [11.8].

He did, however, accept the efficiency figures for the plant which were claimed by Castner and proceeded with his assessment on that basis. Importantly, he was able to pin-point areas in the calculations where an expected fall in, for example, fuel conversion efficiency would produce an immediate and substantial increase in operating costs. He recorded without comment Castner's brash and quite unrealistic claim that:

... no depreciation had been allowed because the plant was practically indestructible and having reached finality as regards the invention, there was no fear that the process would soon be superseded ...

Hurter's final conclusions were that:

Mr Castner's cell is certainly the best which has been brought to our notice, but more extensive experiments are required to settle finally the cost with respect to fuel consumption, cell labour, wear of carbons and mercury.

The relative quantities of caustic soda and bleaching powder produced from one ton of salt are at present only fair guesses, it being within the bounds of reasonable probability that losses of both salt and chlorine may occur.

The caustic soda produced by the process is extremely pure and if brought into the market in large quantities will, on account of its freedom from chlorides and carbonate, become a serious competitor to our own product.

The cost as far as one can judge will be about £4 per ton of mixed produce or, if differently expressed, £6 per ton of caustic and £3 per ton of bleaching powder.

The capital needed for a production of 60 tons of caustic and its equivalent of bleach per week will be about £70,000.

At this visit Hurter and Brock were told that other parties, including Mond, had been approached with a view to their taking up Castner's invention, and that a speedy decision from UAC was required. It appears that Mond and a representative of the Solvay Company had considered Castner's process at Oldbury in June 1894, when they were shown the same cost estimates as had been placed before the UAC representatives. Although Brunner, Mond & Co was in a much better financial position than UAC (it paid a 30% dividend in 1894), Mond took the same decision as UAC - to turn Castner down - but not for the same reasons. Mond had never been enthusiastic about electrolytic processes and, whilst he wanted to make chlorine, he had no wish to make alkali simultaneously. He was making all the alkali he required by the ammonia-soda process and did not see electrolytic alkali as a threat to this. Also, he still harboured his early ambition: to recover chlorine from the ammonia-soda process; without success, as it eventually proved [11.9]. Hurter was therefore not alone in being cautious about the Castner process and Mond was probably as capable as he was at assessing the situation.

Brock presented his report on the visit to UAC's Board on 24 August 1894, but this is referred to only briefly in the minutes [11.10]. After that, there appears to be no documentation in existence about any further negotiations between UAC and the Aluminium Company, nor any reference in the Board minutes to the matter. It seems that the Board's decision of 16 February 1894 stood and that the approach to the Aluminium Company in August had been no more than a "last ditch" attempt to revive the negotiations. The matter, it appears, had been left to die.

It is probable that UAC would have purchased or licensed Castner's process from the Aluminium Company, to the benefit of both parties, had the financial arrangements been more realistic. Alternatively, they might have been interested in a partnership agreement, but the Aluminium Company was apparently not prepared to consider this.

In considering Hurter's advice to the Board of UAC, three questions arise:

To what extent did his report to the Board affect their decision not adopt Castner's process, compared with the other factors which they had to consider?

How did the UAC's decision not to adopt an electrolytic alkali process affect its future prosperity?

Would the eventual demise of the company have occurred anyway, but for quite different reasons?

Of the historians who have addressed these points, Reader has strongly implied that Hurter's advice to UAC not to adopt Castner's process was responsible for the decline and eventual demise of the Leblanc alkali industry in Britain:

[Hurter] entered with authority into the firm's highest councils. ... it is unfortunate that when his advice became really influential, *it turned out to be disastrous* [11.11].

The negotiations [on adopting the Castner process] *foundered mainly on Hurter's advice* [11.12].

It is a final ironic comment ... that UAC's future was wrecked [because they] set up Central Research Laboratory *and acted on the advice of the man at the head of it* [11.13].

These statements are misleading. Hurter's position in UAC was not nearly as prestigious as Reader obviously believed it to be. Although Hurter was Chief Chemist of UAC, he was only an employee and not a director. In the 1890s (and later) this was a clearly defined difference in status and he was unlikely to be as influential with the Board as someone in a similar position would be today. Also, his advice to UAC's Board was only one factor out of many which they had to consider in reaching their decision.

Reader was an economic historian, not an industrial chemist, and it may be that in forming his opinion he did not fully appreciate to what extent Hurter's background and long experience in the Leblanc industry properly and responsibly influenced his report to the Board of UAC. It is also doubtful whether he analyzed fully the substantial documentation relating to the negotiations between the Aluminium Company and UAC, or Hurter's reports on the various studies which he carried out on electrolytic alkali processes (see Chapter 10). These are of considerable value in clarifying the position.

Hardie claimed that Hurter was prejudiced against electrolytic alkali processes because of his long association with, and loyalty to, the Leblanc industry:

Hurter combined in a remarkable fashion a sincere desire for scientific truth with *a strong prejudice against the new process*, a prejudice derived, to all appearances, from emotional loyalty to the system he had so long defended [11.14].

This comment is unfair. Had Hurter been prejudiced against Castner's process, he would hardly have recommended that UAC open negotiations with the Aluminium Company with a view to adopting it. It was against his nature to be other than completely objective in assessing any matter upon which he was asked to give an opinion.

Having recommended that UAC should consider the Castner process, Hurter undertook his study of it impartially and with efficiency and thoroughness. He stated, fairly, that the process possessed technical merit and, because of the considerable experience which he had acquired in studying electrolytic alkali processes, he was well qualified to make this judgement. The negotiations failed primarily because of high capital and operating costs. If any criticism is to be made of Hurter, it is that he was typically cautious, one could say responsible, in the face of Castner's obviously over-exaggerated claims.

As early as 1888 Hurter had suggested that electrolysis of brine would probably be the most efficient way of making alkali from salt, but for the prohibitive cost of electrical energy at that time. He was certainly never unreasonably prejudiced against it. Dickinson very sensibly remarked that:

Hurter's faults, if any, were dictated by caution in the face of obviously exaggerated claims [11.15].

Lastly, Hurter should be credited with the fact that he gave his advice to the Board of UAC in 1894, when Castner's process was by no means fully proven. It was not to be so until several years later.

The documentation relating to the negotiations between UAC and the Aluminium Company shows that these were almost entirely in the hands of Brock, advised by Hurter. It is apparent that it was Brock who was really responsible for UAC's decision, but there is little doubt that he relied heavily upon Hurter's advice on technical matters. The UAC Board minutes for the period reveal that the Board members themselves had very little involvement, the only reference being to the discussions on 19 January and 16 February 1894. Brock must have been aware, as a result of informal discussions with the Directors, what their feelings on the matter were. Hurter reported to the Board of UAC on two matters: the technical viability of Castner's process and its operating costs, and he did this thoroughly and efficiently. He would have been aware of the other factors which they would need to take into account in deciding whether to adopt the process or not, even though these were outside his remit. It would be surprising if, perhaps even subconsciously, he did not write his report so as to take some account of them. He was conservative in his advice, but he was also advising an extremely conservative board. UAC's Board regarded Hurter's advice as only one factor in their decision; they would not have considered it to be an absolute recommendation on the matter. They would have compared Castner's over-optimistic, even irresponsible, claims with Hurter's objective, unbiased approach.

Whilst the evidence supporting this assessment of Hurter's role in the rejection of Castner's process is clearly documented, that which refers to the other matters which the Board of UAC considered in deciding not to adopt the process is somewhat more speculative. It includes an appraisal of the economic position of both UAC and the British chemical industry at that time and also upon information, discussed in Chapter 2, about the measures which UAC adopted to ensure its survival

following its formation. Factors which influenced the Board's decision not to adopt Castner's process were: the availability of capital, the age and conservatism of the directors, diversification into new product areas, and rationalisation of the company's products and production units.

The availability of capital for purchasing the process, and its operating costs, were closely connected and, at the end of the day, were the deciding factors in the negotiations. Admittedly, even Hurter's most pessimistic estimate of the cost of operating Castner's process did produce a small overall advantage in Castner's favour, compared to the Leblanc process. This margin would not, however, cover the high royalty terms demanded, nor the cost of borrowing the enormous amount of capital required to purchase the plant, even had this been possible. The availability of capital was greatly influenced by the unsatisfactory financial state of UAC in 1894, a year in which no dividend had been paid. At the 1895 Annual General Meeting, the Chairman claimed that the state of trade world wide was poor, 1894 being one of the worst experienced for many years. It is hardly surprising, therefore, that the offer of a highly speculative process, which required a large outlay of capital, should be unattractive to UAC at that particular time. It was unlikely that, in such a financial climate, they would have been able to borrow sufficient capital, or to afford the interest on a loan.

The second factor which influenced UAC's rejection of the electrolytic alkali process was that many of the Directors, who came from the component companies which comprised UAC (see Appendix VII), were elderly and steeped in the traditions of many years in the Leblanc industry; their principal interest was in keeping their ailing factories in existence. By the 1890s, the Leblanc system had been in operation for about a hundred years and most of the improvements which could be made to it had been implemented. The electrolytic alkali process was new technology, and the challenge of having to learn, develop and improve it was unattractive to these elderly practitioners of the Leblanc system. Their conservatism made them suspicious of Castner's brashness and what they perceived as his over-optimistic claims for his process. It is true that Castner was over-optimistic - for a largely unproven new process - but this optimism was to some degree necessary if the next generation of alkali manufacture was to evolve successfully. Indeed, this optimism was justified when, several years later, his process became a commercial success. History has shown that major new industrial processes are frequently introduced and developed, not by an existing industry, but by a new one set up solely for that purpose [11.16], [11.17]. Castner's invention was an example of this and it resulted in the setting up of the Castner Kellner Company.

It was also necessary for the Board to consider whether the remedies which it had implemented following the formation of UAC had been effective enough to ensure its survival and future prosperity, without the need to change over to the electrolytic alkali process. These were, rationalisation of the production units and processes, disposing of those which were inefficient, and diversification into new product areas. Historians have commented differently on these matters.

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Brock was complimentary:

> As a result of Hurter's warning UAC off the newly-developed electrolytic processes, the Leblanc alkali works missed the opportunity of dominating the market in chlorine, bleach, and caustic soda production. Instead, by diversifying its products as much as possible, and by dominating the production of sulphuric acid, UAC remained viable and increasingly efficient as its constituents gradually moved over to the Solvay process [11.18].

As was Haber:

> Considering the company's plight, a great deal was done both to improve methods of manufacturing existing products and to develop the preparation of new chemicals. ... the range of chemicals was widened. In 1906 besides making alkalis, bleaching powder and sulphuric acid, UAC was producing a great many other chemicals, amongst them acetic acid, acetone, arsenic, carbon bisulphide, cyanides, fertilizers, artificial ultramarine, nitric acid and sulphur ... and ... chlorinated hydrocarbons [11.19].

Reader was, typically, critical of UAC:

> What rationalisation was attempted was too little and not radical enough. The management displayed nepotism, amateurism, lack of technical knowledge and scientific training [11.20].

Of these comments, Brock's and Haber's are felt to be the fairest and most accurate.

Laudable as UAC's decision to diversify was, this was done too slowly, too late, was approached with insufficient commitment, and was spread too widely and too thinly. Too many products were attempted, leading to indifferent performances in each. It would have been wiser to have invested in a narrower range of strategic chemicals. The resulting low profits limited the amount of capital available for new processes, causing a downward spiral in the company's fortunes.

Because of the delay in rationalising and diversifying, UAC's reputation in the business world suffered. Its position in the money markets would have been stronger had its improvement exercise been seen to be carried out more efficiently and enthusiastically. It may well then have been possible to borrow money to finance the Castner (or another) electrolytic alkali process.

In 1895, a development took place which consolidated Castner's position and was to prove a major factor in the decline of UAC - the Castner Kellner Company was formed. A problem with Castner's original invention had become apparent when a full-scale plant was constructed. Also in 1892, the Austrian, Kellner, had patented an electrolytic alkali process which itself had a problem (but not the

same one as Castner's). Castner and Kellner joined forces, solved their problems and avoided expensive patent litigation.

An agreement was then made between the Aluminium Co, who held Castner's patent, and the Solvay Co, who owned the continental rights of Kellner's. They jointly financed the formation of the highly successful Castner Kellner Co, whose plants at Runcorn and Niagara Falls commenced production in 1897 and are still in operation today.

Brunner Mond did not invest in the new company at that time, although it was widely assumed that they had done so, because Solvay's name appeared on the prospectus [11.21]. However, in 1915, after Mond's death, Brunner Mond acquired a 25% interest in the Castner Kellner Co and, in 1920, complete control of the company [11.22].

In the late 1890s, the manufacture of alkali by the electrolysis of brine did become profitable, principally because of a substantial fall in the cost of electrical power: the main running cost of the process. A number of other electrolytic alkali processes, in addition to Castner's, were soon in commercial production, both in Britain and overseas, e.g. those of Hargreaves-Bird, Griesheim and Le Sueur.

After rejecting Castner's process, UAC continued to consider the possibility of adopting an electrolytic alkali process, but still had to be convinced that it would be a commercial success. Several other processes were examined by Hurter (see Chapter 10), but none was adopted. The Board minutes of 20 November 1897 record a discussion of the subject, when a letter from Robert Hasenclever was read in which he presented his views on the long-term prospects for the Castner Kellner process for manufacturing chlorine, compared to the Deacon-Hasenclever process [11.23].

In March 1898, the Board was still not greatly impressed by electrolytic alkali. At the Annual General Meeting, the Chairman, Brock, stated, albeit in a somewhat muddled fashion:

> Everybody nowadays after having neglected electricity [i.e. electrolytic processes] for a long time, thought
> that there was some magic in electricity and that goods could be produced with electricity provided that
> they had electricity to manufacture them with. This, however, is not the case. UAC had examined, to the
> best of their ability, every new [electrolytic] process that had been brought out, but they had not yet come
> to the conclusion that they [the operators of electrolytic processes] could manufacture cheaper than their
> company [UAC] could [11.24].

The words in parentheses are inserted for purposes of clarification.

By 1912 it was obvious even to the Board of UAC that the electrolytic alkali process had so improved, and was so profitable, that there was no longer any doubt as to its success. In 1914 the company

installed a number of Gibbs electrolytic plants, intending that the electrolytic alkali process should entirely replace Leblanc's. During the war, they benefited greatly from this decision, due to the increased demand for chlorine and caustic soda.

*   *   *

UAC's decision not to adopt the Castner electrolytic alkali process was based primarily on economic considerations. The enormous capital cost was well beyond their financial capability, whereas their existing Leblanc plants were already paid for and only attracted maintenance costs. Furthermore, they had already decided to commence the manufacture of ammonia-soda, which did not involve so great a technological leap, or such a large financial expenditure, as electrolysis. It is probable, therefore, taking all relevant factors into account, that UAC reached the correct decision for the company at the time.

Hurter had no control over the company's finances and, indeed, financial matters were outside his terms of reference. It is clear that little or no blame can be attached to him for the commercial demise of UAC following the Board's decision. His advice was soundly based, if somewhat conservative, and was entirely in accordance with what was required of him.

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CHAPTER 12

EDUCATION AND TRAINING

This thesis is mainly concerned with the value of Hurter's scientific and technical work to the British chemical industry. Away from the laboratory and production plant, one of his principal interests was the communication and acquisition of knowledge - education in its broadest sense. Through this medium he also made an important contribution to the nineteenth century chemical industry.

The following matters are examined in this chapter: Hurter's own formal education; his self-education; his membership of learned societies; education and training in the chemical industry; his lectures to the public; and his publications. His professional reputation was due to a large extent to his interests in the dissemination of knowledge and to his own high standard of education.

Throughout his life, Hurter never lost sight of the importance of educating himself. An obituary confirms this:

> He had the advantage of a sound and wide education during his student years; but to his credit it may be said that by strenuous and unremitting effort he continued his education during his whole life. He was an intellectual athlete and kept himself in splendid mental training by increasing practice [12.1].

His personal notebooks give details of his studies of new subjects which had not been covered at university and of new developments in subjects of which he already possessed some knowledge.

When he joined Gaskell Deacon, he had to familiarise himself with the operation of chemical process plant before he could begin inventing new processes or improving existing ones. He did this by leaving the environment of the laboratory and involving himself closely with practical process operations. Hardie records that:

> ... when Deacon was showing the famous chlorine plant to a distinguished visitor from Oxford he introduced a much-begrimed young man, struggling with a gas leak in one of the pipes, as "the chief of my laboratory, Dr Hurter" [12.2].

Lunge commented about Hurter's perception that self-education in mathematics and, by implication, theoretical physical chemistry, was essential to his work:

> ... the mathematical equipment [i.e. training] that Hurter possessed to a degree very uncommon among chemists, and still less among technical [i.e. industrial] chemists ... He had no opportunity, or inducement, for learning higher mathematics ... at Zurich, nor ... at Heidelberg. His having acquired higher mathematical knowledge later on by private study, ... is evidence both of a special gift in that direction, and of extraordinary energy [12.3].

The "In Memoriam" article published in the Journal of the Society of Chemical Industry shortly after Hurter's death remarks that:

> It is interesting to note that the extensive and useful applications of mathematics made throughout Hurter's work was almost entirely the outcome of his own teaching [12.4].

Hurter made considerable use of mathematics in both his chemical and photographic research. In 1920 the Royal Photographic Society commissioned an evaluation by H.S.Allen, professor of mathematics at Kings College, London, of some complex mathematics which Hurter had published. Allen concluded:

> ... there can be no doubt as to the correctness of the mathematical development given by Dr Hurter [12.5].

Hurter maintained his passion for self-education throughout his life. As late as 1897, he wrote to Driffield:

> I have read last summer the whole of a French work on chemical dynamics [12.6].

Membership of learned societies provided Hurter with a forum for disseminating his ideas and acquiring knowledge. The one with which he was most closely associated was the Society of Chemical Industry. The history of its formation is described by Herman [12.7], Hargreaves [12.8], Broad [12.9] and Hardie [12.10].

The SCI had, as its forerunners, a number of local organisations which were concerned with applied chemistry. In the North-West, foremost among these was the Faraday Club, which became the South Lancashire Chemical Society and later the Liverpool Section of the SCI [12.11]. The first meeting of the Liverpool Section was held on 30 November 1881, when Hurter read a paper entitled "The dynamic theory of the manufacture of sulphuric acid" and was elected to the committee [12.12].

Before the formation of the SCI, the most favoured means of communicating information in the chemical industry in Britain was to publish papers in the Chemical Trade Journal or Chemical News, although this was somewhat limited by the desire to preserve trade secrets. After the Society came into existence, papers could be presented and discussed at its meetings and be published in the *Journal of the Society of Chemical Industry*. From the first issue of the Journal the proliferation of papers submitted shows that the industrial chemists of Britain were impatient to share their technical knowledge with their peers. Hurter always regarded attendance at meetings of the SCI and the publication of papers in its Journal as his principal means of communicating his ideas and inventions to a wide audience.

His reputation in the chemical industry was not confined to the North-West. He was elected to a number of high offices in the national organisation of the SCI: he was twice Vice-President, Hon Foreign Secretary and a member of the Publications and Editorial Committees [12.13]. At a local level he was twice Chairman of the Liverpool Section and Chairman of its Editorial and Revision Committees.

His enthusiasm for the work of the SCI was unbounded. He worked tirelessly for the society until the mid-1890s, when his failing health and the high work load at UAC forced him to slow down. Although he retained his membership of a number of committees and sub-committees, his attendance at general meetings began to decline, but he was still active in the Society at the time of his death [12.14].

Such was the esteem in which Hurter was held by his contemporaries that, after his death, there was a desire to perpetuate his memory. The Liverpool Section was given the task of deciding how this might best be done. It first proposed an annual lecture under the auspices of the national body, but this idea was later withdrawn and it was agreed that the Liverpool Section would endow a biennial memorial lecture locally [12.15]. This was the first of the Society's endowed memorial lectures and it is fitting that it should be associated with the Liverpool Section, for which Hurter had worked so enthusiastically. The Hurter Memorial Lectures were unique within the SCI, being the only ones to be established in memory of a particular member, demonstrating the high regard in which Hurter was held.

Hurter's enthusiasm for learned societies was not confined to industrial chemistry. In 1890 he became a member of the Liverpool Physical Society almost by accident [12.16]. He was originally a member of the University College Photographic Society because of his interest in photographic science [12.17]. He presented a paper to that Society entitled, "Recent photo-physical and photo-chemical investigations" [12.18]. When, in 1890, the University College Photographic Society amalgamated with the Liverpool Physical Society [12.19], Hurter automatically became a member of the latter [12.20]. At the same

time, the Photographic Society became a section of the Liverpool Physical Society, with Hurter as section Chairman. His inaugural address was entitled "The action of light on the sensitive film" [12.21].

Hurter took part in all the activities of the Liverpool Physical Society (not only the Photographic Section) with such enthusiasm that he twice became its Vice-President [12.22], [12.23] and later President [12.24]. His presidential address: "On Electrolysis" [12.25] (see Chapter 10) aroused a great deal of interest and was discussed at subsequent meetings [12.26], [12.27].

Hurter relinquished the Presidency in 1894, reverting to Vice-President [12.28]. His attendance at subsequent meetings was infrequent but, at a meeting in January 1897, he presented his last paper to the society entitled: "Van der Waal's equation". In 1897, the society amalgamated with the Liverpool University College Physical Society, of which Hurter remained a member until his death.

The records of the Liverpool Physical Society give some valuable information about the state of Hurter's health in the 1890s. He was unable to attend a Council Meeting in March 1894 due to illness, but a letter from him was read out:

I am sorry to have to ask you to place my resignation in the hands of the committee and to ask them to ... elect a new president. It has given me pain, that I was unable to fulfil my duties to the Physical Society more efficiently...

Unfortunately my medical advisers are of opinion that I shall not be able again to use my voice at public meetings and that my health will not be sufficiently robust to allow me to undertake any but the most necessary duties [12.29].

Compared to their continental counterparts, Victorian industrialists were late in realising how essential technical education and training were to their prosperity.

In the early years of the British chemical industry, up to the mid-nineteenth century, the leading personalities in it rarely received a formal education in chemistry. The best that could be hoped for was training as an apothecary, and even this was rare. Many of were self-taught amateurs, with a flair for exploiting the commercial potential of the new chemical processes. For example, Josias Gamble began his career as a minister of the Presbyterian church; James Muspratt was apprenticed to an apothecary and manufacturing chemist, and studied practical and theoretical chemistry at home; Henry Deacon was apprenticed to a mechanical engineer and tool maker, and Walter Weldon was a poet and journalist.

By the second half of the nineteenth century, however, a new generation of scientifically-trained industrial chemists was emerging. They were often of continental nationality, their formal education in chemistry having been obtained at German universities, e.g. the schools of Liebig at Giessen and Bunsen at Heidelberg. Because the British chemical industry was more developed technically and

commercially than that of most other European countries, German-trained chemists were attracted here. Hardie and Pratt confirm this:

Only during the last quarter of the 19th century did a few continentally trained chemists, notably George Lunge and Ferdinand Hurter, initiate the application of physical chemical methods to the study of the more important processes of the Leblanc system [12.30].

German-educated *British* chemists were comparatively rare. Towards the end of the century, education in industrial chemistry to university standard became more available in Britain [12.31]. George Davis was an example of a chemical engineer who was trained in this way. Lunge, Mond, Muspratt and Davis were all contemporaries of Hurter who spent some time in the Leblanc industry. They were of the "new generation" of industrial chemists who had received this high standard of scientific education, three of them in Germany and one in Britain.

The perceived need for better educated chemists and engineers began to result in improvements to the standards of technical competence of managers in the British chemical industry. Donnelly's account of the training and qualifications of the managerial staff of a number of Leblanc firms in the Widnes area confirms this [12.32].

Hurter was interested in the education of the working classes, particularly in scientific and technical subjects. Edmund Muspratt shared this interest and, when he stood as parliamentary candidate for Widnes in 1885, Hurter supported him with a speech in favour of free education:

I have great pleasure to support Mr Muspratt, who promises to do his best for an improved system of education, so badly needed in this country [12.33].

His views on education were probably influenced by the fact that, despite his childhood having been spent in somewhat reduced circumstances, he was able to receive a high standard of education in Switzerland. He pointed out that:

... The schools are open to all classes, and it is owing to this that I, the son of a widow who had to work for her living, am to-day able to call myself a doctor ...

He drew attention to the advantages which a free education system could bring to Britain:

> ... Switzerland, which has neither coal nor iron, neither ships nor harbours, has this day an industry and a railway system which, considering the small population, is only second to the industry of this country. ...

Hurter's enthusiasm for technical education was generally shared by his colleagues in the SCI. In his Presidential address in 1886, Muspratt expressed the desire of chemical manufacturers for an improved system in this country:

> ... I look forward to the not too distant day when, by means of improved secondary education and the work of institutions like Owens College and the University College Liverpool, our manufacturers will be able to find men ready to carry on with even greater success than in the past a large portion of the great chemical industry of the world [12.34].

In the North-West, Prof J Campbell Brown, in his Chairman's address to the Liverpool Section in 1886, expressed an opinion which is still relevant today:

> If everyone who is to be engaged in chemical manufactures were to pass through a course of training such as it is my intention to sketch tonight ... the waning or lost supremacy of this country in manufacturing industry, of which we hear so much, would I think soon re-assert itself [12.35].

Muspratt and Hurter expressed their agreement with these sentiments [12.36].

In 1889 Hurter, in his position as Chairman of the Liverpool Section, developed the theme further when he introduced the discussion, "In what direction is state aid needed to assist technical education in chemistry". The meeting resolved that it was not desirable to introduce "special technical instruction" into elementary schools, but that secondary schools should embrace the technically important sciences. Furthermore, chairs of technical chemistry and allied subjects should be endowed at universities and colleges, with the appropriate supporting infra-structure [12.37], [12.38]. At a local level, the Leblanc manufacturers in Widnes gave substantial financial support to the setting up of University College, Liverpool and the Widnes Science and Art Classes for the part-time education of their workers.

The need for proper training of process managers and operatives is well-accepted in modern-day industry, but Hurter was innovative in not only realising the need for such training, but also persuading his employers to spend money on it without the prospect of immediate commercial rewards.

In 1893 he successfully requested the Board of UAC to provide the not inconsiderable sum of £7,000 to finance the construction of an experimental pilot plant for the manufacture of potassium chlorate by electrolysis [12.39]. In addition to its value in developing the new process, the plant had the important function of training process operatives and managers in electrolytic process technology. Hurter emphasised the importance of funding such training:

> In handing you this estimate of cost of an electrolytic plant I must emphatically insist upon the Directors keeping before their eyes that such a plant will never make money but will always be a source of expenditure. [One reason] for creating such a plant [is that] whatever may ultimately be the fate of electrolysis as a manufacturing operation, only those firms are likely to succeed which can command a staff practically acquainted with the handling and maintenance of large dynamos and currents, and who are able to pre-calculate and design electrolytic cells. Such a staff requires education, and as there are few establishments in the world where this education can be obtained, there is no other way open to us but that of establishing such a school for ourselves where we may train younger men to this method of manufacturing chemicals.

The pilot plant successfully achieved its objectives of providing this training for the managers and operatives, and at the same time contributing to the development of the potassium chlorate process.

Hurter's interests in education were broadly based. He was an enthusiastic teacher, lecturing to the Widnes Science and Art Classes in the 1880s. He also gave public lectures on popular science. "The Philosopher's Stone", a lecture on alchemy, included an authoritative account of the history of chemical manufacturing from the times of the ancient Greeks and early Egyptians up to the nineteenth century [12.40]. Other examples were: "The Discovery of Nitrogen - an Historical Sketch"; "The History and Uses of the Barometer" and "The Elements of Meteorology" [12.14], [12.41]. There was also a more academic presentation: "Sulphuric Acid Manufacture", which is discussed fully in Chapter 5.

One important way in which Hurter influenced the development of the chemical industry was through his prodigious output of published work. Early in his career he favoured the German industrial chemistry publication: Dingler's Polytechnisches Journal, in which he published seven papers, and the British journal, Chemical News, in which he published four. When the SCI was formed, he transferred his allegiance to its Journal and, during the fifteen years of his membership, presented twenty three formal papers at meetings and made many major contributions to the discussions of other people's papers [12.42]. These contributions were rarely just a few brief remarks; they were lengthy and erudite, demonstrating a range of knowledge and constructive ideas on subjects on which he would not

necessarily have been expected to possess expertise. They included: patent law, printing indigo on calico, river and air pollution legislation, wood for paper-making, home-grown sugar, tanning colours, refrigeration theory and practice, and micro-organisms in fermentation processes.

Broad comments on the number and high quality of Hurter's publications:

> ... the many masterly papers presented by Hurter on sulphuric acid, caustic soda, chlorine, or hydrochloric acid, to name but a few. All are ... eminently worthy of the man ... so gifted in the art of exposition ...

> ... the many, and always popular, Hurter papers on all manner of Leblanc topics [12.43].

When Hurter was head of the UAC Central Laboratory, his enthusiasm for disseminating knowledge led him to impress upon his staff the need to communicate their work widely. As a result, they published numerous papers which have provided valuable information on the work carried out there.

The publications of Hurter and his contemporaries provide a valuable insight into the way in which technical knowledge was communicated in the nineteenth century. The papers were often long and detailed, which makes them all the more impressive since they were usually written by working industrial chemists and managers. It is hard to visualise how, in addition to their normal duties, they found time to produce their papers, often at short notice. Good examples of this can be found in the JSCI [12.44], [12.45].

Hurter's extensive range of patent specifications contain a wealth of chemical and engineering information about his work. He was the author of thirty two British and four US patents; most of them solely in his name. They are meticulously written and often include detailed engineering drawings of plant and equipment. The clarity of these patents is so impressive that many of them virtually constitute full instructions for constructing and operating a new process plant. This is very much in contrast to many modern patents, which seem designed more to confuse rather than educate the reader.

Despite his prolific output of published papers and patent specifications, Hurter appears never to have written a book. Information on the matter was found in a letter dated 7 August 1918, which was written by his daughter Annie to W B Ferguson of the Royal Photographic Society, who was cataloguing the collection of documents on Hurter and Driffield's photographic research [12.46]. Annie sent Ferguson some documents and remarked that:

> There are two notebooks which look like the sketch for a book on Chemical Engineering commenced in 1883 but evidently not published, and another on Thermo-chemistry. If it would interest you to see them I could send them on.

It is of course possible that the notebooks referred to were simply notes made by Hurter on various subjects. She may have mistaken them for the drafts of books. Much of Hurter's work was concerned with chemical engineering and thermo-chemistry and it is logical that the books which he would have been most interested in writing would have been on those subjects. It appears, though, that they were never published and, regrettably, the notebooks referred to by his daughter have not survived. A number of Hurter's laboratory notebooks which were obtained from other sources contain studies of thermodynamics and of the thermo-chemistry of processes, possibly as a preliminary to the writing of a textbook [12.47], [12.48].

Hurter was involved in the production of two books, but not as their author. In 1883 Lunge published his *Taschenbuch fur die Soda, Potasche und Ammoniak Fabrikation*, written for the German Society of Alkali Makers [12.49] to [12.51]. In 1884 an English edition was published, entitled The Alkali-Makers' Pocketbook [12.52]. This was a complete revision, in which Hurter assisted by translating the original book from German into English, contributing several new chapters and converting the tables from metric to imperial units. The Preface gives credit to him:

> To him is due the extensive labour of recalculating all the tables for English weights and measures. In making these calculations every one of the tables was, as far as possible, reconstructed from the original data by graphic interpolation. Errors of computation were avoided as much as possible by the use of Thomas' Arithmometer and Fuller's Calculator ....

The English translation appears to have been well-received. In 1884, Professor Roscoe, of Owens College Manchester, wrote to Hurter in 1884 praising his efforts:

> I beg to thank you for your alkali makers' pocketbook just received. I feel sure that it will be much used and anything coming from yours and Prof. Lunge's pen will command confidence amongst all who use the book [12.53].

The work must have afforded Hurter the satisfaction of exercising his qualities of painstaking attention to detail. The book was very successful, being revised and re-published in 1891 as the Alkali Makers' Handbook [12.54].

In 1897, Hurter commenced translating from French to English the book, "Outlines of Physical Chemistry", by A Reychler, but he was able to complete only part of the work before his death. There was a dedication to him in the preface and, in letters of condolence to his daughter and wife, Reychler acknowledged his debt to Hurter [12.55].

The outstanding reputation which Hurter enjoyed among both his contemporaries and latter-day historians was due to a great extent to his published work:

> Hurter carried out the study of the theoretical and practical problems connected with the [Deacon] process in a manner which will always remain memorable, and will never be surpassed as an example of the application of scientific methods to manufacturing problems.   Mond [12.56].

> In looking over the long list of Hurter's activities and accomplishments, one cannot help regretting that we had not the advantage of his great abilities when faced with so many difficult and important industrial problems [during the 1914-1918 war] .... There can be no difference of opinion as to the immense value of Hurter's contribution to the technology of chemistry .... He seems to me to represent the highest type of industrial chemist or chemical engineer, whose example should be an inspiration to all of us.
> McNab [12.57].

> He was one of the outstanding figures of the chemical industry of his day.
> Jubilee edition of JSCI, 1931 [12.58].

> By 1880 his reputation was already established as a world authority on the alkali industry.
> Broad [12.14].

> ... he had shown the way of bringing the exactitude of the laboratory to bear upon investigations in the works, and from beginning to end his worth had been of an essentially practical character.
> Smetham [12.59].

> ... Dr Hurter, who took a very great interest in the application of physical methods to technical problems, about which at his time little was known.   Baly [12.60].

One of the principal disciplines to which Hurter made valuable contributions was chemical engineering. Although he was by education a theoretical chemist, he can certainly be regarded as a member of the newly emerging chemical engineering profession. In his history of chemical engineering in Britain, Donnelly shows that it was hardly recognised before the 1880s; in 1882 only five per cent of the members of the Society of Chemical Industry described themselves as chemical engineers, although many papers on the subject were being published at the time [12.61].

Hurter's higher education included theoretical and practical chemistry but, it appears, no applied or process chemistry [12.62]. When he joined Gaskell Deacon in 1867, he was described as Works Chemist, a less prestigious title than Works Manager. At that time a Works Chemist would be engaged mainly on routine analytical testing, with limited managerial status, even though he may have

been well-qualified [12.63]. To Henry Deacon's credit, he soon recognised Hurter's abilities and encouraged him to undertake process design and development (i.e. chemical engineering) [12.64].

Hurter's first major project at Gaskell Deacon was to develop the Deacon chlorine process (see Chapter 3) and this, and many subsequent projects, had a considerable chemical engineering content. His notebooks and published work demonstrate his sound knowledge of the basic precepts of chemical engineering: heat transfer, thermodynamics, reaction kinetics, fluid flow and process economics [12.65]. By the 1880s he was producing designs and calculations of a standard which would be acceptable in the present century. If he did, in fact, write the draft of a book on chemical engineering, it is probable that he regarded himself at least in part as a qualified chemical engineer.

There is little doubt, therefore, that Hurter could claim to be one of the earliest chemical engineers and, in view of his theoretical approach to the subject, one of high professional standing. Hardie concurs with this:

We should classify Hurter as an early chemical engineer, with his painstaking efforts to apply his mathematical as well as chemical knowledge to the Leblanc process [12.66].

Broad agrees:

... [Hurter] introduced into his work procedures for using the new science of thermodynamics and the study of heat balances to design for optimum process conditions [12.14].

Further confirmation is found in the Hurter Memorial Lecture delivered by McNab in 1922, the year in which the Institution of Chemical Engineers was formed:

There can be no difference of opinion as to the immense value of Hurter's contribution to the technology of chemistry, whether we consider his studies of the means of promoting action between gases and liquids which were conducted in such a scientifically systematic manner, or his work in connection with the manufacture of chlorine, or indeed any of his many activities. He seems to me to represent the highest type of industrial chemist or chemical engineer, whose example should be an inspiration to all of us.

I cannot but think that he would have approved of this new Institution [of Chemical Engineers], and if it takes him as an ideal after which to strive to fashion its members, chemical industry will benefit and Hurter's memory be kept greener than ever [12.57].

Hurter's outstanding abilities as an industrial chemist and chemical engineer having been established, the information that he was held in high regard by the academic leaders in *pure* chemistry comes as something of a surprise. The evidence for this was found in the archives of the Royal Photographic

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Society, in a letter written just after Hurter's death by James Cadett, Managing Director of Cadett & Neal, photographic plate makers, to Driffield, Hurter's photographic co-worker. It states:

> Last Friday I was informed in confidence by a very eminent chemist that he was supporting the candidature of Dr Hurter for the Fellowship of the Royal Society, and was told that the support received was such as to almost ensure Dr Hurter's election, and it is very sad to think that this greatest honour which the scientific world can bestow was thought of too late [12.67].

The words speak for themselves. It is not clear whether the honour was to be bestowed for Hurter's work in industrial chemistry or photographic science, since in both disciplines he had ably demonstrated his abilities in theoretical chemistry. It was unusual that an FRS should be considered for someone who was an applied rather than a pure scientist, which makes his achievement all the more outstanding.

Lunge recommended Hurter for an academic post on several occasions:

> I exerted myself (and that not for the first time) to recommend him for a professorship of technological chemistry ... as the best candidate I could propose [12.68].

It is clear that it was Lunge's belief that Hurter would have been successful in an academic career, had he so wished.

*   *   *

Hurter's enthusiasm for education and the dissemination of knowledge took many forms. As a result, his own work became widely known and applied, and he was able to influence the provision of education and training in the widest sense, particularly in the chemical industry. His formal education, coupled with his continuing interest in educating himself throughout his life, ensured that he was always fully informed about the latest developments in his discipline, a fact that was widely acknowledged by his contemporaries.

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12.61   J Donnelly, "Chemical engineering in England" *Annals f Science*, 45, (1988), pp. 555-590.

12.62   Hurter's university certificates. CRO file DIC/X10/119.

12.63   J Donnelly, "Consultants, managers and testing slaves: changing roles for chemists in the British alkali industry, 1850-1920", *Technology & Culture*, 35, (1994), p. 109.

12.64   Hardie & Pratt, op cit [12.30], p. 32.

12.65   Hurter's laboratory note-books. CRO file DIC/UA9/1/4.

12.66   D Hardie, "Two immigrants", *Chemistry & Industry*, 22 April 1967, p. 627.

12.67   Letter from J Cadett to V Driffield 7 March 1898. RPS file Hurter Misc.

12.68   G Lunge, "Impending changes in the general development of industry and particularly the alkali industries". First Hurter Memorial Lecture, *JSCI*, XVIII, (1899), p. 892.

CHAPTER 13

CONCLUSIONS

The purpose of this thesis has been to evaluate Ferdinand Hurter's scientific and technical contributions to the British chemical industry in the second half of the nineteenth century, and to assess his qualities as an industrial chemist and one of the earliest chemical engineers. To provide the framework within which this study has been carried out, the economic and technological factors which influenced the changing fortunes of the Leblanc, ammonia-soda and electrolytic alkali industries, during the period of Hurter's involvement with them, have been examined and discussed.

Hurter's work on the improvement of chemical processes was driven by the need for the Leblanc alkali industry to be more efficient in the latter part of the nineteenth century. He was fortunate to have entered the industry at a time when it was just beginning to develop scientifically from a regime in which its processes had been operated largely empirically. Because there was little serious competition, there had been little incentive to make improvements. Hurter's period in the industry was that of its greatest change regarding both the range and quality of its products and the efficiency of its processes. This was essential in the face of competition from a number of sources, particularly the ammonia-soda and electrolytic alkali processes. From the 1880s, the Leblanc industry began to decline, despite efforts made to prevent this, but Hurter's work undoubtedly contributed to prolonging its life.

Why did Hurter embark upon an industrial career when he left university? He declined an academic post, where it would have possible for him to carry out fundamental research. Because of his abilities, he may well have achieved a degree of eminence in the academic world far greater than he did in the less glamorous environment of the alkali industry.

His reasons may be traced back to his formative years in the dyeing industry. This was later to become technologically advanced following the invention of synthetic dyestuffs but, when Hurter was involved in it, natural dyes were employed and the process techniques were largely empirical. His impatience with these may have encouraged him to obtain a proper education in chemistry, and to enter an industry where there was the challenge of applying his scientific knowledge and intellect to the solution of practical problems. His Doctorate at Heidelberg did not include a research project, so he

may have felt that he would be able to carry out applied research, applying his talents over a wide range of processes and disciplines.

Hurter's principal objective thoughout his career was to apply theoretical chemistry and physics to the development and improvement of chemical processes. When he entered the Leblanc industry, subjects such as thermo-dynamics and reaction kinetics were relatively new, and mainly academic, concepts; they were rarely applied to the study of chemical processes. From his earliest days at Gaskell Deacon, Hurter was implementing this philosophy. Although hindsight shows that his understanding of some of the theoretical concepts was not always entirely correct, it should be remembered that many of them were still in their infancy. He should be credited with the fact that he was one of the first industrial chemists to be far-sighted enough to make use of them.

The procedure which he established was first to study the theory of a process: its mechanism, reaction kinetics and thermo-dynamics. He then carried out laboratory experiments to investigate its basic chemistry and physics, constructed a pilot plant and, using results obtained on this and in the laboratory, designed the full-scale plant. Finally, he carried out commissioning trials to establish optimum operating conditions. His use of pilot plant studies was innovative and his frequent references to it suggest that he either invented the technique, or at least was one of its earliest users. It is standard practice in modern-day process design, when teams of scientists and engineers may be involved.

On new technical and scientific matters, Hurter was always ready to propose an explanatory theory, even if in hindsight it transpired that he was not entirely correct. He was not afraid of attempting to advance knowledge on a subject, often when no-one else was prepared to do so. He seldom ventured an opinion, or criticised another person's work, without a sound scientific basis for this. He was always ready to carry out lengthy and detailed experimental work to support such an opinion, and he was widely respected for this.

Hurter could perhaps be criticised for the fact that his work was rarely pro-active, in either an academic or practical sense. This may be explained by the fact that, throughout his career, he was never his own master, having only a limited choice of the type of work which he was required to do. Most of his work was, accordingly, reactive: to improve an existing process or develop a new one. Because he worked in the alkali industry during the period of its greatest change, the need for reactive research was immense. He did make the opportunity to carry out a some pro-active research: examples include his studies of the reaction kinetics of the sulphuric acid process and the mechanism of gas-liquid absorption.

In his first major project, the development of the Deacon chlorine process, Hurter began to evolve from a laboratory-based chemist to a chemical engineer. The ability do this was greatly to his credit, since

his university education appears to have included very little applied chemistry, or engineering. He had to educate himself in pilot plant studies, including scaling-up from laboratory data; the chemical engineering design and optimisation of full-scale plant; heat transfer and fluid flow; and process economics. Because chemical engineering was a comparatively new discipline, little published data on the design of chemical plant and equipment existed and Hurter often had to derive these from first principles. A valuable part of his work was the introduction or development of many of the standard techniques in use in chemical engineering today, including heat transfer, thermo-chemistry and gas-liquid absorption. Within ten years he was producing chemical engineering designs which could stand comparison with those of the twentieth century.

It is difficult, from among Hurter's many achievements, to select the most outstanding ones; the danger of trying to do so is that those omitted may be undervalued. It is felt that the following projects best illustrate his abilities and the value of his work: the Deacon chlorine process; bleaching powder manufacture; the lead chamber sulphuric acid process; the absorption of gases by liquids; electrolytic alkali processes; and the manufacture of chlorates.

The reputation which he gained at Gaskell Deacon was a major factor in his being appointed Chief Chemist of UAC. This was a water-shed in his career and it revealed a new aspect of his character: the ability to establish and manage a complex laboratory organisation, an opportunity which he never had at Gaskell Deacon. With characteristic enthusiasm and efficiency he carried out the task virtually single-handed and was widely complimented for his achievement. Central Laboratory was unique in the chemical industry in the 1890s; it was to develop into the extensive and prestigious research facility of ICI in the next century. Hurter's status at UAC was higher than it had been at Gaskell Deacon. He was responsible for recruiting, training and supervising staff which included a number of graduates, and for reporting and discussing the work of the department the directors and senior management of UAC.

Hurter's talents were wide-ranging. He was a meticulous laboratory worker at a time when laboratory techniques depended very much on the manual skill of the operator. His practical expertise in the laboratory included the construction of laboratory equipment and early photographic instruments. The Central Laboratory report books show that, even in his senior position as Chief Chemist, he still carried out practical laboratory work. He had included a laboratory exclusively for his use in the specification for Central Laboratory, showing that he did not intend being solely an administrator. It is to his credit that he considered it important to be involved in practical matters.

Hurter did not devote all his time to industrial chemistry. His interests away from the factory were numerous, one of the foremost being education, particularly in technology. He was an enthusiastic

member of several learned societies. He was a founder member of the SCI, in which he served in various official posts, as he also did in the Liverpool Physical Society.

Hurter was generally well-regarded by his contemporaries and by historians. It is surprising, therefore, that Hardie wrote in 1951:

> It is a tragedy of Ferdinand Hurter that, professionally, his great scientific abilities had to be given to the sterile bolstering and defence of the dying Leblanc system. A kinder fate and a longer life might have permitted him to devote his energies to the modern developments in the chemical industry in which, with his chlorate cell, he had shown himself in his last years to be a potential pioneer [13.1].

Whilst this is not exactly a criticism of Hurter, it only really applies to the last few years of his life. Hardie implies that his professional life was wasted, and gives him little credit for the many valuable contributions which he made to the Leblanc industry during its earlier, more prosperous, years.

Because of his life-long association with the Leblanc process, Hurter was accused of being unduly loyal to it and that this loyalty adversely affected his attitude towards other alkali processes. This accusation has been shown to be unfair. His professional integrity and honesty would not have permitted him to insist that, in the 1880s, the Leblanc alkali process was still superior to the others, when this was demonstrably not so. The fact that he invented his own version of the ammonia-soda process, and carried out extensive studies of electrolytic processes, confirms his impartiality.

The accusation of over-loyalty to the Leblanc alkali industry was particularly made when it was alleged that Hurter advised UAC against adopting Castner's electrolytic alkali process and, as a result, caused the demise of the industry. This is one of the few aspects of his career which has been written about with any emphasis by historians, particularly Reader and Hardie. Hurter could not be blamed for UAC's rejection of Castner's process. The factors which influenced its decision were many and complex and most of them were outside his terms of reference as Chief Chemist.

The methods of the industrial chemists in the early part of the nineteenth century owed little to theory; their approach was mainly empirical. If, eventually, the desired product emerged, little attention was paid to its quality, or to the economics and chemistry of the process. Taking a broad view of the period, it would be easy to overlook Hurter's work, but this would be a mistake, since his approach to industrial processes was intrinsically, and untypically, modern. He applied theoretical chemistry and physics to chemical processes, so improving the efficiency and competitiveness of the Leblanc industry. The industry could not have hoped to compete in the long term with the ammonia-soda and electrolytic alkali processes. It is fair to say that its profitability was increased, and its productive life extended, because of Hurter's work.

This having been said, Hurter was inevitably a person of his own time. Thus, in spite of his new approach and the recognition which was given to him in his lifetime, he existed in an industry which to modern eyes must seem archaic. In judging him, therefore, it is fair to ask the reader to consider the environment and the time in which he operated, and to reach a conclusion based upon the effect which he had in changing and improving procedures and, above all, in bringing those procedures closer to present-day scientific methods. For this is Hurter's most important contribution to the history of technology.

It is fitting that the final words of this thesis should be taken from the "In Memoriam" article which was compiled by Hurter's colleagues in the alkali industry and published after his death:

> His wide knowledge and consummate skill in bringing the highest branches of pure science to bear upon the details of practical working were unique, whilst his ardour and capacity for work were only equalled by the masterly grasp and enthusiasm with which he communicated his discoveries [13.2].

References

13.1   D Hardie, *Chemist & Druggist*, 17 March, 1951, p. 355.

13.2   "The life and work of Ferdinand Hurter, Ph.D.", *JSCI*, XVII, (1898), p. 406.

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APPENDIX III

AMMONIA-SODA AND LEBLANC SODA PRODUCTION COSTS

Date	Ammonia-soda	Leblanc	Ratio a-soda to Leblanc
1872 [a]	£7.8.0	£9.11.6	0.77
1883 [b]	£4.0.0	£6.8.0	0.62
1894 [c]	£2.18.9	£3.4.0	0.92

Cost is for one ton of sodium carbonate per annum

Sources

[a] Average of three similar estimates made by Brunner and Mond during their first year of production.

[b] Weldon, *On Mr Davis' paper on economy in the manufacture of bleaching powder*, 1st ed., (London, Wyman & sons, 1883) p4.

[c] Editorial, "The crisis in the alkali trade", *Chemical Trade Journal*, XV, (1894) p270.

APPENDIX IV

ALKALI PRODUCTION 1880-1890

Date	Leblanc	Ammonia-soda	Total
1880	218	19	237
1881	196	20	216
1882	191	39	230
1883	186	53	239
1884	167	61	228
1885	152	78	230
1886	136	85	221
1891	90	180	270

Quantities are 1,000 tons per annum of 100% sodium carbonate.

Sources

[a] For 1880 to 1886: A E Fletcher, *On the present position of the alkali manufacture*. Paper to the British Association meeting, Manchester 1887.

[b] For Leblanc 1891: R H Davidson, ICI internal memorandum, 1932. *Some notes on the U. A. Co. and its constituent companies*, CRO file no. DIC/X10/UA Hist.

[c] For ammonia-soda 1891: L F Haber, *The chemical industry during the 19th century*, 2nd ed., (Oxford, Clarendon Press, 1958) p158.

APPENDIX V

BLEACHING POWDER PRODUCTION 1877 - 1915

Date	Quantity produced
1877	88
1878	87
1879	96
1880	109
1881	136
1882	112
1883	118
1884	107
1885	111
1886	113
1887	117
1888	118
1890	127
1891	132
1892	131
1893	113
1894	104
1895	101
1896	87
1897	86
1898	83
1899	94
1900	91
1910	80
1915	50

Quantities are 1,000 tons per annum

Sources

[a] A E Fletcher, *On the present position of the alkali manufacture*. Paper to the British Association meeting, Manchester 1887.

[b] UAC production statistics ledger. CRO file no. DIC/UA5/4

APPENDIX VI

COMPANIES FORMING THE UNITED ALKALI COMPANY

Acquired on formation, November 1st 1890

Seaham Chemical Co, Seaham Harbour

Henry Baxter, St Helens

Hay, Gordon & Co, Widnes

Mort, Liddell & Co, Widnes

Sutton Lodge Chemical Co, St Helens

Wallsend Chemical Co, Wallsend on Tyne

Hall Bros & Shaw, Widnes

N Mathieson & Co, Widnes

P A Mawdsley, St Helens

Hazlehurst & Sons, Runcorn

A G Kurtz & Co, St Helens

Thos Walker, St Helens

J Muspratt & Sons, Widnes and Liverpool

St Bedes Chemical Co, Newcastle upon Tyne

Newcastle Chemical Works Co, Newcastle upon Tyne

R J & W H Richardson, Newcastle upon Tyne

J Hutchinson, Widnes

Greenbank Alkali Co, St Helens

Sullivan & Co, Widnes

Atlas Chemical Co, Widnes

Liver Alkali Co, Widnes

J McBryde & Co, St Helens

Wigg Bros & Steele, Runcorn

Runcorn Soap & Alkali Co, Runcorn

Weston Works, Runcorn

T Snape & Co, Widnes

Globe Alkali Co, St Helens

Netham Chemical Co, Bristol

Gaskell Deacon & Co, Widnes

Jarrow Chemical Co, Jarrow

Widnes Alkali Co, Widnes

Boyd Son & Co, Dublin

Dublin & Wicklow Manure Co, Dublin

Morgan Mooney, Dublin

W Pilkington & Sons, Widnes

Eglington Chemical Co, Irvine

North British Chemical Co, Glasgow

C Tennant & Co, Glasgow

Heworth Alkali Co, Newcastle upon Tyne

Golding Davis & Co, Widnes

Muspratt Bros & Huntley Co, Flint

Irvine Chemical Co, Irvine

St Helens Chemical Co, St Helens

C Tennant & Partners, Hepburn upon Tyne

Duncan McKechnie, St Helens

T C Gamble & Co, St Helens

Hardshaw Brook, St Helens

Fleetwood Salt Co, Fleetwood

Acquired after formation

Ammonia soda works, Fleetwood (1892)

Tharsis Sulphur & Copper Co, Widnes (1894)

Clyde Wharf Works, London (1896)

North American Chemical Co, Bay City, USA (1896)

Wimboldesley Estate, Wimboldesley (1900)

Tinto Santa Rosa Mines, Spain (1903)

Sotul Coronado Mines, Spain (1905)

W Henderson & Co, Irvine (1906)

Buitron Railway, Spain (1906)

Buitron, Poderosa & Conception Mines, Spain (1906)

J B Aitken, Widnes (1916)

Power Station, Widnes (1918)

Raynes & Co, Hysfaen (1920)

Source CRO file: DIC/X10/471   (Date acquired shown in brackets)

APPENDIX VII

DIRECTORS OF THE UNITED ALKALI COMPANY AT ITS FORMATION

Name	Company
C Tennant	C Tennant & Co
E Sullivan	Sullivan & Co
J Dennis	Globe Alkali Co
Holbrook Gaskell	Gaskell Deacon &Co
J Huntley	Muspratt Bros & Huntley Co
J Brock	Sullivan & Co (Chairman)
C Wigg	Runcorn Soap & Alkali
J Stevenson	Jarrow Chemical Co
T Alexander	C Tennant & Co
A Allhusen	Newcastle Chemical Works
C Barlow	Widnes Alkali Co
E Baxter	Henry Baxter & co
J Davidson	Newcastle Chemical Works
Holbrook Gaskell Jnr	Gaskell Deacon & Co
J Gaskell	Gaskell Deacon & Co
W Menzies	Greenbank Alkali Works
E Muspratt	J Muspratt & Sons
G Pilkington	W Pilkington & Sons
R Shaw	Hall Bros & Shaw
J Rayner	A G Kurtz & Co
J Tennant	C Tennant & Co
G Wigg	Wigg Bros & Steele
P Worsley	Netham Chemical Co
D Gamble	Josiah Gamble & Sons
W Gamble	Josiah Gamble & Sons
D McKechnie	Duncan McKechnie

Source CRO file: DIC/X10/471

APPENDIX VIII

AMMONIA-SODA PRODUCTION 1890-1923

Date	Brunner, Mond	UAC	Ratio UAC/Brunner, Mond %
1890	142	15	11
1891	155	15	10
1892	167	20	12
1893	180	71	39
1894	192	72	38
1895	205	72	35
1900	220	57	26
1905	270	67	25
1510	320	92	27
1915	500	128	26
1920	600	172	29
1923	630	200	30

Quantities are 1,000 tons per annum

1890-1892 UAC Mathieson works only.

1893-1896 UAC Mathieson and Fleetwood works together.

1897-1923 UAC Fleetwood works only.

Sources

[a] Brunner, Mond & Co. figures: W F L Dick, *A hundred years of alkali in Cheshire*, 1st ed., (London, ICI Ltd, 1973) p121.

[b] UAC data: R H Davidson, ICI internal memorandum, 1932, *Some notes on the U. A. Co. and its constituent companies*, p3, CRO file no. DIC/X10/UA Hist.

APPENDIX IX

ALKALI PRODUCTION 1891-1915

Date	Leblanc	Ammonia-soda	Total
1891	71	157	222
1892	57	170	227
1893	53	251	304
1894	48	264	312
1895	42	277	319
1900	12	277	289
1905	9	337	346
1910	6	412	418
1915	1	628	629

Quantities are 1,000 tons per annum of 100% sodium carbonate.

Sources

[a] Ammonia-soda: W R L Dick, *A hundred years of alkali in Cheshire*, 1st ed., (London, ICI Ltd, 1973), p121, and R H Davidson, ICI internal memorandum, 1932, *Some notes on the U. A. Co. and its constituent companies*, p3, CRO file no. DIC/X10/UA Hist.

[b] Leblanc: UAC production statistics ledger. CRO file no. DIC/UA5/4

[c] Some notes on the U. A. Co. and its constituent companies, p3, CRO file no. DIC/X10/UA Hist.

APPENDIX X

FERDINAND HURTER - A PERSONAL PROFILE

If the term workaholic had existed in the nineteenth century, then it would certainly have applied to Hurter. On his own admission, he was obsessed with work. In a letter to a member of his family, written in 1896, when illness dominated his life and was beginning to affect his work, he wrote:

> Love for work. That is the only virtue which keeps a man alive and out of mischief of the worst sort, love for his work. Without it a man is absolutely no use in this world.... There are always so many things coming between me and my work [A.1].

This characteristic of Hurter's manifested itself particularly in his strenuous efforts to maintain the viability of the Leblanc process in the 1890s, even when it was obviously declining. Several writers have interpreted this energy as misplaced and emotional loyalty to the Leblanc industry. An alternative, and perhaps more credible, explanation is that Hurter loved a challenge and was happy to work hard on whatever task was presented to him for the satisfaction which he obtained from so doing.

As a full-time employee holding senior positions in the chemical industry, Hurter would have been fully occupied with his day-to-day duties for five and a half days each week. This thesis has not dealt with his vast amount of photographic research, which was carried out in his spare time: evenings and week-ends and (according to his letters) whilst on holiday with his family. Most of the great volume of documentation relating to this, which includes published work, reports, letters and other papers, has been examined by the author at the Royal Photographic Society's archives.

In addition to his full-time work and photographic research Hurter had numerous other interests. His involvement with the founding of the Society of Chemical Industry, and the official posts which he held in it, are discussed in Chapter 12. An examination of the minutes and proceedings of the Liverpool Section of the Society shows that, except for the last few years of his life when his health was failing, he rarely missed a Section or Committee meeting. The society's Journal shows that he invariably took part in the discussion of other workers' papers. His contributions, even on subjects outside his own specialisation, were lengthy and well informed and they obviously took a great deal of time to prepare. He was also a member of the Liverpool Photographic Society and the Liverpool Physical Society.

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He lectured at evening classes and gave a number of those public lectures on popular science which were so well-supported in Victorian times as a form of family entertainment. Audiences at these, even in a medium-sized industrial town such as Widnes, were frequently numbered in the hundreds.

Hurter's interest in politics led him to address a public meeting on at least one occasion. In addition to all this, he found time to devote to his family, teaching his wife German and attending music concerts and similar entertainments. Given the diversity and extent of all these interests, it is difficult to appreciate how he found enough hours in the day to accomplish all that he did.

There is little evidence that Hurter was personally ambitious; he spent most of his working life in the same post, with Gaskell Deacon, and would probably have stayed there had it not been for the formation of UAC. He showed some surprise when offered the post of Chief Chemist of UAC, since he had apparently not lobbied for it and, in a letter to Lunge, expressed doubt as to whether he should accept it. It is probable that these doubts stemmed not from a lack of confidence in his technical and scientific abilities, but more from the fact that the new post was expected to have a considerable content of management and organisation of laboratory staff. His previous post of Chief Chemist at Gaskell Deacon seems to have been concerned mainly with technical work and not management. Donnelly points out that this was not uncommon in the British chemical industry at that time [A.2].

Despite this apparent modesty, Hurter did not lack confidence in his own abilities. He was always positive, even inflexible, when expressing an opinion, probably because he rarely did so without having first carried out meticulous experimentation and calculations to support it. This characteristic is confirmed by the "In Memoriam" article in the Journal of the Society of Chemical industry [A.3]; many other examples have been discussed throughout this thesis.

Hurter died at the comparatively early age of fifty four, having been in poor health (or so he believed) for some years. Callender [A.4] has reviewed a number of letters written by Hurter about his health between 1895 and 1898, from which the following quotations are taken:

I am staying at home to avert a long troublesome illness as last year. I have a bad cold, sore throat and chest....

I have not yet ventured out, my voice being still missing.

My cough got so troublesome that I decided to stop in bed ... yesterday the violent effort at coughing almost exhausted me.

I am still incapable due to loss of rest, [and] incapacity of taking food. ... However, the amputation of the uvula has certainly reduced the terrible paroxysms of coughing.

The Drs ... have decided that whatever is the matter is nothing deadly. Dr Carter finds that I am really better than I was in October or November, and does not think the blood in the sputum means much or serious harm.

These quotations suggest that, in the latter years of Hurter's life, he demonstrated a degree of hypochondria. Accordingly, advice was sought on the medical implications of the letters by the author [A.5].

Hurter complained of throat and chest complaints over a number of years and was wont to retire to bed on, it seems, the smallest pretext. This was very much in contrast to the excessive zeal for work which he had shown in earlier years. To be fair, though, such conditions would have been aggravated by the polluted atmosphere of a Widnes chemical works in the nineteenth century. He would have been in close contact with acid fumes and chlorine on a daily basis throughout his life and he was apparently a heavy smoker: virtually every photograph of him shows him so engaged. Without the benefit of antibiotics, respiratory infections would have been far more serious than they would be today.

So troublesome was Hurter's cough that, in 1897, his uvula was surgically removed, an operation which would not be carried out for the same purpose today. His sudden death from an aortic aneurysm was possibly due to hardening of the aorta (and other arteries) as a result of his heavy smoking.

Some impressions of Hurter's attitude towards people have been obtained from comments by his contemporaries, his personal letters and reports of discussions at meetings of the Society of Chemical Industry and the Liverpool Physical Society. The latter often report flashes of humour and light-weight repartee from many of the participants but not from Hurter. It appears that he was cold and lacked humour, taking himself very seriously. On the rare occasions when someone proved him wrong, he briefly acknowledged the fact, but without apology. He was, however, generous in giving credit to the person who had done so.

In the year following Hurter's death, Smetham, Chairman of the Liverpool Section of the SCI, and one of Hurter's close associates in the society, introduced the first Hurter Memorial Lecture, which was delivered by Lunge [A.6]. Smetham made some valuable comments about Hurter. They were generously worded and, although they cannot be other than subjective, they have a ring of sincerity. They are therefore quoted below at some length, because it is felt that they are probably the most accurate description of Hurter's personality which is available to us.

... Hurter died at the very zenith of his fame ... His genial presence, his humility - the humility of true greatness - his loveableness, his flashes of genius so characteristic of the man, are too well remembered by all of us.... Spare in form, with the student's stoop - begot, I fear of a too persistent devotion to duty - a quickly awakening glance of intelligence when a new subject was broached, and a rapt attention to the subject under discussion.

... when he ... gave expression to one of his masterpieces [i.e. presented a paper], ... it became evident that he was a very giant among his compeers, and everyone present felt the spell of his power.... a fuller revelation of his greatness was to be found in the simplicity of a noble life, which was ever ready to sacrifice itself if, by so doing it could add ... to the happiness or advancement of the youngest aspirant to scientific fame.

... not only will the published work of Dr Hurter have a lasting effect in moulding the destinies of manufacturing chemistry, but the more subtle influence of a noble example will make itself felt in circles ever widening to the marge of time.

References

A.1   *A history of the chemical industry in Widnes*, 1st ed., (London, ICI, 1950), p. 172.

A.2   J Donnelly, "Consultants, managers, testing slaves: changing roles for chemists in the British alkali industry, 1850-1920", *Technology & Culture*, XXXV, (1994), p. 121.

A.3   "The life and work of Ferdinand Hurter, Ph.D.", *JSCI*, XVII, (1898), p. 406.

A.4   R Callender, "A few lines to let you know...", *J Audiovisual Media in Medicine*, 2, (1979), pp. 122-123.

A.5   Letter to C A Townsend from Prof R Marshall, Cardiff Medical School, University of Wales, 23 September 1996.

A.6   A Smetham, Chairman's address to Liverpool Section, SCI, 4 October 1899, *JSCI*, XVIII, (1899), p. 892.

SOURCES

The considerable amount of published and unpublished material written by Hurter, and by others about him, has provided a sound basis upon which to carry out the research on his professional career. In particular, for the period during which he was employed by UAC, a comprehensive record of his work is given in the complete set of Central Laboratory report books.

His most fruitful research period was when he was with Gaskell Deacon; at UAC his work had a higher content of supervision and management. Some of his Gaskell Deacon laboratory notebooks still exist; these and the many published papers and patent specifications which he produced at that time have been fully analyzed and evaluated. He may have carried out other research which was not published for reasons of commercial confidentiality, but it is not felt that its absence will seriously affect this thesis. His close associate Lunge commented:

> ... his situation as chemist to a private firm prevented him from publishing the results of his work as far as manufacturing interests were concerned. ... his business position ... imposed upon him the obligation of keeping secret just the most interesting results of his labours ... [S.1].

Primary sources

There is, fortunately, a great deal of primary source material available which deals with Hurter's professional career, including publications in journals, patent specifications, original laboratory notebooks and reports and company documents. Thus the thesis has a high content of primary source material. He published many papers, letters and contributions to the discussions of other authors' papers, in technical journals, including Dingler's Polytechnisches Journal, Chemical News and the Journal of the Society of Chemical Industry.

When he was head of UAC's Central Laboratory, Hurter encouraged the chemists who worked under his supervision to publish papers, mainly in the Journal of the Society of Chemical Industry. These have provided valuable information on the work being carried out in the department at that time.

Hurter's British and United States patent specifications provide a great deal of chemical and engineering information about his inventions, often including detailed engineering drawings of plant or equipment.

Hurter's laboratory notebooks were of great value; several of them which cover his period with Gaskell Deacon are still in existence and they have been referred to where appropriate in the thesis [S.2]. Contemporary UAC internal documents and ICI internal reports of later date, have also yielded valuable information which has hitherto been unpublished. Hurter's private letters and reports have also been accessed.

In reviewing the technology and economics of alkali manufacturing in Britain in the nineteenth century, use has been made wherever possible of text-books which were actually in use in industry at the time. In spite of the difficulty of producing technical books, compared to the present day, these were always surprisingly well and comprehensively written, containing much practical detail. Descriptions of the processes discussed in this thesis have mainly been taken from these books. The most valuable for providing process details are: C T Kingzett's, "The history, products and processes of the alkali trade", published in 1877; G Lunge's, "A theoretical and practical treatise on the manufacture of sulphuric acid and alkali, with the collateral branches", published from 1879; and a later one, "The alkali industry" by J R Partington, published in 1919.

Details of these and other sources are given in the Bibliography.

Primary source archives

The Dickinson Archive is a massive collection of original documents which is held at the Cheshire Record Office. They appertain to ICI in the North-West, including the United Alkali Company and its constituent companies and Brunner, Mond & Co. They extend back to the middle of the nineteenth century. The collection includes some of Hurter's original laboratory notebooks, the report books of UAC Central Laboratory from its first day of operation, original copies of Hurter's private letters and reports and the Board minute books and other company documents of UAC.

When Hurter died in 1898, all the documents referring to his photographic research passed to his partner, Driffield. When Driffield died twenty years later, the enormous collection of documents, now known as the Driffield Bequest, was given to the Royal Photographic Society. This body commissioned an eminent photographer, W B Ferguson, to collate and catalogue them.

Although this thesis is not concerned with Hurter's photographic research, it was felt prudent that the Driffield Bequest, which is kept in the Royal Photographic Society's archives at Bath, should be examined in depth by the author. This was a considerable task in view of the vastness of the collection, but it yielded some surprising results. Included in the collection was a quantity of material which was concerned with Hurter's activities in the chemical industry: documents which had obviously never been examined for at least a hundred years. They included manuscript note books, some of which contained

both industrial and photographic chemistry, a report on the Castner process and unpublished essays on philosophy and thermodynamics. This discovery has provided valuable data for inclusion in this thesis.

An explanation of the presence of these papers on industrial chemistry in the archives of the Royal Photographic Society, and of the fate of some others, is found in a letter written on 7 August 1918, by Hurter's daughter Annie to Ferguson [S.3]. She sent Ferguson a number of documents which she had in her possession and offered to send him a quantity of other papers which referred to industrial chemistry, including:

> ... copies of letters or reports for the United Alkali Co - and various notebooks on the so-called "Deacon Process", which was one of my father's earliest works ....

Since these would have been of no interest to the Royal Photographic Society, it seems that her offer was not accepted by them. It is unlikely that they would have been sent to UAC, although the Dickinson archive does include some documents which answer that description. It is probable that those referred to by Annie, sadly, did not survive. In fact she ends her letter:

> I am afraid that many papers of my fathers which might have been of value had they got into the right hands were destroyed.

Hurter's photographic researches have been fully described in the literature; material published on the subject is listed in the Photographic Bibliography.

Other primary source archives consulted include:

Catalyst, Museum of the Chemical Industry, Widnes.

Brunner, Mond & Co Archives, Winnington Hall and Mond House, Winnington.

University of Liverpool Archives.

Society of Chemical Industry, Liverpool.

Liverpool Record Office

ICI Library, Runcorn

Secondary sources

Brief references to Hurter appear quite frequently in histories of the chemical industry, but a detailed account of his professional career has never been written. A chapter devoted to him in Hardie's excellent history of the chemical industry in Widnes gives a comprehensive personal biography, but his professional work is not fully described and there are few literature references [S.4]. This is understandable, bearing in mind the wide range of subjects which the book covers.

Reader, in his history of ICI [S.5], discusses Hurter's possible part in the decline of the Leblanc alkali industry, a matter which is examined in detail in this thesis. However, a close examination of Reader's statements shows that, in most cases, he is simply repeating the opinions which Hardie had written twenty years earlier; little new material emerges. But Reader (or perhaps more correctly his research team) must be credited with having written a comprehensive history of ICI, which has provided valuable factual information about the Leblanc, ammonia-soda and electrolytic alkali processes.

Warren [S.6] and Haber [S.7] were specialists in economic history, who looked at the history of alkali manufacture mainly from that view-point; their books are valuable sources on that particular aspect of the subject.

Literature references

These are shown in the text as [*.*]. For example, [2.3] is the third reference in Chapter 2. They are listed at the end of each chapter. This positioning and their format are in compliance with the requirements of the Journal of the Society for the History of Alchemy and Chemistry (AMBIX).

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S.1   G Lunge, "Impending changes in the general development of industry and particularly the alkali industries", First Hurter Memorial Lecture, *JSCI*, XVIII, (1899), p. 893.

S.2   Hurter's laboratory notebooks, CRO file: DIC/UA9/1

S.3   Letter, Annie Hurter to WB Ferguson, 8 Aug 1918, RPS file: Driffield, Misc.

S.4   D W F Hardie, *A history of the chemical industry in Widnes*, 1st ed., (London, ICI Ltd, 1950).

S.5   W J Reader, *Imperial Chemical Industries - a history*, 2nd ed., (London, Oxford University Press, 1970).

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