Appendix A: The Influences of Societal and Political Forces on Scientific Research.

It is not normally necessary to consider historical, sociological or political forces in a paper on physics or chemistry. The reason is clear: for practically all areas of classical physics and chemistry, appropriate laws have long existed. However, concerning the unusual aspects of vapour phase electrochemistry, no valid laws have ever been developed. It will be argued in what follows that non-technical forces are mainly responsible for this situation. Although a few eminent scientists have seen the importance of the missing science, very few others have accepted its significance. Under such circumstances the absence of any theory that is valid in a moist gas is hardly surprising; nor is the fact that even those few scientists who fully understand the nature of the problems can do nothing about it if they wish to maintain any sort of career in research.

A1 Historical Support - When Available.

One way of understanding how those in power can significantly influence the development of a field of study is to consider the types of study they are likely to support. For millennia, expensive research was supported largely, but not entirely, in the expectation of practical benefits. Pure science has normally taken second place. The branches of physics that have advanced most spectacularly did so largely for two reasons. One was that early astronomical knowledge was acquired though religious authorities. The other was that, at little cost to society, there have always been a small number of inquisitive people with outstanding mathematical abilities (see e.g. Penrose 2005).

Precise astronomical studies have always been expensive but, from antiquity, they were supported by powerful priestly elites. Usually these elites would have provided advice that was valuable to farmers. Had there later been no powerful patrons prepared to fund astronomical studies, it is possible that Newton's mathematical skills might never have been demonstrated. Crucially, Kepler had relied on wealthy patrons (Muir, 1994) and his studies were, in turn, built on very precise measurements, begun between 828 and 833 AD, at large observatories that had been supported by the Islamic religious authorities of the time (Al-Khalili. 2010).

Currently, the situation is very different. Wealth creation and defence are now almost the only concerns of governments as well as of the rich and powerful individuals and companies that influence them. The result is a complete triumph of competitive forces over the concept of public service. Until the collapse of the Soviet Union, very basic research was reasonably well supported in both East and West. However, once capitalism took complete control of the world, things began to change and this has had very serious consequences for the specific problems discussed in Section 2 of this paper.

In practice, the policy changes resulting from the rise of neoliberalism (Harvey, 2005; Klein, 2014; Giridharadas, 2018; Goodman, 2022) have ensured that no significant advances in vapour phase electrochemistry have been possible. A related problem is the very profitable advances in the so-called "tech" industry, which have led to increasing trends towards populism and the deliberate ignoring of well informed opinion (e.g. Nichols, 2017).

As a past Director of the Cavendish Laboratory, and thus a successor to such great physicists as Maxwell, Thomson and Rutherford, Sir Brian Pippard provided much valuable advice to me - both before and after the closure of CERL. His initial approach to me, when representing the government's position, was to act as Devil's advocate by arguing that there is no need for studies in the absence of gravitational fields because there must surely be far less expensive alternatives than using space vehicles. However, having earlier written a textbook on thermodynamics (Pippard, 1957), he quickly accepted that such studies were needed if we were ever to understand electrolyte behaviour in near critical water and in steam (Pippard, 1989). In a letter to me (dated 21st March 1989) shortly after our meeting, he made the following comments: "... I am convinced that there is a very interesting scientific problem here, one which will be troublesome to resolve fully, but all the same one which I should very much like to see studied".

He also said he would try to obtain increased support from senior CEGB management for our collaboration with ESA. At the time, he happened to be one of the principal scientific consultants to the CEGB and he succeeded. Largely as a result of his intervention, three preliminary sounding rocket experiments were successfully completed *on schedule*. An equally vital contribution he made was to ensure that a few physicists and mathematicians were assigned to the project to work on the theoretical aspects of the problem.

Unfortunately, CERL was closed before these mathematical advances had made much progress and the only records of this preliminary mathematical work are in a few internal documents (Peters, 1984, 1985, 1987a,b). I am fairly certain Sir Brian was also responsible for circulating my early ideas on ball lightning (Chown,1993; Matthews, 1994; Anonymous, 1994). With decades of hindsight, a rather less satisfactory, though at the time essential, aspect of my interactions with Sir Brian should probably be mentioned.

It concerned the difficulties in publishing my first paper on ball lightning which were referred to briefly in the Acknowledgments. At the end of the meeting discussing our collaboration with ESA, I raised a potentially serious difficulty: the impossibility of making a logical case for my ball lightning model in a paper that was short enough to be acceptable in any normal scientific journal. In the event, the paper was published without referring to much of the background physical chemistry that is missing. At the time, winning the argument for publication of the paper (Turner, 1994), seemed such a triumph that we were not really concerned over the fact that the missing science, *which had originally led to the development of the model*, was not even discussed in the paper. As we shall see, this early over-confidence seems to have been a mistake.

Advice Sir Brian gave me much later, after we had both been forced to accept the very unfashionable nature of the field, led directly to some more recent experimental work (Bartlett and Turner, 2023a). His eventual advice had been clear: unless *convincing experimental evidence* in support of the ball lightning model could be obtained, my ideas would probably continue to be ignored and the funding situation was unlikely to improve. His final letter to me (dated 25th April 2003) began with the words "Once again, I fear, I am useless...." Despite his disappointment over the limited success of his later efforts to help, his unshaken faith that the work is important has sustained me for decades.

A2 Technological and Societal Problems.

A brief summary of relevant aspects of the history of CERL needs to be mentioned here. In the mid 20th century, major changes in the management of research were made as the electric power industry in the UK slowly recovered from the aftermath of World War II (Mogford, 1993). The recovery was so slow that, even by 1958, the electric grid was regularly unable to cope with the increasingly high voltages needed for the efficient, and therefore the most economical, transmission of electricity. As a result, the private generating companies, under periodic pressure from governments, were repeatedly forced to *acknowledge* that their investments in research were inadequate. However, they did nothing about it.

Eventually, after many years during which the problem continued to be ignored, the CEGB was formed and it began to manage the industry. One consequence was the creation of CERL. This laboratory concentrated on basic, often long term, problems of the industry while urgent problems were dealt with at much smaller regional laboratories. By 1962, when the CERL facility was opened, interdisciplinary research was widely accepted as highly desirable and it was strongly encouraged from the start. In 1964, I was offered employment at CERL and the offer was taken up the following year. It does not seem possible to explain why so many 19th century studies of ions in moist air have been neglected without referring to what was learned as a direct consequence of the extremely unusual way in which research was carried out at CERL.

A primary task of many staff members at CERL was to act as consultants to an appropriate Division of the CEGB. By the time the laboratory was built, electric power stations were being operated at much higher temperatures than the normal boiling point of water. Typical boiler temperatures for new, high efficiency, plant were close to 350^0 C. As long as power stations were still operating at temperatures not very much higher than those of steam engines, no need was seen for chemical treatments to control scale or corrosion. Periodic removal of sludge was found quite sufficient. Soon, however, a variety of methods for treating boiler water began to be used. It seems that the addition of potatoes or the heads of sheep were among the earliest methods tried. Then, increasingly scientific approaches began to be sought.

As electric power stations began to be built for higher *generation efficiencies*, by boiling at ever higher temperatures (and consequently higher pressures), the need to control the chemistry of the water that is fed to the boilers became obvious. A major need was to understand the role of additives in pH control. Some early experimental studies on the dissociation of electrolytes up to 300^0 C (Noyes, 1907) might, if the need to expand on them had been seen, have put the industry on a good footing from the start. Unfortunately, the significance of these findings seems not to have been appreciated for another 50 years, when power stations started to be operated at this sort of temperature.

One of the peculiarities in the way research was carried out at CERL now seems, to me at least, to have been crucial in helping define the missing science. For most of the research conducted there, instead of the usual three years for a clearly defined research project, a general direction covering obvious long term needs would be agreed and progress *reviewed annually*.

Presumably this approach was adopted because it enabled the senior managers of the CEGB to obtain maximum benefit from the research they were supporting. If normal three year contracts on well defined problems had been in use, more research papers would certainly have been published, but it is extremely unlikely that an increasingly clear picture of the *missing science* would ever have been provided - at least not by scientists at CERL.

This unusual management system clearly allowed more rapid *changes of direction* for research projects than would otherwise have been possible. It meant that major current concerns of the industry could, when necessary, determine such things as the allocations of manpower and funding for current and subsequent years. Specific topics for the year would be prioritised depending on the needs of greatest current concern.

Many of the staff were expected to conduct parallel work as both modellers and experimentalists. In the Steam-Water Chemistry Section, this approach could be particularly beneficial because of unavoidably long delays in the design, manufacture and safety-testing of any new high pressure autoclaves that were needed. However, an early consequence of these delays was that, because of even minor changes in priorities, far more was frequently learned about the extent of our ignorance than about how to counter it.

A3 Some Long Term Consequences of Privatization

Before CERL was closed, shortly after privatization of the CEGB, it seems that rather few of the staff at the laboratory had seen the full significance of this way of operating. Many probably saw it as of advantage only to the senior decision makers in the company. I now see it as evidence of the CEGB's commitment to act, as far as possible, in the public interest. It was a lack of this kind of commitment, on the part of the successor companies to the once nationalized industries, that so greatly concerned Sir Michael Atiyah in a Presidential address to the Royal Society (Atiyah, 1994). Essentially, the privatized companies (at least in the case of the successors to the CEGB) were reverting to the practices of their pre-nationalization predecessors by ignoring the value of long term research in favour of maximizing profit.

Atiyah's (1994) comments were made three years after completion of the privatization of electricity supply in the UK. By then, the policies of the privatized companies had become clear and Atiyah was becoming increasingly concerned over the fate of all the basic research that had once been supported by the previously publicly owned companies such as the CEGB. One clear indication of these policies was the closure of CERL, the laboratory where much of the work discussed earlier had been undertaken. In his presidential address, Atiyah expressed his concern that the basic studies once supported by the CEGB had all been abandoned by the privatized companies. It seems that, *twenty years later*, Atiyah's worst fears have been fulfilled - if the evidence of a detailed public opinion survey is anything to go by. The *financial* benefits to investors had been great (see e.g. Newbery and Pollitt, 1997) but the benefits to consumers were certainly not (Rowe, 2014).

Had the CEGB not been privatized and CERL closed, three decades would not have been almost totally wasted before the validity of the main novelty in the electrochemical model for ball lighting could be demonstrated experimentally. Even if artificial air plasmas had not been created by now, we would be much nearer to developing a new, carbon free, way of producing electricity that is available day and night, wind or no wind.

Well before the government had shown the full extent of its disdain for basic research, its motivations were clearly demonstrated in a totally different way. Apparently the government suspected that support for ESA's contribution to the International Space Station was not in the country's best interests. ESA's contribution was the design and construction of the Columbus module, in which microgravity experiments were to be carried out. Assessing the project overall was put in the hands of the "Core Team of the UK Columbus Utilization Programme", acting for the Department of Trade and Industry. This Team then appointed a "Microgravity Panel" which was asked to prepare a report on the benefits of microgravity research to the UK economy. A report on the Panel's work was issued in due course (Wolff, 1986).

All those currently involved with the ESA program were invited to serve on this Panel. The experience was a revelation to me since, to my knowledge, the *technical* need for our studies had never been doubted by anyone in the CEGB management - or by ESA. At an early meeting, I caused much amusement among the more experienced members of the panel, and to the civil service representatives present, when I questioned why so little time was being devoted to the *practical economies* ultimately expected from the research. The answer was that the only really important consideration was how much financial benefit and prestige might accrue to the British aerospace industry. I was also told that the problem with *every one* of the UK proposals was that none of the research could possibly yield a rapid return on investment.

How could it, when so many influences of gravity had been so completely ignored for so long? For an indication of the wide range of these problems, see Walter (1987).

During the early planning stages for privatization, fears over the future of research began to be felt by the staff at CERL following a rumour concerning a meeting between our Chairman, Lord Marshall, and the then Energy Minister, Cecil Parkinson. According to this rumour, toward the end of the meeting, the Minister said to our Chairman that he had "probably won all the technical arguments" but that he was "politically naive". His political naivety was generally thought to be related to the need for the government to destroy the power of the National Union of Mineworkers under the leadership of Arthur Scargill. It should be pointed out that, at the time of the meeting and the rumour at CERL, the Planning Department of the CEGB had *already* ensured that ample supplies of coal, thoroughly adequate to outlast any strike, were available at all large coal-fired power stations. Faith in privatization was clearly all that really mattered to the Thatcher government.

Other aspects of the privatization plans were more concerned with the CEGB's longstanding views on how the industry should be run. Some politicians and influential commentators had long accused the CEGB of being too thorough in its policy of always using the best technical information available in its decision making. This must have been the context of a truly extraordinary instruction the staff at CERL were given one morning at this period. All the staff were instructed to assemble for an important presentation.

It was given by a very young man who was presumably from some management consultancy company. He spent the best part of an hour explaining to us, in detail, why a Volkswagen is a *better car* than a Rolls Royce. His argument was **not** that a Volkswagen is better *value* for money than a Rolls; *he insisted that it was altogether a better car* and implied that anyone who could not understand this fact was a simpleton. Clearly, this kind of message did not inspire confidence for the future of research after privatization; nor for the kind of service that the privatized companies would be likely to offer to the public in future.

A4 Attempts to Alleviate Some Very Long Term Problems.

The adverse consequences of the closure of CERL to studies of all the electrochemical problems discussed earlier were, in fact, delayed for a few years because our collaboration with ESA was still active while the CEGB was being privatized. At that time, there still seemed to be hope for the project. With financial support from one of ESA's contractors and help from department heads at Bristol University, I was encouraged to spend some time in Bristol completing CERL's part in what turned out to be our final low gravity experiment. The hope was, of course, that the move would allow time for a new source of support to be found to replace that from the CEGB.

At the time, I happened to be Chairman of the small group of British scientists and engineers who represented the UK at the International Association for the Properties of Water and Steam. So I used my position to inform the relevant government ministers and civil servants about the steam-related areas of research that had once been of considerable concern to the CEGB. I also contacted several private foundations that fund research.

No organization offering funding, apart from the Royal Society, had responded positively to my initial requests for support. Unfortunately, before a detailed proposal to the Royal Society could be prepared, all hope of securing funding in the UK was dashed: the Bursar at the University informed me one day that universities were not permitted to host visiting researchers if they lacked financial support. By then, the agreed project with ESA had just been completed so this applied to me. I was therefore asked to leave as soon as possible. I took advantage of the fact that my wife is a citizen of the USA and we moved to the USA.

Well before this move, ESA was starting to promote more microgravity studies in *industrial* fields (Guyenne, 1991). One proposal, as a part of this project, was submitted by scientists and engineers from the UK, France and Germany (Turner et al., 1991) and then with a representative of the electric power industry in Canada (Tremaine and Turner, 1993). The first proposal was quickly approved by ESA as a potential candidate for studies on the European contribution to the International Space Station. The closing of CERL put an end to this proposal, so a substitute proposal (Tremaine and Turner, 1993) was then encouraged by ESA but it failed to gain support from the electric power industry in the USA. Several representatives of the Canadian industry, although interested themselves, felt that their industry was unable to support the project alone.

These experiences imply that real progress in understanding compressible electrolyte solutions is unlikely unless completely new ways of funding scientific research can be found. If such changes are not made, aspects of meteorology and astrophysics for example, are likely to continue to be held back by the *absence of theories that are valid for ions in moist gases*.

As pointed out earlier, air is not the only gas affected by the lack of a valid theory. In the cold, predominantly hydrogen gas clouds from which stars and planets are born, there seem to be closely related problems (Turner, 2023d). It is quite possible that electrochemical cooling might control the *formation* of molecular clouds from the intergalactic plasma. It is clear that, if astrophysicists wish really to understand the processes that occur at the gas-plasma boundaries of molecular clouds, they will certainly need to be *much more careful* than in the past to avoid using mistaken concepts in modelling exercises. No remotely *valid* conclusion can be drawn if it is necessary to assume the identity of ion activities and ion concentrations.

A5 Some Consequences of Scientific Tribalism.

Unfortunately, lack of funding is not the only problem if there is to be any hope of *quantifying* basic vapour phase electrochemistry. Specialization, public opinion and fashion can also be important. Tendencies to scientific tribalism have long been present in society but the accelerating rate at which huge quantities of new data are being obtained, as well as the new questions they raise, further act to *inhibit support for unfashionable studies* of basic science. At the moment, almost no-one can afford to address really old questions or to explore the consequences of the non-availability of answers to them.

Such problems tend to be dismissed as purely philosophical and thus of no practical concern. The result is that the only *valid* arguments that are possible are qualitative and thus tend to be dismissed as "hand waving" or pure speculation. Such arguments were, in effect, used to justify rejection of a paper I submitted to the journal Physical Chemistry Chemical Physics in 2008. The paper was titled "Energy from the Air". Global warming was by then seen as a very serious problem by most scientists. My paper described the reasonable hope of *eventually* solving the global warming problem by simulating natural air plasmas. It covered the basic facts available but it did not describe the missing science in detail as this would have made the paper far too long. The present paper is an attempt to address this problem.

The paper was rejected on the advice of one referee. Most of the rejection letter reads as follows: "This manuscript was submitted as a PCCP perspectives article. It deals with the possible existence of 'balls of fire', which were occasionally reported in the 18th century physics literature. A ball lightning effect can be attributed to nitrogen oxidation to nitric acid, a process which requires very special thermodynamic conditions. While the manuscript is well written and interesting to read, from a rather cultural point of view, the scientific part is almost purely speculative. I am afraid PCCP is not at all appropriate for publishing such a manuscript. I recommend rejection of the present manuscript, which would perhaps be better suited for a journal in the field of history or epistemology of science".

It should be pointed out in the context of " 'balls of fire'... occasionally reported in the 18th century" that, by 2002, 10,000 reports on the phenomenon had been received by scientists interested in ball lightning (Singer, 2002). The referee's ignorance concerning ball lightning as well as the lack of valid theories is typical of most scientists. In the rejection letter, the assistant editor invited comments or questions. When I offered them I was advised that my best approach was to write something for Chemistry in Britain, the monthly "trade" journal of the Royal Society of Chemistry (RSC). Having long been a Fellow of the Society, I took the editor's advice but my letter went unanswered. This was, by then, no surprise at all as the Society no longer seems to consider itself an academic one.

The internal shortcomings of the RSC were illustrated in a different way at about the same time. In April 2008, the Society introduced its long term plans for the future of chemistry in the UK. This was to be provided in the "Roadmap for the Chemical Sciences" and the RSC began seeking the collaboration of its members. My contributions and comments were submitted under three of the "Sections" under whose titles the exercise was conducted. When the final report (Prest, 2009) came out, however, there was *not a single reference to the possibility of using air plasmas to extract solar energy*.

The basic difficulty I have had, in most of the examples illustrated, is presumably related to the problem mentioned earlier; that it is quite impossible, in a single paper, to show convincingly *that an important part of basic chemistry is missing*. No individual reviewer or editor, asked to assess a communication addressing this kind of problem can afford the time to acquaint themselves with all the previously published papers on which the arguments of the submission depend. Most likely, in the case of the RSC's Roadmap, the Society had subcontracted the assessment of its members' inputs to individuals who were totally ignorant of important facts.

If, as the earlier-mentioned problems suggest, even chemists cannot understand my arguments, who can? As a very disheartened Sir Brian Pippard seems to have been forced to accept eventually, there is very little chance of making real progress unless much more experimental evidence in the area can be accumulated.. It can only be hoped that our new *experimental* findings (Bartlett and Turner, 2023) will encourage the obtaining of new experimental evidence that can eventually convince those in power that the missing science is important. A more realistic hope is that the new results might encourage a change in the views of a few scientists.

A6 The International Association for the Properties of Water and Steam.

A quite different illustration of adverse societal forces can be seen in the long history of the groups of scientists and engineers who have, since the beginning of the 20th century (Callendar,1900), been concerned to assess and recommend the best available physical properties of steam. Internationally agreed Steam Tables were early required to ensure that the boilers and turbines of electric power stations could be reliably purchased from manufacturers in different countries. Now these tables, as well as formulations for their use in the design of new plant, are provided by the International Association for the Properties of Water and Steam (IAPWS) which, in addition to its main task, also organizes five-yearly conferences.

Until 1989, these conferences were called International Conferences on the Properties of Steam but, by then, the crucial role of solutions, in particular electrolyte solutions, was being seen. By this time, many electric power stations had been operating at around 350^0 C and 165 bar pressure for many years and the industry was finding that the electrolyte content of boiler water was occasionally causing such serious corrosion problems that these *chemically related* problems were of far more concern than most needs for more precisely defined properties of pure steam. The names of the Association and of the conferences it was organizing were therefore changed to more accurately describe its purposes - by including the word "Water" in both titles, the abbreviated titles thus becoming IAPWS and ICPWS. Scientists and engineers from many industrialized countries now contribute annually at IAPWS meetings and, at 5 yearly intervals, at its Conferences.

Somewhat earlier than the privatization of the CEGB, unhelpful changes in government attitudes to science, both in the UK and in the USA, were already being seen. Governments in both countries were increasingly concerned not to waste taxpayers' money. One consequence was the closing of two small research groups that specialized in thermodynamics. One was at NPL (the National Physical Laboratory) in the UK. The other was in the USA at what used to be called the National Bureau of Standards (NBS); now the National Institute of Standards and Technology or NIST.

At the time I joined IAPWS, as the CEGB's representative, chemistry problems in both fluid phases were being seen as major industrial problems and the secretarial needs of the Association had long been in the hands of the NBS. However, the government-imposed staff reductions (which happened to fall heavily on those providing secretarial support to IAPWS) meant that providing this service was no longer possible. For a few years after NIST's support for IAPWS had become impractical, its *objective scientific* standards were still unquestioningly upheld.

During this earlier period, a plan was drawn up which aimed to use what little influence the Association possessed among the *academic community* to list some internationally agreed research topics that would be of value to the electric power industry worldwide. The idea was that IAPWS endorsement might assist any interested *academic* scientists in obtaining support from appropriate national funding agencies. One early suggestion was for work on compressible electrolyte solutions and this suggestion seemed to be generally accepted. I later learned that, at the first annual meeting I had been unable to attend since I joined IAPWS, this suggestion had been *deleted from the approved list*.

When new secretarial services for IAPWS were needed (to replace NIST), the responsibilities had been transferred to the Electric Power Research Institute (EPRI) which is also based in the USA. Significant changes at IAPWS were soon apparent. As the new Secretary of IAPWS later pointed out to me, EPRI is funded entirely by private industry so that there was no longer any chance of it being willing to support, even indirectly, the kind of long term research once conducted at CERL and elsewhere. As he put it, managers in the companies that fund EPRI would rapidly withdraw their support from the Institute if it were seen to be supporting studies that were considered too academic.

The Association no longer appears to have any interest in advancing basic knowledge on solutions under conditions where electrostriction unavoidable invalidates existing theories. This means conditions between *about* 300^{0} *C and the critical point of water* and in *steam at any temperature and pressure*. Such information would have benefited both the electric power industry and geochemists.

The problem was further demonstrated by what happened to a paper of mine that was supposed to have been published in the Proceedings of the 13th International Conference on the Properties of Water and Steam (Tremaine et al. eds, 2000). My paper had been approved by the organizing committee and duly presented at the conference. I had written it in a renewed attempt to have the industrial needs, as I saw them, drawn to the attention of the academic community. However, in writing it, the significance of the secretarial changes at IAPWS had not occurred to me. I had simply assumed that the objective scientific standards once applied by the NBS and then NIST were still in operation.

The paper's introduction had included the following words " Scientists tend to assume that all the basic laws needed to model any system are available and this is true for most practically important chemical systems. However, for systems where ions dissolved in a saturated vapour are important, it is not true." I had made similar points several times previously at NBS and EPRI sponsored symposia (e.g. Turner, 1990b) and at IAPWS meetings, so the statement did not seem particularly radical to me. However this phrase, and the other content of the paper, seem to have acted like a red rag to a bull as far as EPRI was concerned because the paper "accidentally" failed to appear in the published proceedings of the conference.

The organizers (the *formal editors* of the conference proceedings) had not realized that this had happened and apologised profusely after being informed of it but nothing could be done - my arguments had been silenced, apparently by the electric power industry in the USA. I feel fully justified in suspecting more than a mislaid manuscript because a briefly mislaid manuscript could not easily account for the title having also been removed from the content list of the Conference. Furthermore, it would have been clear to the EPRI staff responsible for issuing the Proceedings that very few of the plant owners, who fund their activities, would have welcomed my message. Some of the EPRI staff were also well aware that I was, by then, retired and working purely as an amateur.

Most engineers, like physicists, feel obliged to rely on *facts that can be quantified* and simply to ignore those that cannot. Such ways of thinking can be very beneficial if they lead to new predictions that can be tested. They are not beneficial if they lead to potentially dangerous decisions, the abandonment of possibly valuable research areas and to the public being required to pay for the resulting ignorance. It seems probable to me that the attitudes of company managers, displayed in all the above examples, contributed significantly to the very unfavourable attitudes of UK electricity consumers revealed in the public opinion survey of Rowe (2014). This survey was mentioned in Section A3. The inference is that privatizing the CEGB has not benefited electricity consumers at all. The abandonment and ignoring of research (not only in the areas discussed here) probably contributed to the dissatisfaction of electricity consumers in the UK mentioned earlier.

At present, there is no incentive whatsoever, among the academic community, for investigating very basic electrochemistry problems at high temperatures *except* at solution densities that are far higher than the critical density of steam. Fortunately, in only for the survival of IAPWS, there is still support from geochemists for studying solutions at very high pressures. Under these conditions, existing theories are expected to be reasonably reliable. Geochemistry is an important discipline and IAPWS still provides some services to its original sponsors. However, in my opinion, it no longer serves the electric power industry as well as it might.

Appendix B. Limitations of the Debye-Hückel Model.

The basic Debye-Hückel model (Debye and Hückel, 1923) has been used as a fundamental element in treating the properties of electrolyte solutions for nearly a century. It is applied to both equilibrium properties and to rate processes such as conductance. The theory attempts to derive the radial distribution function for ions of one charge around a single ion of the opposite charge by combining Boltzman's energy distribution law with Coulomb's law of electrostatic attraction. Essentially, the energy difference between an originally charge-free fluid and an appropriately charged one is calculated.

Consequently, it involves an exponential function that is the ratio of the electrostatic energy of the system to its thermal energy. It has long been realized that there are serious problems with many of the approximations needed in its development - but it is still the only simple model that has proved to be of much practical use. One obvious limitation is that the model ignores the molecular structure of a solution. As pointed out by MacInnes (1961), "There is no detail of the derivation of the equations of the Debye-Hückel theory that has not been criticized".

The problem discussed most frequently arises from the mathematical expansion of an exponential function (involving ratios of the electrostatic and thermal forms of energy) to a power series and retaining only the first term in the expansion. Numerous experiments have shown that the predictions of the model are very reliable at very low concentrations and that this tends to be true even for solutions far more concentrated than those for which the theory should break down (see e.g. Harned and Owen, 1950; Robinson and Stokes, 1955). It seems that more elaborate theories must have all tended to provide *worse fits to experimental data than the simple version*.

Before the Debye-Hückel model had been developed, a wide variety of different approaches had been proposed which attempted to address the problems of quantifying the consequences of a far earlier discovery. This was by Arrhenius (1887) and it showed that, when sufficiently dilute, solutions of electrolytes are always fully dissociated into ions. Some electrolytes are fully dissociated into ions at *any* concentration while others only become fully dissociated in very dilute solutions. These chemical species are described respectively as strong and weak electrolytes. The degree of dissociation is normally only of *practical* importance when *acids* could be involved since their degrees of dissociation control the pH of any mixture.

One of the less frequently considered problems with the Debye-Hückel theory is that neither of the two energy terms it employs allows for a change in volume. In other words, the mechanical work done during the charging process is ignored. Hence, in a compressible electrolyte solution, neither the thermodynamic nor the kinetic properties can possibly be *validly* calculated and there *should be* no expectation that the theory is valid under these conditions. The evidence of Fig. 1 confirms that the theory is completely useless when a solution is sufficiently compressible.

In recent years, many studies of the solution chemistry of electrolytes have abandoned the hope of trying to use analytic expressions in favour of using either molecular dynamics simulations or Monte-Carlo approximations (see e.g. Franks, 1973). However, despite its well known limitations, the Debye-Hückel model is still used in interpreting most experimental results. As seen in Section 2.1, even solutions at 25^{O} C are actually somewhat compressible. One obvious improvement to the Debye-Hückel model would have been to add an energy term involving the compressibility of the solvent. This obvious suggestion would have been tried early on but, it seems, its neglect allowed the simpler theory to be applied to significantly more concentrated solutions.

Appendix C: The Ions Most Readily Formed in an Air Plasma.

The ions present inside, and at the edge of, an air plasma are believed (Turner, 1994) to be of crucial importance to the stability of ball lightning. It so happens that the identity of the ions involved is quite clear because they are easily the *most stable ions known* that can exist at the temperatures near the edges of an air plasma. This conclusion was reached mainly as consequence of a spectroscopic study by Powell and Finkelstein (1969). These authors had exposed mixtures of nitrogen and oxygen to brief, but very powerful, radio-frequency pulses. They were thereby able to obtain self-contained mobile blobs of plasma over a range of ratios of nitrogen to oxygen. The plasmas were not very spherical but they all had temperatures close to $2{,}500^0$ C. Radiation was emitted over a wide range of frequencies for periods of up to one second and numerous emission spectra of the plasma blobs were obtained.

The authors took their plasmas to be incipient lightning balls - and so did I when their findings were used in the thermodynamic assessment referred to above (Turner, 1994). The following chemical species were among those considered by Powell and Finkelstein, (1969) in attempting to interpret their spectra : N_2*, O_2* (where * indicates a long lived excited state), NO, NO_2, O, H, OH, N_2^+, O^+, O_2^+, NO^+, O^-, O_2^-, NO^-, H_2O^-, H^- and OH^-. Once all the high energy ions have reacted with other components of the mixture, only the most stable ones would be expected to remain. In order fully to assess the thermodynamic consequences of the most likely reactions (Turner 1994) the following chemical species were also considered: O_2, N_2, H_2O, H^+, H_3O^+, NO_2^+, O_3, HNO_2, HNO_3, N_2O_3, N_2O_4, N_2O_5, NO_3, NO_2^- and NO_3^-.

At a sufficiently high temperature, many of the above species might be expected to form - if only for very brief periods of time. In the cooler regions near to the surface of an air plasma, the highest energy species would all be expected to be transformed into lower energy species. It is thus extremely unlikely that any of the species with the highest enthalpies of formation could survive at temperatures much below 500 K or so. For the purposes of the ball lightning model, it was a simple matter to eliminate from the above lists all the least stable species. For the remaining ions however, it was necessary to consider the actual energies released during all such plausible processes as the following:

$$2\,O_2^+ + N_2 = 2\,NO^+ + O_2 \qquad (7)$$

$$2\,H^+ + N_2 + 3/2\,O_2 = 2\,NO^+ + H_2O \qquad (8)$$

$$2\,NO_2^+ = 2\,NO^+ + O_2 \qquad (9)$$

$$O_2^- + NO_2 = NO_2^- + O_2 \qquad (10)$$

$$OH^- + HNO_2 = NO_2^- + H_2O \qquad (11)$$

The Gibbs free energies of all these reactions are strongly negative - that is favourable. The clear conclusion from all the possible reactions considered feasible is that the only ions left by the time an air plasma has cooled to room temperature are NO^+ and NO_2^- (Turner, 1994). No other ions are expected to be formed (until hydration of the ions begins). If ions are formed in the air at room temperature, by UV radiation instead of by being released from an air plasma, again only these ions are expected to be produced initially. In moist air, hydration would rapidly follow and NO^+ would soon afterwards be converted to H_3O^+.