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POWER FOR THE FUTURE

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What is it that most us would wish for from the research laboratories around the world? A cure for cancer? The synthesis of a convincing meat substitute from soya? Or perhaps a Fifth Generation computer! A moment's reflection will remind us that however desirable such breakthroughs might be, without a continuing supply of energy, in the right form, in the right place and at the right price, civilisation as we know it today would collapse and the very tiny fraction of the human race to survive would revert to a medieval existence. The supreme importance of energy conversion and power sources as intrinsic requirements of an advanced civilisation call for no further justification, and mastery of Energy Technology demands the highest of all research priorities.

Today, the world is not short of energy, though millions of people in the Less Developed Countries cannot afford even enough to cook a modest meal, and indigenous sources such as dung and brushwood are failing to meet the demands of an exploding population. In the West, the energy situation is confused and fluid. Most of our requirements are met from oil or natural gas sources. How much longer will they last? Estimates vary from 20 to 50 years or more. One single development, namely, the means of increasing the extracted fraction from its present 30% or so to significantly higher levels, could transform such estimates, and research to this end is being actively pursued. Coal reserves will last far longer at present rates of consumption. Yet, increasingly, there is alarm over the despoliation of European forests, due to "acid-rain" resulting from coal burning. In many parts of Germany, over 30% of forests are stated to be irreparably damaged. Nor is the damage limited to the trees themselves, and changes, possibly irreversible, in soil composition resulting from acid-leaching are widely reported in Scandinavia. The only solution to this may lie either in abandonment of coal as a fuel, or costly conversion of existing power stations by addition of stack-scrubbing devices or adoption of fluidised combustion with limestone injection. The U.K. cost alone is estimated at £4 $\times 10^9$ capital costs, with proportionately high running costs.

No such hazards are posed by nuclear power. In spite of this, its future is extremely clouded. At Zwentendorf, near Vienna, a virtually-completed nuclear station lies idle and unfinished, condemned to this state by a majority vote in an Austrian national referendum. Now there is talk of converting it to fossil fuel operation and most certainly it is not envisaged that it will ever function as was originally intended. Yet the Austrians are (rightly) more conscious than most of the dangers of acid rain. In the U.S.A. too, there are idle nuclear power stations, though in this case due to disastrous accidents

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during operation. Around the world (except perhaps in Russia and the East) peasants and students combine with scientists, including Nobel laureates, to oppose nuclear power for a range of reasons — from high cost, through radiation pollution (which many fail to realise results equally from coal combustion) to fears of plutonium "leakage" to terrorists or irresponsible countries. The merits of such arguments are partly unproven and partly unprovable, which is not by any means to say that they should be dismissed out of hand.

Scope for further hydro power developments in Europe is extremely limited, and in the U.S.A., only somewhat less so. Once again, ecological pressure groups frequently oppose proposed developments. In Africa or South America, there is much more development potential. And yet, in many çases, such schemes are unattractive because there is no industrial requirement close by to accept the power generated.

We see, in all this, that the provision of power now and in the future is, and will be, coupled to lobbying and social pressures which can be an extremely powerful counter-force. Quite possibly, it is only when the immediate consequences of acid rain or power shortages stare these protest groups in the face, that their vociferous opposition will crumble.

From similar groupings too, we have had something over a decade's exposure to ideas for "Alternative Energy Sources". At Surrey University, the Savonius rotor, beloved of the "Alternative Society" and made of two halves of an oil drum, turns desolately. One wonders whether this device was ever utilised by these people or whether its significance was more akin to those advertising boards which spin round by the roadside to attract attention. More seriously, windmills of 1 MW rating and above are now being constructed or under test in New England, at Tvind in Denmark, and in the Orkneys, to name but a few sites. The worth of their energy contribution has yet to be assessed.

Tidal Energy schemes and Wavepower (the two frequently confused by lay science reporters) appear to have got stuck. The first calls for a single, massive capital outlay, and the French scheme at Rances (successful enough) is the only fully operational example. The British Bristol Channel scheme moves from one feasibility survey to the next, not suggesting any high degree of committment by the U.K. government. Nor does another scheme on the US/Canadian seacoast appear to be moving much faster. Wavepower schemes are, likewise, seemingly stuck, with a multitude of equally ingenious designs from several nations lurking around the scale-up stage. The problems of anchoring a large structure in the aggressive environment of a coastal region have, it seems, been underestimated at least by some groups. The Japanese have revived, and are testing, an old idea for utilisation of thermal gradients in oceans. Once again, there is the problem of anchoring large equipment at sea, while the economics of energy extraction from small differences in temperature must be always difficult.

Solar energy after an initial period of wholly premature optimism, is now advancing on a wide front, ranging from heat extraction to direct conversion to electrical energy. Progress in this last field seems promising in that costs are falling with the rapidity one has come to expect of all electronic devices. Geothermal energy is also making steady progress and while, for example, in New Zealand or Iceland, its application is relatively easy, the cracking of rocks deep below the surface to provide steam or hot water, is being successfully pursued in Cornwall, and elsewhere.

Thus it is that there are grounds for confidence that, even after oil reserves have been depleted, we shall not be without energy to heat our homes or power our industries.

Energy conversion, equally important, displays a somewhat patchier picture. The thermal efficiency of internal combustion engines is being slowly improved, and the adoption of high-temperature operating materials, notably ceramics, both in reciprocating engines and gas turbines, promises really significant improvements in performance. Of the Stirling engine or MHD (magnetohydrodynamics) we hear almost nothing, suggesting that these ideas have run into serious, if not unconquerable, problems. And in respect of fuel cells, perhaps the only thing to say here is that large scale units now being constructed in New York and Japan will teach us whether this technology has, indeed, something to offer. Other ideas, such as the use of semiconductors as thermocouple devices, have disappeared from view.

Less glamorous and less appealing, but equally important, are schemes for energy conservation, whether by insulation or multiple use of thermal energy, as, for example, with district heating and, in all of these, engineers, scientists, and architects can usefully combine to produce savings in energy.

Thus it is seen that in the winning of energy or its conversion from one form to another, there is a great deal of activity and a fair amount of promise on a wide front. In the end, and apart from applications in very specialised circumstances, be these in Space or remote islands, the success of one or more of each technology will eclipse the rest. Quite possibly, Nuclear Fusion will eclipse all other energy sources, but as all these projects leave the laboratory and enter the slower and costlier engineering stages, it may be many years before the picture becomes clear.

However active and successful we may be in the winning of energy or its conversion from one form to another, there is a third term in the energy equation — its storage. Storage is important as a means of buffering fluctuations in demand over hours, days, or even the time of year. Storage is important because it provides another means of transporting energy. Only by means of energy storage (as a liquid fuel) can we fly, or effectively travel in cars. If, and when, petroleum reserves are exhausted, this is where we shall find their absence the hardest to replace. In energy storage, it is unfortunately true to say that less progress has been made than in most other aspects of Energy Technology. We may mention flywheels, compressed gas (for storage of mechanical energy), or hot rocks in cellars, or fluidised sand beds (for heat storage), but though pumped hydro storage energy (as in the Ffestiniog scheme in Wales) has proved completely successful, sites for construction of such schemes are rare.

The other ideas remain as development projects about which one hears little. Storage of domestic heat using phase changes in chemicals such as calcium sulphate (hydrate) has also shown some advance but these technologies have not been transformed, perhaps slightly improved. Above all, our ability to store electrical energy has shown pitifully slow progress in the last few decades, and this lack of progress has led to the advocation of ideas such as the "Hydrogen Economy" in which our weakness is compensated by storing energy as a chemical (in this case hydrogen). Equally, the production of organic fuels such as alcohols, from CO_2 or by fermentation, offers a means of maintaining present technology after a time when oil is exhausted.

Let us focus then, on our crucial inability to store electrical energy, except in indirect (*i.e.*, as potential energy) or chemical energy form. Let us focus, in particular, on secondary storage batteries. The sad but brutal truth is that we have made improvements, but hardly a breakthrough in the last 100 years. More depressing still is the fact that such stagnation does not stem from lack of effort, for billions of dollars must have been spent in pursuit of more efficient secondary batteries. The ultimate irony in this is the fact that in the lead–acid battery, Pb, as a "vehicle for valence change", which is all it is, by virtue of its very high atomic weight, is almost the least promising species in the Periodic Table. Let us take as a premise, unprovable though this be, the assumption that better batteries can be made. Will they stem from improvements in existing couples, such as the lead–acid battery? In this case, we can, of course, calculate the theoretical maximum energy density. Or will they come from novel couples? Here we might note the American approach which has been to go for systems with the maximum theoretical energy density, such as $Li–Cl_2$. It is generally understood that the failure of such high energy-density systems to form usable batteries stems partly from construction materials problems, and partly from the very lability of the species involved, even at rest conditions. Most thoughtful battery scientists flinch at the thought of the large investments ploughed into the sodium–sulphur battery project which, twenty years afters its conception by Weber and Kummer from Ford Motor, still seems far from providing a commercially viable system.

Many scientists within the battery industry itself convey a certain impression of smugness, not to mention conservatism. Easy enough, they will say, to dream up a high-energy-density couple. Not that difficult to knock up something that registers on a voltmeter in the laboratory. Only they, however, within the industry, it is suggested, can really appreciate what lies beyond, namely, problems of cycle life, degradation of the battery, or loss of its charge during storage. They will call to mind the seemingly mundane problems of sealing the case, which put paid to at least one firm's efforts to market a Zn–air cell. And last of all, the killer, they will suggest, is the problem of making not just one battery that satisfies all these criteria, but the manufacture of hundreds of thousands of batteries. Where and how shall we find our better battery? Can we insist that it will be by means of one or more of the following processes?

(a) Evolutionary Development of Existing Cells.
(b) Novel "Engineering" of known couples.
(c) Totally novel couples/engineering.

Let us, using the lead–acid cell, review some of these approaches. The evolution of the SLI or traction (gauntlet) battery has been taking place, but how slowly! Thus, the idiocy of using almost the heaviest metal we know to act as a valence changer is matched by using that same metal (one of the weakest and softest) as a material of construction within the cell. Long ago, J. B. Cotton and others advocated the use of Ti grids and other Ti components in a battery. The idea found no favour. Presently, the rapid advances in conducting plastics leads itself to a re-appraisal of the approach. Beyond this, it is hard to see where significant evolution might lead. There has been no shortage of ideas in terms of revising the engineering approach, ranging from Faber's "metal wool" plates, to the construction of bipolar plates, not forgetting the Irish academic who stuffed a series of circular plates into an overgrown sausage skin. Work at Varta has shown how the pumping of electrolyte through the plates brings a dramatic increase in utilisation. But would the parasitic consumption of pump energy as well as its additional weight, outweigh the gains? All these ideas are based on the immobile and basically two-dimensional plate. More drastic still, is the idea of basing the battery on a slurry system, as the French showed could be done with a Zn-based system. Going even further was the "Lösungsbatterie" of Beck, where the Pb and PbO_2 reacted with $HClO_4$ to give a soluble discharge product.

Is there really nothing at all of value in these, and other, ideas? Have they really all been properly evaluated? One thing is certain, namely, that virtually nothing has been published about many of them, or a description of an idea is followed only by silence, so that a newcomer to the field, or maybe even the expert within it, is not to know where and how it failed. Was the idea properly implemented? Were the results correctly interpreted? Was the failure of the idea due to a weakness of construction materials which might today be overcome? The personal experience of the writer suggests that in at least one of the examples given above, the answer (in discussion with senior scientists in the battery industry) was in the negative. It may well be that the lateral thought required to make a major advance comes from outside the industry. All we can do to launch such lateral thinking is to present the facts, the history of ideas, in the most informative way and hope for the best. Inventions do not come to order, but by "clearing the ground" and presenting the facts succinctly, one does one's best to encourage them.

If we knew of a novel, workable, and economically attractive battery, the world would lie at our feet. But failing this, perhaps the best one can do is to "till the soil" until the right idea takes root. The history of the storage battery is so long, and so many generations of scientists have worked in search of better cells, that a very real danger exists of "re-inventing the wheel". This, perhaps, is no danger, but rather a means by which an earlier "near-miss" may one day be converted into success. This is the dilemma we must consider.