A FRESH LOOK AT THE PROSPECTS FOR ALGAL BIOFUELS AND CO-PRODUCT PRODUCTION

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Background

This review was suggested by the Manager of Bioenergy Australia (BEA) during a discussion at the BEA meeting of 7 July 2011 in Melbourne. The suggestion was made in response to my comments on the Participation Update presentation on IEA Task 39, *Commercialising Liquid Biofuels from Biomass,* prepared by Les Edye and delivered by Brendan George. That presentation was largely based on the report to IEA Bioenergy Task 39, *Current Status and Potential for Algal Biofuels Production* (6 Aug 2010). The authors of the report are Al Darzins (NREL), Philip Pienkos (NREL) and Les Edye (BioIndustry Partners). The full report may be found at: http://www.task39.org/LinkClick.aspx?fileticket=MNJ4s1uBeEs%3d&tabid=4348&language=en-US. I refer to it throughout as "Darzins".

A later, but closely related, paper by Jana Hanova, John Benemann, Jim McMillan and Jack Saddler in the IEA Bioenergy Annual Report 2010 (http://www.ieabioenergy.com/LibItem.aspx?id=6780) summarises, updates and extends the findings; I refer to this document as "Hanova". My review addresses both documents. The manager of BEA proposed that my arguments and evidence be discussed at the next meeting of BEA. BEA may also determine whether any follow-up action might be useful to a wider audience.

Introduction

Both reports contain a wealth of valuable material consistent with current thinking. In scientific discourse, however, no orthodoxy is above challenge. After all, this is one of the main ways by which science advances. Challenges may come, as in my case, from outside the establishment. Many worthwhile innovations have come from iconoclasts. It is pleasing that BEA members are prepared to countenance both constructive criticism and novel approaches. These may offer some of the potential breakthroughs necessary for the nascent biofuels and biorefinery industries to flourish as they must, in order to address critical, global challenges.

The Darzins paper is 146 pages long and the Hanova paper in the IEA Bioenergy Report 2010 is 35 pages, a total of 181 pages. At the risk of not doing justice to the very real contributions they make, I will deal in this paper only with the assumptions I am challenging, together with how changing them and adopting a new approach might more rapidly develop a viable and sustainable algal biofuels industry. My expectation is that the fresh approach will be shown to provide effective ways to avoid the show-stopper problems I identify in the assumptions and approach recommended by Darzins and Hanova.

It should also be appreciated that, whilst I attempt to base my challenge to their theory on scientific principles, my new approach is as yet untested. This means that there are opportunities for enterprising researchers and companies to test the validity of each of my proposed technological approaches and to investigate their various applications. Likewise, the overall concept may be modelled for viability, sustainability, and its prospects for existing industries and governments in their planning for a low-carbon future.

Discussion

The first point at issue is that the titles of both documents are too broad for their contents. The titles relate to the whole field of algal biofuels. The effect of equating the entire field with the relatively narrow ground covered by the authors is to discourage investment. Drawing on the full scope of what truly can be comprehended within the enterprise of algal biofuels and based on better assumptions, has the potential to attract the necessary industrial investment. The work described in both documents is admittedly partly based on hypothetical operating scenarios rather than on real production systems. The authors accordingly indicate that their conclusions are, at best, tentative. It is unsatisfactory that these vital qualifications tend to be buried in lengthy bodies of text. Futhermore, Darzins et al. do not acknowledge their restricted scope until pages 34, 64, 73 and 105. Even then, their list of omissions is demonstrably and damagingly incomplete. A more comprehensive list of what they exclude is:

- Any algaculture system that is not an open raceway pond; that is, all kinds of closed bioreactors, bioreactors where the algal soup has an air surface through which light penetrates, air-lift reactors, trickle-down reactors, biofilm reactors, covered ponds, sea- and lake-based systems, and fermenters.
- All mixotrophic and heterotrophic systems.
- Systems using anything other than high-lipid microorganisms.
- Sites that are distant from concentrated CO_2 sources.
- Sites that do not require massive, nearby and low-cost sources of water.
- Sites that are not *extremely* flat.
- Sites that are not preferably located on impermeable, rock-free soil.
- Sites that are not located in the dry tropics.
- Systems that can generate their own macronutrients.
- Systems that can extract and store CO_2 efficiently overnight.
- Systems that waste little or no nutrient.
- Systems that have little or no blowdown and disposal problems.
- Systems where factors such as algal predation, infection and competition are not effectively ignored in the recommendations.
- Systems that optimise light usage without incurring high-energy costs to create turbulence.
- Systems that have better uses for residual algal biomass than just for stockfeed or the production of methane.

Failure to include these items in a consideration of algal biofuels prospects misrepresents the broad scope of this promising field. Scientists and entrepreneurs have struggled for over seventy years to develop a profitable and sustainable algal biofuels industry. Pessimistically, both Darzins and Hanova conclude that we are still many years away from success. Darzins concludes that:

> The potential of algal biofuels must be framed by the realization that virtually none of the technologies necessary for their production … have yet been demonstrated at scale or in an integrated fashion under conditions resembling a full-scale production facility. The potential of algal biofuels is based upon bench-scale observations, limited outdoor production data, extrapolation, assumption, and limited critical and economic analysis. … [E]conomic feasibility is unknown. (Page 89)

Darzins also states that “process step (costs) were also prohibitively high” (page 104). In addition, on page 105, Darzins noted several factors that the authors did not take into consideration, most of which, I argue, will be shown dramatically to reduce the number of viable sites for their proposed algaculture system and its economics.

The Hanova report is equally pessimistic, concluding that:

> The creation of a vibrant algal biofuels industry will require continued and significant long-term industry and government RD&D support. … Research, development and demonstration breakthroughs and advances will be required along all stages of the value chain, from basic science to process engineering, including bench through pilot-scale process development and larger scale demonstrations. … To reach the required productivity levels, strain development will need to rely on genetically modified algae and the development of these strains will likely take a decade when regulatory approval, scale-up, etc., are considered. Assuming success in these first commercial venture and accelerated rates of adoption beyond 2020, construction of 1 million hectares of algae production systems by 2030 might be feasible. (Page 34)

Finally, capping off the dismal prospect under the scenario outlined in both papers, using the assumptions, tendered facts and reasoning of and from the papers, there appears to be insufficient suitable land. Even at their excessive estimates of slope that is suitable, only a few million hectares worldwide are identified as being suitable for their raceway system. And these may well have other current or better economic use. Moreover, the full extent of this land could only contribute around 5% of what is required for current world liquid fuel supply needs (see pages 34-35). By this thinking, algal biofuels will contribute far too little and far too late to the necessary decarbonisation of the global economy.

The two papers imply that the technology chosen for the Aquatic Species Program (ASP) that ran from 1978-96 is the only way to go; that no other approach can outdo it; that their chosen system can be improved only at the margins; and that no sufficiently transformative breakthroughs to provide economic viability are likely in the foreseeable future. This is despite the fact that the modestly-funded ASP trialled the ideas from only one or two scenarios out of the many possible.

Happily, there is at least one other scenario where solutions are tendered that, together, might not only deliver sufficient fluid fuels substantially to replace those from depleting fossil fuel reserves, and in a much shorter timeframe, but also might profitably deliver on other critical energy, food, chemical, material and global temperature requirements. In part, the new scenario does this because it amends what are seen as faulty or incomplete assumptions made in the papers, or which were accepted insufficiently-critically from those made by earlier workers. Hanova reinforces this point by stating that, “As has been shown in other biofuel sustainability studies, even seemingly small changes in these types of assumptions can lead to quite different conclusions” (page 9).

Assumptions Critically Affect Outcomes

It should be no surprise that one’s assumptions and their comprehensiveness determine, perhaps more than any other factors, what are the outcomes of much research. It is mainly these probably faulty assumptions, together with a certain dearth of lateral thinking, that have led to the essentially pessimistic outlook of the papers. This review seeks to replace the faulty or incomplete assumptions with better and more complete ones. Changing the assumptions, and developing technologies to match, leads to a much better outcome – one which indicates that profitable algal biofuel and co-product production can be achieved in the short to medium term.

It will be noted that the new assumptions do not go all one way. Some actually benefit the narrow scenario(s) outlined to the IEA by the papers. However, most do not.

Of course, there is no guarantee that the new assumptions will themselves be without fault or gap. That is an unreasonable demand. However, at least readers will be able to assess and discuss whether they represent an improvement on the older ones – perhaps even proffer further improvements themselves.

AVAILABILITY OF SUITABLE LAND FOR ALGACULTURE

The two papers take different assumptions regarding the acceptable maximum slope for sites on which raceways of such size may be constructed economically, Darzins says 5%, Hanova 2%. In fact, both appear to be seriously, non-conservative estimates as 0.5% will be shown to be more reasonable.

For the purpose of the exercise, assume an even 2% slope, such as might be found on flat land sloping down to the sea. Now, the recommended raceway is over 1,000m long and is 160m wide. In the most favourable raceway orientation to the slope, this means that one side is 3.2m above the other. In the least favourable orientation, one end is 20m above the other. Thus, noting that the entire biofarm of hundreds or thousands of raceways is likely to have them aligned or otherwise close-packed (even terracing would cause major problems for their economic model), and taking a rough average for the real, variable terrain, excavations averaging around 10m may be required over much of each raceway. Alternatively, material excavated to around 5m may be moved to fill the low points. However, such a cut and fill operation would require that the consolidation of the fill be such that waterlogged fill would sink less than twenty centimetres over a period of 25 years – otherwise the raceways would neither function properly nor possibly even hold water. Even with expensive compaction, any fill over one metre deep would be unlikely to meet this requirement. It is submitted that sites would be uneconomic for such large raceways having average slopes of greater than 0.5%, or one tenth that proposed by Darzins and one fourth that proposed by Hanova. There is very, very little land in suitable, underutilised coastal areas with slopes less than 0.5%.

Unfortunately, that is not the worst. Low, coastal land of the sort required by Darzins' raceways is typically subject to storm surges, flooding and tsunamis. It tends to be vegetated and/or inhabited by humans, other industries, or protected wildlife. Often, it has heritage or scenic value. Infrastructure tends to cross it. Due to the expected minimum economic size of such algal biofarms being in excess of 10,000ha (this might accommodate ~500 raceways), the biofarms could not readily be nestled amongst existing land uses, claims and infrastructure. Even systems of as little as 1,000ha, such as is suggested in Darzins (page 72), might not be easy to accommodate. (See Darzins et al. pages iii, vi, 9-11, 30, 51-55, 57, 60, 62, 74-76, 87-88, 100, 105, & 123-124 and Hanova et al. pages 21-22, & 34-35.)

COST OF LAND & SITE PREPARATION

Most coastal land is regarded as having valuable seaside amenity. Typically, it has other beneficial uses. These factors mean that it is usually not cheap, even if it is available and zoned for algaculture. Darzins bases most of his capital and operating cost assumptions on those of Benemann et al. (1996). Benemann uses a land value of $12,500/ha and pond construction cost, including site preparation that presumably includes levelling cost, of $8,300/ha. Infrastructure, plant and equipment costs are additional. Most of the pond construction might be expected to be for site clearance, levee construction, access roads, drainage, compaction and geotextile fabric. Thus, little or nothing would be left for levelling and severe compaction, certainly not enough for the scale required for even 2% sloping land, far less 5%. Nor are the potential problems of encountering rocks and rock strata, and the effects of potentially-necessary terracing addressed.

Algaculture sites are better selected from really cheap, flat land, wherever it occurs in suitable latitudes. Growing containers for algae should not be so large as to require substantial site-levelling and preparation costs. Preferably, they ought to be in minimally-vegetated areas and the containers able to be aligned along contour lines to minimise costs and environmental disturbance (See Darzins page 124).

AVAILABILITY OF SUITABLE WATER

Using large raceways, it has been shown that there is insufficient, suitable coastal land available to support an algal biofuels industry of the size necessary to replace a significant portion of fossil fuels. Pumping very large volumes of seawater inland, daily over long distances, or to anything more than lifts of less than a few metres, is uneconomic. Thus, either the water must be conserved by using different methods of algaculture to open raceway ponds, or other sources of water are required, or both.

Except in unusually wet conditions, permanent surface water resources are already typically over-committed in low and mid-latitudes. This leaves aquifers and seasonal flows. Many aquifers are salty or are themselves over-extracted or soon to be so. This leaves free only salty lakes, sewage or polluted industrial water, uncommitted aquifer water, and the portion of seasonal flows that can be spared. However, many aquifers, both salty and fresh, are fairly deep and require much energy to pump the water to the surface. Moreover, after-use disposal (blowdown) of salty water or brine can cause major problems. Thus, even where cheap, renewable power is available, we should resist extracting large volumes of water from aquifers on a regular basis. For purposes of algaculture, this leaves available only: water from sewage or industrial sources; from possibly-erratic seasonal flows, most of which would need to be treated and stored; and/or from relatively small volumes of shallow, uncommitted, probably brackish or salty, aquifer water – all of which would need to be conserved. Anything else is unsustainable. Hence, open raceway ponds are out, as they lose too much water to evaporation, even for halophyllic (salt-loving) algal species. Nor may evaporating water be used regularly to cool bioreactors. Different technology is required to meet these constraints (See Darzins pages 55-56, 65 & 68 and Hanova pages 15, 22 & 23).

COST OF WATER

For sensitivity analysis, Darzins understandably assumes different costs for water having different quality and uses. Darzins' paper assumed that water for processing (not salt) water cost \$20/ML (see page 71), though on page 124 it seems to have been wrongly typed as \$0.20/m^3 or \$200/ML. Whilst currently generous, \$20/ML may not be adequate after we fully enter the era of water shortages, climate change, over-population and water wars. Darzins' study also found that varying the cost of process water had potential for significant cost impact on the biofuel produced. As water costs increase, either for pumping, access, treatment or blowdown, so the viability of technologies requiring large amounts of it declines (See Darzins pages 55-56, 71-72, 76, 83, 85, 119 & 124).

SUPPLY OF CARBON DIOXIDE (CO_2) NUTRIENT

With few reservations, Darzins assumes that algal biofarms must be in close proximity to cheap, substantial and concentrated sources of CO_2. Thus, he imposes on virtually all prospective algal biofuel solutions, including his own, a major constraint that is *quite unnecessary.* This faulty assumption both reduces the number of viable sites by a large factor and typically may force those using his preferred system into seeking to sequester for their raceways, land that may be arable or forested, if that is the only land available near to major, CO_2 emitting facilities.

The strange thing is that the Darzins study actually identifies the solution – or at least a significant part of it – but fails to develop and use it. CO_2 pipelines are already in substantial use by the gasoil industry to enhance hydrocarbon recovery by pumping CO_2 into hydrocarbon reservoirs. In addition, more such pipelines are planned or under construction for the carbon capture and sequestration (CCS) industry. Others are being conceived to serve the carbon capture and utilization (CCU) industry. There is even scoped for the USA and Britain national utility grids for CO_2. Darzins does not go as far as to suggest a utility grid for flue gas, analogous to those for natural gas, water, irrigation/flush-quality water, stormwater and sewage, but that is a logical extension. If we can mine good water from wastewater, there is no reason why we cannot mine CO_2, minerals, heat and energy from flue gas and other emissions – the technologies are mainly there, just not widely applied. Nor would such mining only apply to emitters emitting CO_2 at relatively high concentration (>10%). Some of the technologies work quite happily at concentrations of just a few percent.

The beauty of a utility model for both flue gas and CO_2 is that it would also take care of the twin problems of storage, and of daytime-only consumption of CO_2 by autotrophic (photosynthetic) algae versus its typically 24x7 production by CO_2 emitters. Having large capacity grids that conduct supercritical CO_2 long distances to users automatically smoothes out supply-demand fluctuations. It also opens entire continents to the CO_2 market – indeed to the whole world by way of backloading CO_2 in LPG tankers. Should the CO_2 be cooled to a liquid for transportation, the energy cost of cooling each gas might be offset at each end of the voyage by treating the other gas.

By employing the utility model for flue gas, there is no need to separate the CO_2 from other flue gas components gases at source. Anyway, separation at source would be uneconomical for small emitters. Employing the utility model means that far more stationary emissions, large and small, intermittent or not, can be captured at source than by other means. It also means that far more potential CO_2 users could take advantage of the CO_2 and flue gas utility grids (See Darzins pages vi, 49, 57-60, 70 & 118-119 and Hanova pages 19-21).

COST OF CO2

Darzins' base case assumed that CO_2 was available at a cost of $50/tonne, net of any biosequestration value. Although the issue is yet to be resolved internationally, a biosequestration value of half that of long-term sequestration does not seem unreasonable. Even the full value could be justified. After all, the creation of biofuel avoids extracting and using fossil fuel. Thus, for a CO_2 sequestration value of, say, $60/tonne there would be a credit of at least $30/tonne to offset interest on capital, capture, compression, separation and transportation costs for a CO_2 capture system. For a $180/tonne CO_2e (CO_2 equivalent) value, the credit would be at least $90/tonne. It is submitted that Darzins' base case substantially over-estimates the delivered net cost of CO_2 to the algal biofarm under the improved scenario that uses a utility model for CO_2 capture, processing and transportation. The cost is more likely to be zero or negative (See Darzins pages 70, 76, 81 & 124 and Hanova pages 19-20).

RELATIVE COSTS OF RACEWAY VS BIOREACTOR CONTAINMENT

Many studies have concluded that raceways are much cheaper to construct than are closed bioreactors. Typically, this is indeed the case, as may be seen from simple observation of the publicly-available designs and subsequent rough estimation of their capital cost per unit horizontal area of intercepted sunlight. However, there a number of bioreactor designs where the capital costs are similar to those for raceways, particularly when harvesting plant, controls, land, levelling & compaction costs are included.

Raceway construction, of the design favoured by the Darzins paper, by its very nature, cannot be mass-produced. Furthermore, raceway ponds have a long history of development. For these two reasons, any experience curve productivity gains in their future design and construction are likely to be small. On the other hand, bioreactor designs suitable for mass-production can expect to achieve very substantial productivity gains, particularly when their evolving design takes into account transport, adaptability, deployment, recycling, replacement, and land remediation factors.

With such large raceway ponds as Darzins posits, each having a circuit length of some 2km, it is also hard to see how the beneficial, turbulence-inducing effects of the rotating paddle wheels could extend to more than 20% of the entire circuit, nor how the carbonated nutrient would not be substantially consumed, or vented to the atmosphere, long before it could be renewed – thus decreasing both the areal and the volumetric algal productivity of the raceway to a similar degree.

Hanova arbitrarily restricts bioreactors to ones being cooled by evaporation or by being immersed in deep ponds (or the sea, presumably). This fails to imagine or take account of designs that use neither of these cooling methods, or that there may be cool, temperate, or elevated regions and times where cooling is not required. This is all the more surprising, as shading by means of canopy vegetation, of slatting, or of various forms of shade-cloth are means well-known to the horticultural industry for shielding terrestrial plants and aquaria from excessive temperature or insolation (sunlight). Furthermore, it is no great jump to consider that slats covering a moving mass of algal soup might be used to provide the algae with beneficially-flashing light; or that the slats themselves might be composed of thin-film, photovoltaic material, sitting atop each bioreactor, to generate a valuable, electricity co-product; or that the slats might be so constructed as to provide greater shading, the higher is the temperature in sunlight.

Hanova also states that "(all) commercial systems (bioreactors) have … many scalability problems", whereas there is no reason why some designs of bioreactor, being already of sizes ranging up to hundreds of square metres, might not be infinitely replicable, in the same way as leaves on a ground-hugging vine or a tree. Have not forests scaled sufficiently well in the past to cover substantial portions of whole continents? She does, however, note that, whilst making her own comparisons, "additional work such as the definition of critical sustainability factors needs to be done before any meaningful comparative analyses can be carried out". Hanova also admits that "these economic analyses did not factor in several cultivation challenges, including competition by contaminants such as grazers and pathogens, which would arguably reduce productivities and raise costs. These issues are particularly acute in the operation of raceway ponds". They do not affect closed bioreactor systems to nearly the same extent, and some multiply-protected systems may be nearly immune. Nor are bioreactors subject to invasion by rooted, water-loving plants, such as reeds and rushes, together with insects, molluscs, crustaceans, amphibians and wildfowl, to which invasions large complexes of open, shallow and unlined raceway ponds are subject. Furthermore, raceway ponds located in dry, tropical areas could be subject to sand and dust storms filling them sufficiently to require fairly frequent re-excavation (See Darzins pages iii, 30-33, 37, 64, 69, 75, 79 & 83-84 and Hanova pages 15-16 & 31-33).

CONTAINER OPERATING COSTS

Darzins raceways are acknowledged to require huge, new amounts of water daily, together with commensurate blowdown costs. Should this water not be free or be of *very* low cost, or should it require pumping up more than a few metres, then the potential economics of this raceway system are gravely, and almost certainly fatally, compromised. These and other factors mean that very few sites are suitable for the proposed Darzins system – so few as to be almost immaterial, except in the minor case where wastewater remediation and use is feasible. And even in that case, it will normally be found to be more profitable to use gravity well reactors to convert any wastewater biomass cost-effectively into syngas and to re-use the resulting, sterilised and partly de-mineralised water for regional irrigation, flushing or industrial purposes. After all, these uses tend to save on distance pumping costs in both directions. Such technology breaks the nexus between the populated regions that typically produce biomass-rich wastewater and the typically-distant location of areas of cheap, flat land suitable for algaculture. There is thus no comparative net benefit in using vast areas of algal ponds, bioreactors or fermenters to treat city sewage or organic-rich, industrial wastewater.

In Darzins base case, "it is assumed that the value of the algal cake produced … has no value other than its energy content" though later on there is discussion regarding its (possibly compromised) use as animal fodder or for the extraction of chemicals from it. However, this last use is discounted due to the supposition that such a large supply base would swamp the markets, presumably rendering them much less profitable, or indeed unprofitable.

Using algal cake just to produce methane, heat or energy is wasteful indeed of a valuable protein, carbohydrate and bio-actives resource. But it is not correct to say that all markets would necessarily be swamped by such a large, new source of high-quality biomass. As we are over-fishing the sea, there is thus an increasing demand for aquatic species to be grown semi-domestically and fed using just such premium biomass as algae or algal residues. After all, algae form the direct or indirect food source for most aquatic life. Most domestic, terrestrial animals can also benefit from a substantial addition of algal products to their diet, as can we. In addition, either algae themselves, or microorganisms that can use algal residues on which to grow heterotrophically, can be induced to produce most of the organic chemicals or feedstocks on which our civilisation depends. Algae can even be converted cost-effectively into biofuel, biochar and recyclable nutrients. Using algal biochar to improve our soils, whilst biosequestering carbon cheaply and safely in them for millennia, could alone take up all the excess CO_2 in the sea and atmosphere – though it is probably better to use waste, low-grade, cellulosic biomass for this.

The valuable input materials lost in the Darzins proposal are substantial. The raceway model figures recorded on page 119 indicate that the following daily losses are expected to occur: 28% of CO_2, 49% of urea, and 83% of DAP, a combined nitrogenous and phosphatic fertiliser. Such high losses occur in Darzins' system because so much nutriated water is required to be returned to the sea every few days as evaporation makes it too salty to retain. There are other algaculture systems that waste virtually no nutrients.

Disposal of the briney, nutriated effluent from the Darzins system may also cause additional costs and problems. Although the model run on page 121 gives much reduced losses, these too are serious and may well be unsustainable. The fossil-fuel-derived chemicals and the natural gas used for processing are also unsustainable.

On page 32, the Darzins paper makes the incorrect assumption that all bioreactors have their algal media in contact with their upper, containment surface, and thus are subject to surface fouling by microorganisms, with the associated cleaning problems and costs. This is *not* the case. Some bioreactor designs have a gas layer between the algal media and upper, transparent containment. For these, any insolation (sunlight) reduction caused by the containment layers are typically more than made up for by the increase in sideways insolation in bioreactors that may itself be supplemented by the use of cheap reflectors. The gas layer can also be used to address gas transfer problems, whilst capturing the oxygen emitted by the algae as a valuable co-product that open raceways waste.

Generally speaking, it is the long-term algal productivity per square metre that counts. It is here that some bioreactor designs may completely outclass even the best raceway designs, as well as being productive in a far wider range of climates, terrains, soil types and locations. (See Darzins pages 32-33, 72-78, 83-85, 118-119, 121, 123-125 and Hanova pages 9, 15, 17 and 31-33)

HIGH-LIPID STRAINS, STRESSING & GENETIC MODIFICATION

High algal lipidity (oil content) is known to run counter to high algal biomass productivity. As such, it is doubtful that genetic modification (GM) techniques will be able substantially to alter the relationship, to say nothing of the introduction of new difficulties caused by GM. Excluded from this tentative guidance are more general algal selection, breeding and modification efforts that deal with matters such as biomass productivity, nutrition, robustness, tolerance, disease-resistance, adaptation, the size of the algal antenna and other light-harvesting, CO_2 fixation, energy & chemical conversion mechanisms. As with other domesticated organisms, these are most likely to be massively improved by such efforts, particularly for algae grown in protected environments, at least insofar as their utility to humans is concerned.

Similarly, stressing algae by nutritional or mineral deprivation, by temperature or other means, tends to have a negative effect on biomass productivity, whilst it may indeed lift lipid content. Both methods tend to be zero-sum games. Moreover, as there are many methods by which non-lipid and polar lipid biomass may be transformed into drop-in biofuels, strenuous and costly effort to maximise the neutral lipid content of algae appears to be somewhat misplaced – though of course a level of effort should be maintained in order to understand the various biosynthetic mechanisms at play, and in case the productivity relationship or the nature and proportions of the products can indeed be overturned or modified without significant reduction in biomass productivity.

Whilst generally supporting the probably mistaken objective of high-lipidity, both papers make otherwise reasonable conclusions regarding biomass productivity, stressing and GM (See Darzins pages pages iii, 15-16, 18, 22-27 and Hanova page 23).

ALGAL PRODUCTIVITY

The papers appear to be conflictive regarding which is potentially the more productive, raceways or bioreactors - coming down apparently on different sides on different pages. Understandably perhaps, due to over-hyped claims, they also seem to be unduly pessimistic regarding how close we can get to the theoretically maximum areal algal productivity that is taken to be 100g/m^2/day of algal biomass for the active, horizontal area of photosynthesis. Their figure for previously sustained production in open ponds (or raceways) is around 20g (range 15-30) for a semi-industrial operation, representing an incident light-energy utilization efficiency of only 20%. Their Higher Productivity scenario suggests a (future) biomass productivity of 50g/m^2/day. It is submitted that even this figure is somewhat low, because there had been insufficient imagination and has been up until now, insufficient research into ways to improve it. Methods are now available that, taken together, are prospective to improve on this last figure, and in the relatively short term.

Whilst discussing some of the problems that limit algal productivity, the papers suggest few means by which these problems may be mitigated or overcome. Thus, solutions to problems such as insufficient light, light saturation, photo-inhibition, self-shading, photo-oxidative death, sub-optimal nutrition, congestion & waste removal tend to be restricted to increasing energy-intensive turbulence, the use of optical fibres (that introduce major troubles of their own), or to reducing antenna size, rather than by less intrusive methods, such as providing flashing light regimes, additional light, or optimal algal nutrition by other means (See Darzins pages iv, 3, 8, 10, 20 & 65 and Hanova pages 15-16 & 18-19).

CONCENTRATING ALGAE AND SEPARATING THEIR COMPONENTS

Darzins assumes that algae must be concentrated to a wet paste at 10% to 20% solids, prior to further processing. This degree of dewatering is an unnecessary and an expensive assumption, as there are economical processes that rupture algae in water whilst they are at much lower concentrations, such as 2-4%. Once ruptured, the resulting lipids, aqueous proteins, and solids can readily be separated economically, either by old-fashioned, milk separation technology or by a modern, in-line, three-phase centrifuge. These methods work efficiently and cost-effectively, as once algae are ruptured, the three product phases no longer have the near-neutral buoyancy of whole algae and thus are easily separated, each having a markedly different density as well as hydrophilicity (See Darzins pages iv, 37, 39, 41-42 & 119 and Hanova pages 24-25).

CO-PRODUCT VALUE CONTRIBUTION

Both papers mention ways by which valuable co-products may be captured from algal biofuel residues. However, the ways suggested and products are very limited, whereas algae are a premium resource for a host of chemicals, nutraceuticals and bio-actives. There is all too little discussion of what these might be, of cost-effective extraction methods for all or individual molecular species, and of markets. Nor is there much discussion of the value of whole algae in water as premium fodder for molluscs, crustaceans, and fish larvae. The potential size of this one aquaculture market seems to have been totally ignored. Moreover, the suggestion to use algal residues as a substitute for fossil fuel in power plants would be both ridiculously wasteful of a valuable, high-protein resource, and would become a likely source of particularly harmful NO_x, PO_x and SO_x greenhouse gas (GHG) emissions (See Darzins pages v, 7, 9, 12, 31, 65, 70, 74 & 78 and Hanova page 25).

FEEDING ALGAE BY AUTOTROPHY, MIXOTROPHY OR HETEROTROPHY

The papers have elected not to consider seriously algae that grow by heterotrophy (eat biomass to live, grow and reproduce) or mixotrophy (consume light and/or biomass). Only autotrophic algae which grow exclusively using light as their sole energy source are considered seriously, despite the fact that many algal species can utilise more than one method, and that many more can probably be induced to do so. These are serious omissions. Papers stating the potential for algal biofuels and co-products *cannot* be regarded as being any way complete without their positive inclusion.

Amongst other things, the authors have failed to recognise the potential of mixotrophic feeding, where daytime autotrophic feeding gives way to heterotrophic feeding at night, or where mixotrophic feeding can occur in overcast or moderately low light conditions, such as at sunrise and sunset. Fodder for heterotrophic feeding can come from a number of sources, including from hydrolytically-treated waste biomass, from algal residues, or from the low-cost, impure, glycerol co-product left over from the transesterification process of algal lipids into biofuel. Such fodder has the potential to transform the overnight algal biomass loss due to respiration (~25% per night) into additional overnight biomass gain that might be as much as 15% or more - a notional 53% plus productivity improvement at minimal cost. Nor should purely heterotrophic microorganisms be dismissed, as even though they may not normally be grown principally for their biofuel potential, biofuels may result as co-products.

It seems no excuse to say that the papers were only intended to review the current status of the technology and established, commercial processes, when components of their proposed solutions are themselves hypothetical or unproven on a commercial basis, and when, to be most useful, the papers needed to provide a sense of what might well be achievable in the foreseeable future. As they stand, and when read closely, they are (wrongly) highly discouraging of algal biofuel enterprise (See Darzins pages 1, 18, 21, 34 & 44 and Hanova page 20).

EXTREMOPHILES, FLUE GAS & CLIMATE TOLERANCE

As the papers concentrate on a method to grow algae in dry, tropical climates, so they virtually ignore other suitable climates and the algal strains and species that might grow well there. They knock out virtually 80% of all algal species, including those that grow best in temperate or cool environments, both natural and crafted, or in media rich in salts other than sodium chloride, such as carbonates, sulphates and sulphides. Nor is any mention made of using different strains for different times of year. This simple concept could ensure year-round algal productivity in climates where otherwise growth would not occur for months at a time. The papers do, however, mention in passing the possibility of developing strains with certain characteristics.

One of the characteristics mentioned is the ability to utilise flue gas *directly* as a multi-purpose nutrient. However, it is not discussed why the somewhat doubtful possibility of developing such an extremophile would be preferable to treating flue gas so that its nutrients (some of them highly toxic) could be separated and the benign residuum used by a wide variety of algal strains, at all times of the year. It would also appear somewhat more likely, that robust and highly-productive algal strains would use metabolic pathways suited to conditions near the middle of the viable range for algae, rather than those at the extremes (See Darzins pages 23-27).

SUSTAINABILITY

Frequent blowdown of large amounts of fertiliser produced by the Darzins system, even if this could be made or extracted using non-fossil-fuel resources, makes the system essentially unsustainable. In particular, it violates the sustainability criterion that water quality be maintained or improved. It sends to waste, part of our critical, phosphorus resource and is likely to cause harmful eutrophication due to the release of nitrogenous fertiliser. Furthermore, due to the system's tight, locational requirements, such as being near a major CO_2 emitter, wastewater facility and/or coastline, it is likely to need to take over either arable land, land already populated, or which is in intensive use by other industries. In its processing method to produce biodiesel, the system Darzins proposes uses large amounts of natural gas, electricity and chemicals, most of which are fossil-fuel based. Some other algaculture and biomass-processing systems proposed are likely to be much less profligate of finite resources (See Darzins pages 35, 47-48, 61-62, 119, 121 & 125 and Hanova 11-12).

CONCLUSION

We do not have to wait ten or twenty years for uncertain research results. We have the concepts and technologies now to create a viable algal biofuels industry. They just need to be tested and put together.

The new industry will not be based principally on the current orthodoxy of raceway ponds, but on low-cost, mass-produced bioreactors. These will typically not be located on coastal land, or necessarily near to major sources of either water, population or CO_2. Instead, they will typically be located in the wastelands and degraded areas of all but cold and mountainous regions. They will come not to depend on fossil fuel for power, fuel, materials or fertiliser. Instead, they will generate these as co-products for in-house use and external sale. They will tend to conserve both water and nutrients, deriving both from regional and occasional sources, except in the case of CO_2 that will be one of the new, pipelined utilities. In short, they will be sustainable.

The new industry will integrate with an emerging biomass biorefinery industry, each leveraging off the other, from other renewable energy industries, and from older, established industries. Together, they offer profitable paths to a low-carbon economy for many of these older industries.

All that is required is a modicum of applied R&D to validate, refine, and characterise the several technologies; and then to model their outcomes sufficiently well as to reduce risk and generate investment in intellectual property commercialisation, infrastructure, facilities, logistics, financing and market development.

These, and the assumptions overturned above, are the ideas for which BEA discussion is sought. One formulation of the proposed new concept and replacing technologies is available for those interested, subsequent to the meeting.

Take home message: ***Forget raceways, use low-cost, mass-produced bioreactors***.