

Don't Stop Thinking About Tomorrow: Research & Development

Mañana is now: R&D and Per Capita Economic Growth

So far we have dealt strictly with what we know how to do now. Current technology could allow us to phase out fossil fuels over the course of 30 years, along with drastically reducing timber harvesting, Portland cement manufacture, and other greenhouse emitting activities at essentially zero cost, while allowing normal economic growth through 2010, and keeping up with population growth thereafter.

There are a number of reasons we want to do better than this. To start with, we do want per capita economic growth, so long as it does not come about at apocalyptic costs, and genuinely benefits the people of this planet. For another, while a 30 year phase out gives us a decent chance avoiding the loss of our industrial civilization or worse, we would like better odds than that. (Think about how you would feel if as the result of a medical visit the doctor were to say in a cheery tone “congratulations – you have a 75% chance of surviving the year.”) For a third, relying strictly on renewable and efficiency technology near the current price of fossil fuel requires us to make choices we’d rather not make – such as continued use of hydroelectricity, the use of a larger quantity of biomass than is strictly preferable, and the creation of a fairly extensive additional high voltage transmission network.

As I mentioned before, when we talk about economic growth beyond population, (per capita economic growth) it is reasonable to talk about breakthroughs that are not here yet; the technology to provide that growth, also, is not here yet. Imagine as a thought experiment that technological innovation ceased. You would continue to get per capita growth in developed nations for a while, based upon stuff “in the pipeline” that is not yet implemented. Past that, growth would stop, other than with population - assuming you did not simply force people to work longer or under harsher conditions. So if we are talking about growth all the way through 2050 it is reasonable to assume some technological innovation. Will it necessarily occur in the renewable energy sector? It always has in the past. Besides this kind of thing is a choice not a prediction. If we intelligently fund research and development in a large number of promising renewable sectors, we will get results - not in everything we fund, but in a high enough percentage to repay multiples of total investment. Here are some examples we might consider. This is not a research program, just a very incomplete list of projects worth further investigation.

The single most desirable breakthrough would be the development of inexpensive photovoltaic cells. This alone could bring about a hydrogen present – even without the fuel cell breakthrough usually paired with it. If we could bring down the price of PV down to the point where it produced power for a cent per kWh, (instead of the 25 cents of present), we could produce solar hydrogen for around \$13 per mmbtu with existing commercial electrolyzers – about 2.3 times the cost of natural gas – within the price we could afford for renewables in the efficiency scenario. This could displace biofuels for industrial, commercial and residential uses. How about transportation? Without inexpensive fuel cells hydrogen is not a particularly suitable transportation fuel. But since we are already looking at drastically reducing transportation energy demand, with a significant portion of that electricity we end up with no more than around 8 quads required for liquid transportation fuel – something we can produce from waste, agriculture and genuine tree thinning that does not take live healthy mature trees, and modest energy farming on existing timberlands - leaving most of our remaining forests untouched.

How hard would such a breakthrough be to make? After all we've been hoping for this for at least 30 years. Well there are never any guarantees. But there is one approach Barry Commoner suggested back in the 70's that a Danish consulting group recently revived. The theory is that the price of solar cells is a chicken/ egg problem. Because demand is low we never get large factories with full economies of scale to bring down the price of solar cells. Because the price is high demand stays low.

The usual suggestion for breaking the deadlock is demand pull – increasing orders to the point where large scale factories could be built. However, the Danish study suggests a large scale supply side approach – building a giant large scale PV factory accompanied by a large scale plant to manufacture solar grade silicon³¹⁵. Currently PV factories buy computer grade silicon, or process computer chip scrap.

The cost would be well below a billion, with a high probability of success and huge potential payouts.

If it succeeded, we could eliminate the need for a huge network of high voltage transmission lines. With cheap enough solar cells you can produce cheap inexpensive electricity almost everywhere – from Southern California to Northern Massachusetts. The few places where this is not the case, like Alaska, have low population densities and nearby access to wind, water, geothermal or biomass. With \$0.01/kWh electricity, you can mix in large amounts of storage without overall electrical price being outrageous.

Is the building of such a pair of factories really likely to bring the price that low? Initially, the answer is no. Cells produced from such a factory are likely to produce electricity no cheaper than a nickel per kWh – about the same price as fossil fuel based electricity, but not cheap enough for a no fuel cell/cheap electrolyzer hydrogen future. But if solar cells really are a chicken/egg problem then the virtuous cycle does not end with the construction of a pair of factories. Once the demand is there, other manufacturers will compete for the new market, applying some of the research that has been completed but not applied to produce PV at a lower cost. And others will compete with them, and so forth. So we have a very good chance of getting \$0.01 kWh electricity out of it.

Even if not, merely producing solar cells through true mass production, and providing a dedicated source of solar grade silicon, thus lowering solar electrical costs to ~five cents per kWh is worthwhile in itself. And frankly I think at least 2 cents to be likely, and 3 cents almost certain.

This does not mean there is no research left to do on PV. The European Commission suggests the following³¹⁶:

- 1) *Low-cost and high-quality silicon feedstock;*
- 2) *Optimisation of crystalline silicon process technologies with particular emphasis on cost and efficiency of wafer cell production;*
- 3) *Thin-film technologies: highly efficient mass production plus an understanding of material limitations, aimed at reducing costs;*
- 4) *Innovative PV concepts for PV cells and modules which have a potential for large cost reductions (such as tandem and concentrator cells, new materials).*
- 5) *Research on reducing the cost of other new and innovative components and systems*

There is another path to a solar future besides PV, one that may be easier to reach. Solar thermal electricity runs 11 cents per kWh, and \$40 per thermal equivalent of a kWh of storage.

However, the National Renewable Energy Laboratory believes they can lower storage cost from the current ~\$40/kWh to around \$15/kWh²⁸⁰ - which would lower storage costs to about a cent per kWh over the lifetime of a plant. There is probably room to lower the cost of solar thermal electricity further as well. If we can do these two things, we can phase out hydropower and avoid most biomass use increase without needing a hydrogen path- though hydrogen remains desirable.

The most critical lack preventing a hydrogen present is not fuel cells but inexpensive electrolyzers. Currently Stuart Energy can provide 75% efficient electrolyzers for around \$400 per KW in 5 megawatt sizes and higher. If we could lower this to \$100 per KW then hydrogen could be brought down to around double the price of natural gas even at 3 cents per kilowatt hour (probably attainable via wind in the near future). This is in part another chicken/egg problem. Demand for electrolyzers is not high. A well designed mass purchase program might by itself bring down prices to that level. If not, it would be well worth an R&D program.

What about fuel cells? I'm not as convinced of the centrality of fuel cells as many. We can use a combination of efficiency and other renewables to meet our needs without them. With the appropriate use of electric trains to substitute both for many auto and plane journeys, heavy rail for freight instead of trucks, electric autos, and non-hydrogen hyper car technology, we can run that part of transport requiring fuel on a comfortably sustainable amount of biofuel –so long as hydrogen or cheap renewable electricity is available for industry. (Otherwise we keep hydro for electricity, and end keeping an uncomfortably large portion of tree plantations continuing to operate as such, rather than converting them into forest.) Still having inexpensive fuel cells available would be highly desirable. They would help increase the efficiency of Hypercars, just as Amory Lovins suggests. Without cheap hydrogen they could still cut carbon emissions during the transition to a renewable society. With cheap hydrogen, that is cheap renewable electricity and cheap electrolyzers, they constitute an inexpensive source of massive amounts of reliable renewable electricity.

They are not as mature a technology as solar cells or electrolyzers, and I'm not completely convinced that fuel cells are a chicken/egg problem rather than something requiring more research. But such research would pay in a number of ways. For one thing, developing an inexpensive fuel cell would almost certainly develop an inexpensive PEM electrolyzer (the most efficient and currently the most expensive type of electrolyzer) because they are by definition the same problem. Extracting most of the available energy from hydrogen in the process of chemically converting it back water (rather than simply burning it) is a lot more difficult and complicated problem than using electrical energy to convert water into hydrogen and oxygen. If you can do the first inexpensively, you can do the second even more economically.

Hydrogen's main potential value is not for transportation, though that is often suggested, but as one possible means of electricity storage, and as a source for high temperature industrial processes.

As we saw in earlier sections hydrogen is not the only potential means of storing electricity. Today's flow batteries offer lower costs than hydrogen as a means of electricity storage, and a higher round trip efficiency than hydrogen is ever likely to have. Research and development can lower the cost of hydrogen storage – but this is true for flow batteries as well. And even more than with fuel cells; research is definitely needed, it is not a simple chicken/egg problem. I contacted a leading flow battery manufacturer (VRB associates) to ask them how much larger batteries could lower the cost. They told me that they thought \$225 per kWh was their current limit, that beyond that you run into diseconomies of scale. No doubt mass production could also bring costs down. But multi-megawatt batteries are not like automobiles. There are limits to how many you will ever produce in a year. In short to bring flow battery costs down will require major research investments, not merely demand pull – just as with fuel cells.

In terms of automobiles, lithium ion batteries for electrical vehicles are much closer to commercialization than fuel cells. It would not take much to bring them down to a reasonable price for auto use – making hybrid EVs or plug-in hybrids possible. A hybrid EV or plug-in hybrid relies entirely on stored grid power for short journeys, fuel for longer ones.

We also need greatly increased research into reversible chemical reactions for storing thermal energy – high temperature heat, low temperature heat and cold. A lot of efforts have focused on phase change, and they remain promising, but such reactions as metal hydrides and zeolites deserve more attention. Low quality natural zeolites may be as effective as high quality synthetic ones for storing thermal energy below the boiling point of water - which might make solar energy practical for close to 100% of low temperature needs.

In addition to solar energy, there is a tremendous untapped in potential in wind class 4 and below, probably accessible only via small wind. Right now small wind is much more expensive than utility scale wind farms (per kWh), but if the price can be brought down it has about 20 times of potential of wind farms – another possible path to a hydrogen future (which requires electricity cheaper than fossil fuels rather than merely competitive with them). So far efforts at small scale wind have focused either on conventional horizontal turbines, or on unconventional vertical ones – with neither resulting in the cost reductions we need. The Selsam wind turbine uses multiple horizontal style turbines on a tilted tower – getting some of the low capital costs of vertical wind, and some of the higher efficiency of horizontal ones³¹⁷. (It has potential for utility scale wind too.) As in any R&D there are no guarantees, but basically we have a cost/output curve of which only the extreme ends have been tested. This turbine explores the whole area of the curve in between. It is at least possible that this will discover a “sweet spot” – a compromise with a lower cost per kWh hour than either extreme. Any R&D funding trying to lead to a renewable future should seriously consider this project.

Of course we should not just turn to small scale. Utility scale wind is already competitive with fossil fuels, and the potential, (though not as great as small wind) is more than we need. There is no reason to think we have come to end of the potential for lower large scale wind costs, and we need continuing research in this area as well. One possibility is gyromills (essentially tethered helicopters with wind generators attached) A gyromill can reach much higher than a tower, accessing high altitude rather than surface winds. Significant wind power can be tapped almost anywhere through this means. Unlike surface mills, gyromills can approach 80% of nameplate capacity. The power required to keep the mill in the air is a tiny portion of that generated. This has been demonstrated on a small scale. One major problem, stability, has been solved recently by adding more rotors – since unlike a helicopter, a gyromill does not actually need to travel. There is even one company who thinks gyromills are ready for commercial deployment, and could produce wind electricity at an unsubsidized cost of less two cents per kWh³¹⁸. Though entrepreneurs are optimists by nature (at least in public), it sounds like financing a pilot project would be a worthwhile use of public funds, followed by encouraging deployment if the cost and output projections proved anywhere near accurate.

Although wind power is environmentally benign compared to any other means of generating electricity, this does not mean it can't be improved. We do need to continue to study bat, bird and insect protection.

Related to wind is wave power. It is not yet commercially priced, and the near term potential quantity is not nearly equal to that of wind or solar. But it has one advantage that makes it very promising indeed. Wave power is extremely reliable – not up to that of hydropower or geothermal, but very much greater than that of wind or solar without storage. Inexpensive wave power added to a renewable mix would significantly reduce the amount of expensive stored electricity needed. So bringing its price down is important even if it could supply only a small portion of total electricity needs.

We need a great deal more research in biomass as well. This includes: less expensive enzymes for less expensive production of cellulosistic ethanol, along with better and less expensive enzymes in general for biomass production, better gasification processes, better bio-refineries to co-produce chemical, pharmaceutical, food byproducts and energy production, integrated biomass production and processing facilities. We also need more research on more sustainable ways to generate biomass – low impact and especially low water; we need lower impact ways to process biomass as well.

Related to this, we need to find out if the high energy, resource, and land efficiency of biointensive agriculture can be duplicated or even approached without the high labor costs. Because of my background, my instinct is to look at robotics, cybernetic, computerization and data processing technology. But, as with no-till agriculture, the breakthroughs there are just as likely to occur via simple common sense questioning of assumptions. Approaching the problem from the other end, hydroponics is already an extremely water and labor efficient technology; we need research to lower its high capital and energy costs.

There is a related study that would be worth doing – a cultivatable and recoverable land survey. In many poor nations cultivatable land is simply held out of cultivation, to drive up the future price for purposes of real estate speculation. (In the rich nations this is not as common – the land already is valuable and there are usually short term ways to exploit land for which you have other long term plans.) Secondly, while a great deal of lost farmland is truly lost, some of it may have been eroded or damaged in other ways without having been permanently converted to other uses. There are well know ways to rebuild the soil on eroded land; sometimes even poisoned land can be recovered – and land that is too toxic to grow food on might still be suitable for energy biomass. Lastly a great deal of land in nations with hungry people is used to grown coffee and flowers and other non-food luxury crops – though in absolute terms the FAO figures do not seem to suggest they account for a large percent of world crop acreage³¹⁹.

We also need more investigation of kelp and algae and other ocean and water based biomass. Right now it is way too expensive to be a source for anything but chemicals, pharmaceuticals and luxury foods – products that sell at high prices per kilogram. But if the price could be lowered the sustainable potential is much greater than that for land based biomass. And to the extent that such material is grown in polluted water rather than fertilized it actually helps reduce eutrophication and clean the oceans of some of the damage we have done to them. Deliberate fertilization, as sometimes proposed, is probably not sound.

We need more research in high temperature solar applications for process heat. Dish concentrators can produce heat above 750 Fahrenheit degrees, but very expensively. If it could be done cheaply, that is another large portion of industrial needs that could be met by high temperature solar. Related to this, inexpensive long term storage of high temperature heat is an area that needs more funding.

While we already know how to make huge efficiency increases, the potential for improvements there is high also. We need more research on enzymes and catalysts that allow the huge number of chemicals involved in industrial production to take place at lower temperatures and for shorter times.

A great many production facilities include a few steps out of many that require energy or material intensive processes - ultra clean environments, highly toxic solvents, very high temperatures. More development of mini-reactors which isolate these steps from others in small tightly controlled environment could save huge amounts of energy, water and materials - especially in the chemical and electronics industries.

In these same industries (and many others) super-critical carbon dioxide might provide huge savings as well. It can substitute for toxic solvents in a great many contexts, and is much easier to keep ultra-pure than water – whose ultra-purification requires substantial energy.

Currently high strength carbon fibers are very expensive to produce in both money and environmental damage. Lowering both costs could allow them to widely substitute for steel and other metals – providing a huge energy and environmental savings.

We need to continue research into high temperature superconductors (of course) to lower transmission costs and risk for electricity. They are currently commercially competitive in certain limited instances; if their costs could be lowered and efficiency improved they have a great deal of potential.

Similarly we need more research into electroactive polymers, which might allow us to literally print lightbulbs, computer monitors and other electronics where nano-second response times are not required.

All of the above are comparatively short term. But we have seen in the past that “blue sky” research based on a large scale vision often pays off. So let’s include a couple of “big picture” long term projects just to show that they fit in as well.

One field we are already putting a great deal into is nanotech - and we should. We have every reason to expect this field to produce low cost solar cells, low cost environmentally sound electronics, low cost electrolyzers for hydrogen and possibly low cost fuel cells. However there is one aspect of nanotech we are not putting enough effort into – how dangerous the waste is and what needs to be done about it. The current and near future versions of nanotech, unlike portrayals in some of my favorite science fiction, mainly focuses not on bacteria and virus sized machines, but on how materials act when processed into very tiny fibers, crystals, tubes and other nanostructures. Carbon, silicon, glass and metals (just to name a few examples) behave in very different and often useful ways when formed into such structures, compared to their normal forms.

Now any manufacturing process produces some waste; nanotechnology usually results in scrap nanomaterial. We are talking about scrap carbon, glass, metal or silicon – all pretty harmless sounding stuff. And maybe it is just as harmless as it sounds. But just as common materials behave differently in useful ways, when formed into nanostructures, they may behave differently in harmful ways as well. The point is, we don’t know. Nanotech has so much potential; we do not want this infant industry to make the same mistake other high tech industries have, and smugly assume their waste products are harmless or that the problem of waste disposal will be solved without effort on their part. Test your material now; find out how harmless it really is; if it is not harmless find out how to turn into something that is. Apply the old business cliché and be proactive.

Is this a real concern? Well bear in mind we see many harmless materials turn dangerous even at the macro level when formed into ordinary small fibers. Cellulose and glass are among the least toxic non-food substances known. But when turned into cellulose fiber for insulation, and fiberglass for many purposes, you do not want to breathe them. Cellulose and fiberglass insulation are always sealed off from building air when properly installed. Workers who install the stuff wear masks and protect themselves in other ways, or they do if their employer cares about their health, or if they have the leverage to force their employer to care. Note that we have not stopped using either material. As a society we investigated what the dangers are, and know how to take appropriate precautions.

That I'm sure is all we need to do with nanotech - find out what the problems are. In some cases there probably won't be any. In others only simple fixes will be required. Some substances may require elaborate precautions or complex post processing. And isn't it better to know that in time not to kill a lot of people and ruin the reputation of an industry that could save us all? Spend the effort now to find out what problems it might create and how to solve them.

Another place I think we need to put some research money is into the space program. One possibility that requires trivial funding compared to the benefits is the space elevator or beanstalk. An elevator to space could get us there and back again a lot less expensively than space shuttles - if it worked. Admittedly that is a big if. There are many problems to be solved - developing the nanotubes to make that large a structure self-supporting, protecting against micro-meteors and space debris, protecting against differential winds, and corrosion from exposure to various levels of the atmosphere, find ways to ensure ultra-long fibers can be free of flaws. But some very serious people think it could be done.

And cheap access to space could provide a lot of benefits even with our 30 year time frame - solar energy that is reliable and predictable, cheap vacuum, a place to produce exotic materials without a biological environment to disrupt. What if it fails and you can't build a beanstalk? Well even then the research needed to make a serious attempt would probably give you nanotubes and a number of useful results. The potential spin-offs, even from failure, and the spectacular rewards of success might make this a project with a very good potential risk/reward ratio.

But I also think it is important to our growth as a species; it will help to make us better people. When I was very small and humankind made it into space for the first time, my late father wrote some verses on the subject. I still think it expresses something very important:

I can hear the planets ring!

I find joy in everything!

And there's no more room in living for tears!

I am glad for every breath!

Life was never meant for death!

And I want to live a million more years! I want to live a million more years!

*The whole human race is my family.
I have brothers and sisters wherever I roam.
This little green globe is just too small for me!
So I count the great wide universe as my home!*

We have lost that joy somewhere along the way, replaced it with an ecstasy of hate. I think it is time the human race make another attempt to become space people.

Now again, none of the above is a research agenda. It is simply some examples worth considering. A real research agenda would be laid out by experts in various fields to maximize results. But the principles, I think, are the ones a real research agenda would follow. Invest intelligently in a variety of promising approaches, and you will get good enough results from the successes to outweigh by many times what is lost to the failures. (It is rather like oil drilling, where the successful finds make up for the dry holes and the productive but uneconomic wells.) Mix short term practical projects with visionary blue sky possibilities – sometimes the low hanging fruit is as sweet as it looks; sometimes the wild and crazy big ideas produce the best results.

End Notes

³¹⁵KPMG Bureau voor Economische Argumentatie; Steins Bisschop Meijburg & Co Advocaten, *Solar Energy: From Perennial Promise to Competitive Alternative - Final Report*, Project Number: 2562. Aug 1999. Greenpeace - Netherlands, 24/Sep/2004 <<http://archive.greenpeace.org/~climate/renewables/reports/kpmg8.pdf>>.

³¹⁶ European Commission, *EUROPA - Research - Energy - R&D Topics*. Jul 2005, European Commission, 28/Sep/2005 <http://europa.eu.int/comm/research/energy/nn/nn_rt/nn_rt_pv/article_1108_en.htm>.

³¹⁷Doug Selsam, *The Selsam SUPERTURBINE*. Sep 2005, Superturbine Inc., 28/Sep/2005 <<http://www.speakerfactory.net/wind.htm>>.

California Energy Commission, *Notice of Awards EISG Program, Solicitation 02-01*. 19/Feb 2003, California Energy Commission, 18/Sep/2004 <http://www.energy.ca.gov/contracts/smallgrant/2003-02-21_awards_02-02.html>.

³¹⁸ Sky WindPower Corporation, *Sky WindPower Corporation*. 6/June 2005, Sky WindPower Corporation, 123/Feb/2006 <<http://www.skywindpower.com/ww/index.htm>>.

³¹⁹(For example wheat accounted for 20 times the acreage harvested compared to coffee worldwide in 2004)

United Nations Food and Agriculture Organization of the United Nations Statistical Service, *FAOSTAT Database Results*. 2004, United Nations Food and Agriculture Organization of the United Nations Statistical Service, 13/Jul/2005 <<http://faostat.fao.org/faostat/servlet/XteServlet3?Areas=862&Items=656&Items=15&Elements=31&Years=2004&Format=Table&Xaxis=Years&Yaxis=Countries&Aggregate=&Calculate=&Domain=SUA&ItemTypes=Production.Crops.Primary&language=EN>>.