Chapter 6

SPS IN CONTEXT

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SPS IN CONTEXT

ENERGY

Introduction

Because of its long development leadtime, solar power satellites (SPS) will not be available to any extent before the early part of the next century and will therefore do very little to relieve our dependence on imported oil. SPS's primary use would be to replace old powerplants and meet any new demand for electricity. Consequently, the potential value of the SPS must be determined in competition with other future electricity sources and in the context of U.S. and global electricity demand. This chapter examines this topic in detail by looking at the future demand for energy, and electric power in particular, in the United States, and the various supply options that could compete with the SPS. Global energy demand and the SPS in a worldwide context is examined in chapter 7.

Overview

The U.S. energy future can be divided into three time periods according to the supply options that will be available. These periods are roughly the next 10 years (near term), from 1990 to approximately 2020 (the midterm or transition period), and beyond 2020 (the long term). Although these boundaries are not hard and fast, they roughly define periods in which particular energy supply forms will dominate.

Near Term

In the near term, there will be no significant change from our current reliance on oil, natural gas, and coal. Currently about 92 percent of our Nation's energy supply comes from these fuels. About one-quarter of the total is imported (almost all in the form of oil). Because of finite supplies, overall consumption of these liquid and gaseous fossil fuels must eventually be reduced. However, the most important goal over the next decade is the reduction of oil imports in order to avoid the severe economic problems that would result from potential supply interruptions and to improve the U.S. trade deficit. To do this, concentration must be placed on lowering demand growth by increasing the efficiency of energy use, and switching to the use of more abundant domestic fuels. Of the two, improving energy efficiency will be the major new source of energy because of the much longer leadtime needed to bring on new fuel supplies such as coal and nuclear. Domestic oil and natural gas can be developed more quickly, but it is not likely that they will contribute to reducing oil imports since both will probably decline in production for the decade. A recent OTA technical memorandum[1] estimates a 25-to 45-percent drop in U.S. oil production by 1990. Thte use of nuclear energy will increase, but at a slower rate than in the 1970's. Finally, solar and biomass energy production will grow rapidly during the 1980's but the absolute magnitude will be low compared to oil imports. Therefore, although an increase in the amount of coal, solar, biomass, and possibly nuclear energy sources is expected, they will probably not be able to contribute enough by themselves to relieve the pressures caused by U.S. dependence on imports.

Transition Period: Midterm

In the period from 1990 to 2020, substantial supply shifts will occur. Although the period will begin with heavy dependence on coal, oil, and natural gas, it will end with a much greater reliance on renewable and inexhaustible energy resources. U.S. dependence on imported oil will almost surely come to an end if for no other reason than that the availability of oil on the world market will have dropped substantially. World oil production may drop as much as 20 percent by 2000 and fall off sharply thereafter. The dominant fuels during this

[1] Office of Technology Assessment, U.S. Congress, "World Petroleum Availability, 1980-2000," technical memorandum, October 1980, OTA-T M-E-5

period are likely to be coal (for synthetic fuels, direct combustion, and electricity generation), natural gas, and possibly conventional nuclear. During this period, strong growth of renewable and inexhaustible sources such as solar and biomass can be expected. Uranium is a small enough resource that conventional nuclear must be considered a transition energy source. However, the supply of coal appears to be substantial enough to play a major role well into the 22d century. Whether these fuels contribute significantly beyond the midterm depends on the successful resolution of their short- and long-term environmental and safety questions.

It is also during this period that SPS and other long-term candidates such as breeder reactors and perhaps fusion may begin to reach commercial status. The transition period will be the time when a number of long-term technologies will compete with one another for a role in the future on the basis of economics and public acceptance. This competition will also depend heavily on the relative economic efficiency of different ways of using energy, as will be discussed below.

Long Term

In the long term, the United States and the world will be almost totally fueled by inexhaustible energy sources. Although rapid growth of sources such as the SPS during the first decades of the century may be seen, it will not be until the middle of the next century that they could become as commonplace as coal, electric, or even nuclear plants are today.

It is not clear which renewable and inexhaustible sources will dominate. It may be that small-scale, onsite solar systems coupled with an extremely energy-efficient economy will be the ultimate future. It may also be that a mix of technologies such as onsite solar, biomass, fusion and/or SPS will be used. However, the choice will be made in the transition period and will be based primarily on the projected costs of competing supply systems and demand technologies.

Determinants of Demand

SPS would fit most easily into a high electric growth future. Such a future is contrary to recent low growth trends. In fact, many conservation initiatives have been directed at reducing the use of electricity because of the high energy losses at powerplants. Nevertheless, changes in relative fuel prices and gains in the efficiency of electric generation and use could dramatically change the picture.

The energy technology choices the United States and the world will make in moving through the three periods described above will be primarily dictated, as always, by relative costs. Until recently the dominant factor determining the development of energy technologies has been the type of resource and its availability. The abundance of oil and natural gas, and the ease with which it could be transported and burned, dictated the development of most of the energy-using equipment currently in existence. Some of this equipment could have been powered more efficiently by electricity, but this advantage was often dwarfed by the cost advantage these fuels had over electricity. However, many applications such as electric motors can be made significantly more efficient, reducing the fixed cost penalty.

In the past few years the relative prices of these energy forms have changed because of the rapid increase in oil and natural gas prices. Current average electricity prices are about twice that of oil and four times that of natural gas. In 1960, the ratio of electricity to oil and natural gas prices was 7 to 1. Even though the costs of new powerplants are rising rapidly, those of electricity will probably rise more slowly than oil and natural gas, primarily because of the relative abundance of coal and uranium. It is even possible that synthetic fuels from coal and biomass may be more expensive than electricity from coal, particularly as newer, more efficient coal combustion technologies are introduced.

The total cost to the energy user also includes the cost of the energy consuming equipment. Electric powered equipment is often cheaper than gas or oil fired counter parts. This advantage will become increasingly important as the prices of oil and gas narrow the gap with the price of electricity.

The implication of these effects is that electricity may become the cheapest energy form, when both supply and demand are considered, for many applications that could use a multiplicity of energy forms. The reason is that the price differential between electricity and the other energy forms (liquid and gaseous fuels, direct solar, etc.) will likely be small enough that it could be overcome by cheaper and more efficient electric end-use technologies. Some of these, such as heat pumps for space and water heating, are already in use, while others, such as inexpensive electrochemical processes and long-life storage batteries, require further development, [f such development is successful and electricity does become the cheapest energy form for most uses, then electric demand growth could become quite rapid even though total energy demand may grow very slowly or not at all.

If this holds, solar power satellites will have an easier market to penetrate than if the electric utilities continue their recent slow growth. Thus, the fate of SPS rests as much on the ability to create energy efficient electrical end-use technologies as it does on the relative economics of other electric generating technologies. One caveat must be added, however. If demand technologies for fuels keep pace with the efficiency improvements of electric demand technologies, such dramatic switching may not occur.

Electric Demand Technologies

To see if such a future is technically possible a closer look is taken at current and potential uses of electricity. Because of electricity's unique properties it has been used for specialized tasks such as lighting because of the high temperature needed to excite the visible spectrum. Here, electrical energy is converted to visible electromagnetic radiation as well as to heat. Nearly 60 percent of all electricity is used to perform mechanical work through the use of motors. Electricity is also used for industrial electrochemical processes such as in aluminum and steel production, for specialized induction-heating applications and for microwave and infrared furnaces. A small but crucial amount is used to power the Nation's electronic systems. Finally, electricity is used in the crudest form possible, namely for direct conversion to heat.

Although these uses are more varied than for the other major fuels, they account for less than 12 percent of the total end-use energy demand in this country. The other 88 plus percent is direct combustion to provide direct heat, steam and mechanical drive. As indicated, for electricity to penetrate this latter market it will be necessary to make technical advances to give electricity a cost advantage at the end-use that can compensate for its higher cost at the production point.

To do this requires making use of the special character of electricity as an energy form. Electricity is a high-quality fuel (thermodynamically work that is heat at infinite temperature). Therefore, it can be used for any kind of mechanical work or it can be converted to heat at any temperature. The best known example of the latter property is the heat pump for space heating. This is now being applied to water heating and certain drying applications with a substantial reduction in energy use over electric resistance heating and apparent cost advantages over solar.

In the industrial area, there is considerable potential for increased use of electricity. For instance, in steel making it can be used for the plasma-arc process and direct-electrolytic reduction of iron. Although these processes have been arourd for several years, technical development is still needed. In a nearer term application, the direct reheating of steel by high, pulsed electric currents could result in a significant reduction in fuel use compared to direct-fired processes, and also reduce material loss by eliminating oxide formation that occurs with direct firing. In other areas advances have been seen recently in the efficiency of electric motors that are now competitive with steam drives in many applications such as mechanical presses for metal forging. A more speculative but very interesting area is the use of laser or microwave radiation to drive industrial chemical reactions, instead of heat.

In ground transportation the principal problem is the development of long-lived, lightweight, reliable storage batteries. Electric drive using motors with precise solid-state speed control can be made very efficient, as has been demonstrated on many of the world's railroads. Advances have recently been made in battery technology but the general feeling is that "ideal" batteries are at least a decade away.

The industrial sector is presently only 13-percent electrified, while the transportation sector only uses a negligible amount of electricity. Thus, these are the markets that electricity must penetrate to become the dominant energy form. However, some new technologies have the potential to reduce industrial demands without creating new markets for electricity. In the chemical industry, for instance, biogenetic methods of feedstock synthesis could replace thermochemical methods, reducing fuel usage without substituting electricity. About half the present industrial electric demand could be offset by cogeneration, a technology that is not strictly a demand technology but which could nevertheless reduce electricity needed from the grid. In the transportation sector, battery research as a key to electric vehicles must compete with the efficiency improvements possible with high-mileage advanced vehicles using synthetic or biomass-derived liquid fuels. The buildings sector is already the most heavily electrified and some electric technologies, such as common appliances, are nearing saturation.

The achievement of highly efficient, electric demand technologies would change not only the balance of fuels now used but also the sectoral usage patterns of electricity, with dramatic growth in the industrial and transportation sectors, and less in the buildings sector which has shown the greatest postwar growth in electric demand.

Conclusion

It is likely that as technologies using electricity are improved or new efficient uses are found, improvements will be made in using other future nonelectric energy sources such as biomass and direct solar. While all of these developments are many years away, it is this environment in which the SPS will compete. The success or failure of these new electric technologies will have a great deal to do with determining whether or not a market exists for SPS as well as the other large-scale, electric-generating technologies.

Energy Supply Comparisons

Introduction

Comparisons with other energy technologies, both current and future, are a critical part of assessing a proposed new energy technology. A host of criteria, only some of which are readily quantifiable, is available for comparison purposes. Costs, environmental impacts, scale, complexity, versatility, safety, and health risks are some of the more important factors of choice that ultimately determine the relative desirability of a given energy technology. For technologies currently in place these factors are generally well known. For future technologies they are more often only poorly known. Nevertheless, choices among future energy technologies must be made, either in the R&D phase, or, later, in the marketplace.

Criteria for Choice

Whenever decisions to proceed with or halt the development of a given technology are made, it is important to lay out the framework of choice, to develop a set of criteria by which one may judge the relative benefits and drawbacks of different technologies. In addition to providing a basis for choice, such a list can also help to identify the essential distinctions between technologies and highlight areas that will need further R&D.

Table 14 lists 32 criteria developed in an OTA workshop that are often used in compar-

Table 14.—Criteria for Choice

Plant ***description***
1. **Scale** of power output (range in megawatts)
2. Power output in relation to load profile (baseload, intermediate, peaking)
3. Versatility (other output besides electricity)
4. Complexity (high, medium, low) and maintenance requirements (controllability)
5. Reliability (percent of time available to the grid)
6. Nominal capacity factor (percent time operating)
7. Material requirements
8. Labor requirements
9. Land requirements
10. Construction leadtime (years)
11. Lifetime (what are key determinants)

costs
12. Opportunity costs of RD&D (dollars and people)
13. Net energy ratio
14. Operating costs (cents/kWh)
15. Capital costs ($/kW)
16. T&D costs (cents/kWh)
17. "Decommissioning" costs

Impacts
18. Institutional (organization and ownership) impacts
19. Safety and health risks (magnitude and distribution)
20. Environmental risks (magnitude and distribution)
21. National security risks of normal or unintended use
22. Military vulnerability

Deployment consideration
23. Time period to commercialization
24. Geographic location; location of plant with respect to load centers
25. Compatibility with other technologies and utility grid

Other
26. Probability for success (high, low, medium)
27. Initial demonstration requirements (large or small)
28. Resource constraints (domestic, international)
29. Risks/impacts of RD&D failure (chance it may become prematurely obsolete)
30. Relative uncertainties to be resolved by RD&D (e.g., sensitivity of efficiency to design parameters)
31. Is it a viable example for rest of world?
32. Nature of R&D process (public, private, classified)

SOURCE: Office of Technology Assessment

ing electrical generating technologies. Most fit into four broad categories: plant description, costs, impacts, and deployment considerations. These criteria establish a context for evaluating the SPS in relation to other future energy technologies.

Five Future Energy Technologies

In the timeframe that the SPS would be most likely to play a role in the U.S. energy future, the other energy sources that are likely to contribute will be predominantly the renewable and inexhaustible ones. OTA has chosen to study the SPS in comparison to terrestrial solar thermal technologies, terrestrial solar photovoltaics, advanced fission (the breeder), and fusion. If the health and safety problems of coal are satisfactorily solved, it could also be a major electric supply technology in the period that SPS could become available. In addition, there may also be a component of conventional nuclear power still operating in the second and third decades of the 21st century (the timeframe after 2010 that is most likely for SPS deployment).

The data that OTA generated for these technologies are supplemented by the electrical supply comparisons which Argonne National Laboratory made for the Department of Energy (DOE/SPS) assessment program.[3] DOE chose to study conventional and advanced coal technologies, light water reactors, liquid metal fast breeder reactor (LMFBR) breeders fusion, the reference system SPS, and terrestrial photovoltaics operating in a peaking mode. Their data will be discussed along with the results that OTA obtained. Coal and conventional nuclear power will be presented first to provide a reference for the future energy technologies in the discussions that follow.

THE COAL BENCHMARK

The coal resources of this country are almost incomprehensibly large. Even if production were to triple, in that case coal would serve about half the present U.S. energy needs, known recoverable reserves would not be exhausted until late in the next century. Estimated additional reserves could take this production well into the 22d century. Thus, for all practical purposes, the supply of coal is inexhaustible.

Unlike any other long-term energy source, coal can be exploited with known, proven technology at costs that are competitive now. Advanced coal technologies such as combined-cycle gasifiers and magnetohydrodynamics, are not vital to coal's future but could

[2]M E Samsa, "SPS and Alternative Technologies Cost and Performance Evaluations," *The Final Proceedings of the Solar Power Satellite Program Review*, CON F-800491 (DOE), 1980.
[3]*Program Assessment Report* Statement of *Findings*, SPS Concept and Evaluation Program, DO F/E R-0085, 1980

improve the efficiency and economics of coal-fired electric power. Thus, of all the options for large-scale, long-term production of electricity, coal is the least uncertain technologically and economically and it is appropriate to view it as a benchmark for evaluating the others, including SPS.

Technological and economic criteria are not the only alternatives to consider. Any energy source must have generally acceptable health and environmental impacts. Coal evokes depressing memories of scarred landscapes, suffering miners and smokey skies. Today, this reputation is no longer deserved. Modern coal mining and combustion techniques, when properly applied, have reduced virtually all these objectionable impacts to the point where damage is clearly a small fraction of what it once was.

The actual future of coal, however, is much less certain than its potential. Issues arising when expanded mining and use are considered can be divided into three categories: interruptions, control costs, and risks. These will be discussed in some detail because if coal does not realize its potential, the reasons will probably be found here.

Interruptions are intermittent events that prevent scheduled plans from being fulfilled. Strikes by miners and transportation breakdowns are obvious examples. Opposition by intervenors that prevent facilities from being built might be included here. These factors can't be completely eliminated, but proper planning can reduce disruption. The major long-term effect is to deter potential users from turning to coal if they have other options and are concerned about the reliability of the coal supply.

The cost of controlling coal's negative impacts is high. Reclaiming surface mined lands and reducing the emissions of combustion have received the most attention. For instance, the Clean Air Act Amendments of 1977 have required the use of the "best available control technology" for limiting emissions of sulfur oxides. Utilities have been concerned not only because of the expense of the flue-gas scrubbers, but also because the equipment in use has generally shown disappointing reliability. However, current systems appear to be considerably better than early designs, so utilities can, if they are careful, be confident that their equipment will function reliably and effectively.

The regulatory approach has been to ensure that the impacts are controlled to the point where it is clear that known damages are sharply reduced. As mentioned above, it appears that this goal has been achieved. As more information is gained, it is possible that control can be loosened without increasing the risk. For instance, new data on the damage caused by sulfur oxides and sulfates, and better data on the long range transport and chemical transformation of these and other pollutants might allow more selectivity in emissions control. Thus, the costs of controlling impacts may be reduced rather than increased in the future. Such a reduction would improve coal's competitiveness with nuclear power or SPS, unless some of the unproven risks are confirmed.

There are three major *risks* to long-term coal combustion that could limit expansion or make it much more expensive: public health effects, acid rain, and carbon dioxide ($C0_2$). Coal combustion pollutants have been linked by statistical analyses to tens of thousands of deaths per year. These studies are highly controversial and have been neither proven nor disproven. If they are generally accepted, considerable reduction of sulfur and nitrogen oxides would probably be necessary. This reduction would probably call for greater use of coal cleaning before combustion, combustion modifications and higher efficiency flue-gas desulfurization systems. Such changes would be expensive but unavoidable if the public demands cleaner air because of concerns over health risks.

The documentation for damage by acid rain is better than for public health effects, but is still not conclusive. Acid rain is evidently caused by the same pollutants suspected in the public health issue, but the scientific understanding of pollutant transport and chemical conversion is poor. Furthermore, while acidification of certain lakes and streams is strongly suspected, extensive damage to terrestrial ecosystems is only surmised. If this damage is proved and found too costly, the remedy would be the same as for public health effects. However, it must be emphasized that proof of damage is insufficient. The pollutants must be traced back to their source in order to know where to implement controls. Otherwise ineffectual or overly expensive control strategies may be implemented.

The final risk, excessive CO_2 released to the atmosphere, is by far the most intractable. The adverse impacts that have been suggested dwarf those of any other human activities with the possible exception of nuclear war, The $C0_2$ produced by burning fossil fuels and clearing forests accumulates in the atmosphere. Some of the CO_2 that is produced is absorbed in the oceans, but the dynamics of the CO_2 balance are not well-understood. The concentration in the atmosphere is increasing by 5 percent per year since 1958. $C0_2$ is transparent to most of the incoming sunlight that warms the Earth. Normally much of this is radiated back to space in the form of infrared radiation, but CO, tends to absorb and block this longer wavelength radiation. This mechanism, the greenhouse effect, is an essential ingredient in maintaining the proper temperature balance on the Earth. However, if sufficient quantities of CO_2 are added to the atmosphere, additional heat will be trapped to warm the Earth significantly.

A number of studies of atmospheric CO_2 levels predict that concentration will rise to two to eight times today's level in the 21st and 22d centuries. While there is continuing discussion about the effects of this buildup, the majority of the scientific community agrees that the probability of global warming and other climate changes is sufficiently high to warrant exceptional attention.[4] Changing climate patterns, even if they turned out to be ultimately beneficial, would cause enormous disruption, especially with agriculture. At least 10 years will be required before enough is known to make intelligent decisions about the significance of the effects of increased CO_2 in the atmosphere. The contribution of fossil fuel combustion to the CO_2 buildup, the results of this buildup on the heat balance and climate, and the effects of climate changes must all be studied extensively. At some point, however, it may be necessary to limit coal combustion in order to limit CO_2 emissions since it is highly unlikely that any practical means of removing CO_2 from the flue gases will be devised.

In summary, as far as we can tell now, coal is capable of supplying most of the electric power this country is likely to need for many generations. The effects of the release of extra CO, to the atmosphere are sufficiently in doubt that other options must be prepared in case they are required. However, until we know that it constitutes a serious problem the development of other options must be justified on the basis that they will be cheaper or more attractive in some other way than properly control led coal.

CONVENTIONAL NUCLEAR

Conventional nuclear plants totaling 55,000 MW of power are now operational and another 106 reactors totaling 118,000 MW are either on order or under construction.[5] This is a substantial base for the nuclear technology, but it is questionable whether it will be fully realized or expanded because of public opposition, licensing problems, financial uncertainties, and eventually resource limitations.

Public opposition has been especially visible. While public opinion polls still show support for nuclear energy, this support has been weakened for several reasons. Low-level radiation release and other problems with routine operations contribute to public concern. Public support has also eroded because of continued lack of a suitable site and demonstrated means for nuclear waste disposal. Further mishaps such as the accident at Three Mile Island could condemn the technology in the eyes of many who now reluctantly accept it. Finally,

[4] 0ffice of Technology Assessment, U S Congress, *The Direct Use of* Coal, OTA-E-86, 1979

[5] Department of Energy, "U S Central Stations Nuclear Generating Units, " September 1980

the possibility that nuclear energy could contribute to nuclear weapons proliferation disturbs many, though it is debatable whether renunciation of the nuclear option by the United States would materially reduce this risk.

Most of these problems, except proliferation, can be ameliorated by improved technology, procedures, and regulations. But if improvements are not made quickly, public opinion could swing against nuclear power in the United States as it has on occasion in other Western democracies (e.g., Sweden and Austria). Even if opponents remain in a minority, they can find many opportunities to trouble the industry through legal actions, regulatory appeals and ballot initiative. None of these may kill a particular project, but they could discourage utility executives from choosing the uncertainty and frustration associated with nuclear power as long as they have other options such as coal.

Utility decisionmakers also have to consider licensing and financial uncertainties. At present, many design criteria for nuclear plants are so poorly defined that it is virtually impossible to get a new reactor licensed. [6] This problem may be resolved over the next few years, but recent trends have not been reassuring. For instance, a review now underway—to determine if fundamental changes in reactor designs are necessary to contain melted fuel cores in case of severe accidents— is expected to last several years.

Some regulatory rulemaking problems stem from a lack of conclusive data. Others appear to reflect the Nuclear Regulatory Commission's lack of a clear picture of what it wants to accomplish and how to do it. Both types of uncertainties have to be resolved before the utilities will consider ordering many more reactors.

Utility companies also face uncertainty concerning both the capital available to build plants and the risk of a long-term shutdown. The cost of a new nuclear plant is now close to $2 billion. Not many utilities can raise that much capital, even when the projected costs of power at the busbar are favorable. Even now, many plants are being built as joint ventures by several companies. A continuation of high interest rates could delay many plans for capital-intensive projects. And after an expensive reactor starts operation, the utility bears an additional economic risk due to the possibility of unplanned shutdowns. The Three Mile Island (TMI) accident and the Browns Ferry fire led to lengthy shutdowns that forced huge expenditures by the owner utilities, which then had to generate or buy expensive replacement power. The present financial difficulties of the owner of Three Mile island, General Public Utilities, illustrate how critical this concern will be for other utilities.

Availability of fuel will eventually be a serious constraint if conventional reactors are used in the midterm to long-term future, without a shift to advanced nuclear breeders. The Committee on Nuclear and Alternative Energy Systems (CONAES) estimated that enough uranium exists in this country to fuel at least 400,000 MW for the lifetime of the reactors (40 years). [7] This would allow the construction of another 227,000 MW of capacity. If ordering of new reactors resumes in 1985 and continues at the rate of 10 reactors per year, the last one would be ordered in 2008. Because of retirements, by 2050 nuclear power would be back to near its present level. Peak energy output under this scenario would be about 5.6 (end use) Quads in 2015. However, discovery rates for uranium ore and imports and exports of uranium could change the total availability in an unpredictable way.

The greatest single long-term uncertainty facing the industry is the future electricity growth rate, just as it is for the SPS. Over the next several decades, moderately high growth rates might require much more nuclear power, but as discussed in this chapter, the growth rate may be more modest. However, low growth need not preclude nuclear, and might

[6] Office of Technology Assessment, U S Congress, *Nuclear Powerplant Standardization*, OTA-E-1 34, April 1981

[7] *Energy in Transition*, Committee on Nuclear and Alternative Energy Systems (CONAES), National Academy of Sciences, Washington, D C , 1979

enhance the attractiveness of nuclear compared to other future central power options, such as SPS, that require large deployments to justify the development cost.

Nuclear energy can have a future if its problems are addressed effectively and decisively. To some extent this is happening. The accident at TM I has revealed weaknesses in reactor plant design and operator training, to which the industry and the NRC are responding with initiatives such as the Institute for Nuclear Power Operations and the Nuclear Safety Analysis Center. As a result of the events in the past 2 years, both regulators and utilities seem more conscious that extreme safety is in everyone's interest.

Whether these measures will ensure safety in the future and enhance the industry's public image without pricing the technology out of reach is still an open question.

FIVE FUTURE TECHNOLOGIES

The following discussion summarizes the salient characteristics of the four central renewable or inexhaustible energy technologies that have been chosen for comparison with the SPS. While each of these alternatives is compatible with centralized electricity production in a utility application, they are not equally applicable for baseload power production. Photovoltaics and solar thermal sources vary over the course of a day and the season in a fashion that makes them well-suited for peaking applications. Fusion, the breeder and SPS would work most efficiently producing constant power 24 hours per day, so they are naturally suited for baseload power production. The applicability of photovoltaics and solar thermal can be broadened to cover intermediate and possibly baseload applications by the addition of storage capability, but over the next 10 to 20 years there may be little cause to do so, for two reasons. The first is that the most cost-effective application of solar thermal and photovoltaic systems is likely to be as fuel savers until all the oil and gas-fired generating facilities have been retired from utility systems. Second, electric storage is far more versatile and cost effective for a utility if it is not restricted for use with a single plant. A recent study by the National Academy of Sciences' concludes that when wind, photovoltaics, or solar thermal is used in a utility system, "it is typically not desirable to have dedicated storage but wiser to provide the backup energy from the grid." Except for a small amount of storage to handle short-term variations of sunlight in solar thermal applications, the conclusion that dedicated storage is not appropriate for terrestrial renewable electric technologies is generally well-accepted.

Currently, electrical generation is fueled largely by oil, natural gas, coal, fissionable material, and stored water. For the time period when the SPS is most likely to find applicability, there may not be as great a diversity of energy supply technologies connected with the utility grid as is now enjoyed; hence terrestrial solar technologies may be used in a different mode than the one that seems most desirable now (i. e., peaking or intermediate). It is also desirable to compare all the future electric technologies on a common basis. For this reason, OTA has prepared cost estimates for solar thermal and photovoltaics operating in a baseload mode. Because photovoltaics also possess the unique property among these future energy systems of being modular on a very small scale, its use in a dispersed mode—both connected to the electric grid and independent of it– will be discussed in a separate section. In the future, it would be also worthwhile to compare SPS to an energy scenario composed of a number of dispersed solar technologies working in complementary fashion.

The following discussion will give the major characteristics, cost sensitivities and uncertainties, factors affecting deployment, and foreseeable impacts of the different renewable and inexhaustible energy sources. First, a short summary of each technology will be given, followed by comparisons. Table 15 presents the relevant characteristics of each of the 5 technologies in matrix form.

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*"Energy Storage for Solar Applications," Committee on Advanced Energy Storage Systems, National Academy of Sciences, 1981

Table 15.—Characteristics of Five Electrical Technologies

Criteria	Fusion	Breeder	SPS	Solar thermal	Photovoltaics
Plant description					
Scale of power output	500-1,500 MW	500-1,500 MW	1-100 GW (lasers smaller)	10 kW to greater than 100 MW	10 kW-100 MW
Power output in relation to load profile	Baseload	Baseload	Base load	Peaking, intermediate, baseload (with storage, but expensive at high-capacity factor)	Peaking, intermediate, baseload (with storage expensive)
Versatility	Also large-scale, high-temperature process heat; synfuels, production of fissile materials	Also large-scale, low-temperature process heat; synfuels; production of fissile materials	Centralized, limited versatility. Some military connection and relevance to space colonies and space manufacturing	Also cogeneration, high-temperature process heat	Cogeneration?
Complexity	High	Medium	High	Low	Lowest
Reliability	Between 0.6 and 0.75	Same as LWR (fuel cycle reliability?)	No good reason to think it's worse than steam technologies. Between 0.6 and 0.9 (laser-exception)	Between 0.6 and 0.9, like other steam plants	Greater than 0.9 (= 1- time for repair)
Nominal capacity factor	0.6 to 0.75	Same as LWR	Between 0.6 and 0.9	Without storage: 0.2 to 0.25. With storage: up to 0.9	Without storage: 0.2 to 0.25. With storage: up to 0.9. Also depends on region
Material requirements	Design specific, can design around; stay away from specialized alloys	None	Can design around, common material, sophisticated processing	Plentiful, domestic materials; need to build manufacturing industry	Plentiful, domestic materials, like nuclear
Labor requirements	Like LWR	Like LWR	Few and skilled for space construction, less skilled for receiver construction	Moderate to large, decentralized larger	Moderate to large, decentralized larger
Land requirements	Same as LWR. Less than 1 acre/MW (including fuel cycle)	Same as LWR	Comparable to other centralized solar systems; 6.5 acres/MW or less	5 to 10 acre/MW	10 acre/MW incremental addition could be zero
Construction leadtime	5 to 12 years?	5 to 12 years (including licensing)	Similar to other centralized technologies, 5 to 12 years	5 years for 100-MW plant	Short; minimum 48 hours for 7 kW
Lifetime	Greater than 30 years (first wall material)	Greater than 30 years (replace steam generator;	Greater than 30 years; design like other systems, but limited experience	Greater than 30 years	Greater than 30 years
Costs of RD&D	$20 Billion to $30	$10 billion to $15 billion (?)	$40 billion to $100 billion to achieve first operating satellite	Low $0.5 billion plus $0.5 billion to $1.0 billion	$1 billion to $2 billion
Net energy balance	Unknown	l-year payback	2- to 20-year payback	1- to 2-year payback	2- to 20-year payback
Operating costs	Almost no fuel costs. Same as LWR, but less confidence	1 to 2¢/kWh	0.3 to 1.5¢/kWh; low as percentage of delivered cost	1 to 4 percent of capital costs; $40 to $60/kW/yr	1 percent; $20/kW/yr; less for centralized
Capital costs	$2,000 to $2,500/kW; lower for a 5-GW plant	$1,500 to $2,000/kW	$1,500 to $17,000/kW	$1,500 to $3,000/kW	$2,000 to $3,000/kW (peak) ($1.60 to $2.20/PW) (without storage)
T&D costs	Same as any central system	Same as any central system	Similar or greater than other central systems (reliability). Need to consider outage problem	Centralized—same as other systems; decentralized is negligible	Centralized—same as other systems; decentralized is negligible
Decommissioning costs	Minor	Minor	Push out of orbit. Small at 4-percent discount rate over 30 years	Negligible	Negligible
Impacts					
Institutional impacts (ownership)	Similar to present institutional structure	Similar to present institutional structure	Requires new management organization; international involvement possible	Decentralized— medium to high impacts; centralized— similar to present infrastructure	Decentralized— medium to high impacts; centralized— similar to present infrastructure

Table 15.—Characteristics of Five Electrical Technologies (continued)

Criteria	Fusion	Breeder	SPS	Solar thermal	Photovoltaics
Safety and health risks	Safer than PWR	Fuel cycle? Same as PWR (higher power density, lower pressure)	Microwave bioeffects uncertain; ionizing radiation in GEO	Small	Low; possible safety hazard with decentralization in event of fire
Environmental risks	Small for routine operation	Small for routine operation	Upper atmosphere effects uncertain	Small	Low possible manufacturing risk of PV
National security implications	Designs other than hybrid less significant than breeder	Significant weapons Proliferation potential	Not efficient weapon but transportation capabilities significant	None, possible benefits of exporting benign technology are good	None, possible benefits of exporting benign technology are good
Military vulnerability	Same as any centralized powerplant	Same as LWR	Slightly greater than other central powerplants, depends on space capability of other nations	Low	Low
Deployment considerations					
Time to commercialization	30 years plus	(Developed) 15 to 20 years domestic (licensing)	Long (greater than 20 years)	Between 5 and 10 years	Decentralized—5 years; centralized— 10 years
Geographic location with respect to load centers	Low population area	Low population area	Low population, no water needed; mixed	Decentralized—very close; centralized— S. W.-less than or equal to other systems. Geographic dependence high	Decentralized—very close; centralized— S. W.-less than or equal to other systems. Geographic dependence high
Compatibility with other technologies and utility grid	Good	Good	Penetration may be limited to 20 percent. Competes with other technology. Nothing obviously unsolvable	Goes down with higher percentage penetration; negligible problems	Goes down with higher percentage penetration; negligible
Other					
Probability for commercial success	Low to medium	High	Low to medium	High	High
Demonstration requirements	Large, but not as large as SPS	Moderate cost for 500 to 1,000 MW. About $1 billion	Large cost (0.3 to 1 GW)	Small (1OO-MW aggregate of 2 to 3 demos)	Small (community systems are medium)
Resource constraints	None	None	Manageable	Small	Small
Risks of RD&D failure	High, but for next 10 years little risk, $20 billion	Technology is here, but public views regarding waste?	High—big program; depends on program size (wait until HLLV available)	Negligible	Negligible
Relative uncertainties	High, complex	Small	High	Small (O&M costs)	Cell costs
Is it a viable example for the rest of the world?	Yes	Proliferation? for developed countries only?	Yes, if t works	Easier to digest in small to moderate chunks	Easier to digest in small chunks; need manufacturer capacity, but good example
Nature of RD&D process	Magnetic—public; inertial —classified	Much public money spent, remainder might be private, but for regulatory uncertainties	Public funds for RDD&T. Then private capital	Needs to be demonstrated by Government with private participation; industry will develop	Need not be demonstrated by Government, large private contribution

SOURCE: Office of Technology Assessment.

1. Central Solar Thermal.– Solar thermal technology is the oldest of the technologies under study. It may also be the one that is nearest to commercial application, since a pilot plant is already under construction in this country. The concept involves simply collecting concentrated solar radiation to heat a working fluid in a central receiver (boiler), which in turn drives a turbine to generate electricity. It has the versatility to provide either electricity or process heat (steam) for industrial applications.

Two generic systems have been proposed for the solar thermal approach: line-focus and point-focus systems. In the line-focus scheme, the Sun's radiant heat is reflected and focused by parabolic trough mirrors onto tubes containing the working fluid. The working fluid is pumped to a central site where it may be used to drive an irrigation pump, produce hot water or steam for a factory, or produce a combination of heat and electricity for a small community. The line-focus approach is also favored for process heat applications such as enhanced oil recovery, but is not being actively considered by DOE for central electric applications.

In the point-focus or "power tower" system, a field of reflectors (called "heliostats") is focused on a central receiver atop a tower in the center of the field. Although there are several designs, a heliostat is basically a flat reflective surface mounted on a computer-monitored gimbal that allows it to automatically track the Sun's course across the sky. The heliostat/power tower approach is being pursued by DOE as a central generating system, though not exclusively so. * It can be used for electrical generation either in a stand-alone system or as a method for repowering existing fossil-fueled power stations. The place of solar thermal in a utility system —whether it serves as a peaking, intermediate, or baseload unit— depends on the storage capability of the solar thermal plant. Without any auxiliary storage, its effective capacity factor will be about 23 percent in a location such as the southwestern United States. Addition of a modest amount of storage (sufficient for 3 hours of extended operation per day) will increase the capacity factor to about 40 percent and make it possible for the plant to supply part of the late-afternoon electric consumption peak that occurs in many utilities. Because it is desirable to smooth out the effects of short periods of cloud cover, it is likely that the technology will incorporate at least a small amount of thermal storage (up to 1 hour). Solar thermal plants could be made to operate in a baseload mode with the addition of a large amount of storage, but this increases the system's conversion loses and raises the overall cost per kilowatt installed. Solar thermal will, therefore, probably be better suited for intermediate or peaking uses since its daytime availability corresponds closely with the peak of the electricity load profile in many areas.

Solar thermal plants will be intermediate in scale between today's coal or nuclear plants and small onsite generators. They can be expected to be deployed relatively quickly– perhaps within 5 years for a 100-MW plant.

The technical feasibility of solar thermal technology is established. Engineering questions remain about the materials to be used in the design of the central receiver. What is at stake in making the technology commercially viable is whether plants can be produced economically. The single most important factor is the cost of heliostats, which accounts for about one-half the cost of solar thermal designs. Present cost estimates range from $1,000 to $3,000/kW of capacity installed.

Much of this high cost reflects the cost of materials. Savings realized from future automated production techniques are built into these projections. Thus, the economic viability of solar thermal technology depends on attaining heliostat cost goals.

The research, development, and demonstration (RD&D) costs associated with the solar thermal development are expected to be in the range of $0.5 billion to $1 billion. In addition

*In 1980, DOE initiated six major studies of the applications of the power tower to a variety of industrial heat demands, ranging from low-quality steam for uranium leaching to high-temperature steam for reforming methane to ammonia, to continuing tests and studies to reduce heliostat costs, R&D for efficient and cost effective storage methods, improved receiver designs and transport fluids are also needed.

2. Solar Photovoltaics. This technology is the newest of the terrestrial solar options under study and it is conceptually the simplest, since it converts sunlight directly to electricity without any working flu ids, boilers or generators. Because the essential element—a semiconductor wafer or "cell" — is modular at a very small size, the technology has a versatility in scale of deployment that surpasses any other option. Photovoltaic (PV) cells have already proved feasible in small-scale applications for both space and terrestrial purposes. However, central PV systems have not been tested yet, even in a pilot plant size. Because the technology is so intrinsically modular, the R&D program is not geared to the demonstration of a series of prototype plants but to the improvement of the cost and performance characteristics of the cells.

A variety of different semiconductor materials is being developed for possible use in central PV systems. When sunlight falls on wafers of these materials, it produces a direct current of electricity. The efficiency of this process depends on many semiconductor properties, and how well those properties match the wavelength spectrum of sunlight. Typically, the materials produce a direct current (DC) voltage level of about 0.5 volts. Some of the more promising PV developments include the four technologies discussed below.

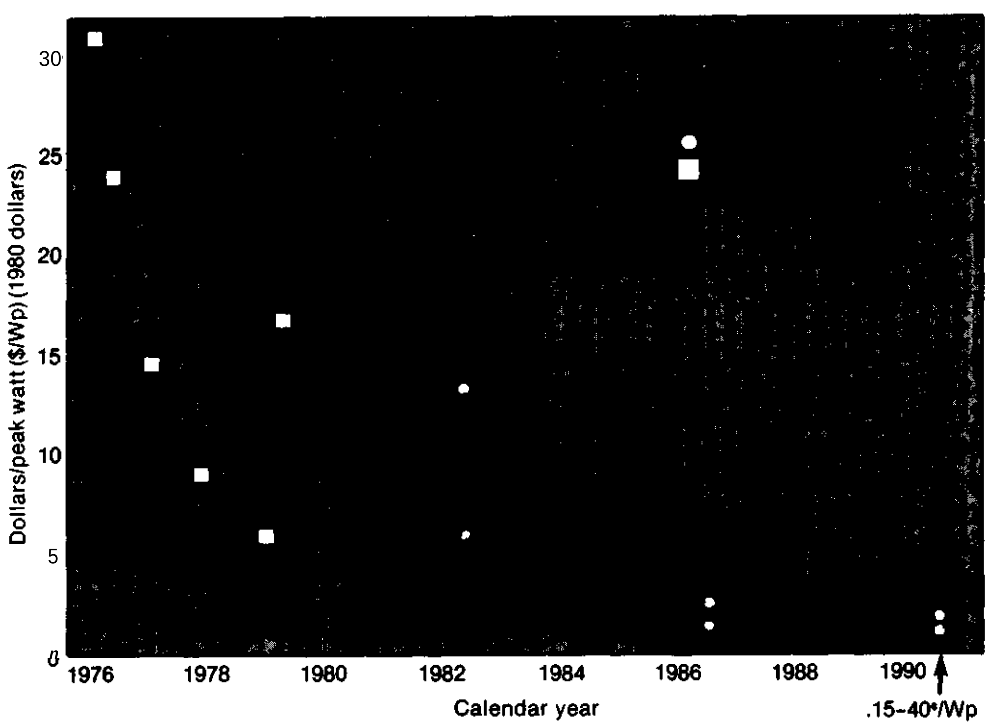
- The single cell silicon technology is the most highly developed, and its introduction dates back 23 years to the beginning of the National Aeronautics and Space Administration (NASA) space program. Its properties are well understood and cells sold commercially for small-scale applications routinely achieve efficiencies of 10 to 13 percent; experimental cells have achieved 15 percent and the theoretically probable maximum is 20 to 22 percent. The single most important barrier to commercial use is the high production cost, even though costs have dropped and performance improved over the past decade in line with DOE projections. Further cost reductions to ($95/m^2) $0.70/peak watt) and performance improvement to 13.5-percent efficiency are the DOE goals for 1986.
- The cadmium sulfide/copper sulfide technology is another approach that is commercially available and holds promise for improvement. This material can be used in thin films because of its high absorbance of sunlight, with a reduction in fabrication costs and materials requirements. Experimental cells have achieved efficiencies of 9 percent, with limited lifetime. Improved cells have the potential for cost reductions to $10/m^2 at 10-percent efficiency. A number of other cadmium sulfide technologies are under study for thin film and standard cells.
- The gallium arsenide technology is another alternative that has achieved efficiencies up to 24.5 percent in experimental cells. The material can be fabricated in thin films (with experimental efficiencies to 15 percent) and can withstand concentrated sunlight at high temperatures. Its major disadvantage is that commercial production is still some time away and costs remain much higher than for single-crystal silicon.
- *The polycrstalline and amorphous silicon* technologies have the potential for orders of magnitude cost reductions compared to the single-crystal silicon technology, but the experimental cell efficiencies have so far only reached 9 to 10 percent. (The probable maximum is estimated to be at least 15 percent for the amorphous technology in thin film cells.) These technologies are not limited to silicon, but are currently being investigated along with other novel materials concepts.

All the technologies discussed above are candidates for use in flat-plate arrays of cells that absorb unconcentrated sunlight. Gallium arsenide is also an example of a high-efficiency material that can be used with a concentrating system. Concentrating systems involve different tradeoffs and are further from commercial viability than flat-plate systems. Both line- and point-focus collectors are under consideration for PV concentrating systems. Costs of concentrating systems can in principle be low, since the receiving area needs only to be covered with a thin reflective sheet, but the technology is not developed enough to make project ions yet.

Up to half the cost in a flat-plate design terrestrial solar photovoltaic plant today is for the cells themselves. Other requirements for a complete plant are materials for packaging and supporting arrays of cells, support structures, cabling to connect the arrays and modules, and power conditioning equipment to convert the DC voltage to alternative current compatible with the utility grid. About 300 cells would be combined into one panel, 30 panels into one array, and 10,000 arrays into one module supplying 25 MW of peak power. A central plant might produce 200 MW from 8 modules. Storage could be added to extend the capacity factor of the plant, at additional system cost. As discussed in the introduction to this section, the economic merit of dedicated storage for utility-based PV systems has been seriously questioned.

The pace of technological breakthroughs in PV technology is impressive. Today single-crystal silicon cell arrays cost 15 percent of what they did in 1974, as can be seen in figure 27. It is on further orders-of-magnitude cost reductions that both terrestrial and SPS PV systems depend. Such price reductions are common in the semiconductor industry for products with large markets (e.g., digital watches, hand calculators, and now hand computers), but they are nearly unheard-of in the energy industry. Therefore, planners familiar with conventional thermal and nuclear energy technology sometimes find them difficult to accept. The goals for the DOE PV program are

Figure 27.—Recent and Projected Solar Photovoltaic Prices



SOURCE: Department of Energy.

for array prices of $2.80/peak watt in 1982, $0.70 in 1986, and $0.15 to $0.40 in 1990 (all in 1980 dollars). At the 1990 level, complete systems are expected to cost $1.10 to $1 .80/peak watt.

Although significant breakthroughs have occurred in the past 5 years, the principal thrust of PV research is still directed toward the identification, selection, and engineering refinement of the cheapest possible semiconductor materials. A concomitant part of this effort is the development of suitable mass-production techniques (now being most intensively pursued for single-crystal silicon and cadmium sulfide) to open the way for mass market penetration. It is upon the outcome of this two-pronged effort (development of cells and development of better manufacturing techniques) that the success of central terrestrial PV plants will depend.

The time-scale for commercial readiness of central terrestrial PV plants could be as short as 5 years or as long as 15 years. The balance of a central PV plant uses familiar building materials and readily available power-handling equipment. Once arrays are available, plant construction leadtime should be short. According to the DOE program, commercial readiness could occur in the early 1990's. If the RD&D program for PV cells is accelerated this date could be earlier; on the other hand, slippage in the schedule for cell development could delay commercial introduction.

Subsequent deployment of central PV systems would be paced by the rate of growth of national manufacturing capacity for PV cells. To achieve substantial penetration of central PV in the time period of 1990 to 2010 will require an aggressive program for PV manufacturing plants. It is possible that decentralized PV centralized terrestrial and SPS energy systems could all be competing for the output of the PV industry during this period.

3. Advanced Fission (Breeder Reactor).– Conventional reactors use uranium ore very inefficiently because only a small fraction of the uranium is tapped for energy. Natural uranium consists of two isotopes 99.3 percent U-238 and 0.7 percent U-235. Only the U-235 is usable directly in a conventional reactor. With conventional reactors, uranium resources would be exhausted relatively rapidly by an expanding nuclear energy base. Breeder reactors on the other hand, can extract 100 times as much energy from a ton of uranium ore and thus extend the nuclear energy resource by several centuries.

In a breeder, the core of the reactor is surrounded by a blanket of the type of uranium not burnable in conventional nuclear plants. This uranium captures neutrons escaping from the chain reaction in the core and is transmuted into plutonium, a premium value nuclear fuel. In this fashion, a breeder "breeds" new fuel that is extracted from the blanket, converted into fuel rods, and later burned in the same or another reactor. An advanced breeder will produce about 10 percent more fuel than it burns. A different fuel cycle could use thorium in the blanket. Thorium is an element similar to uranium, but it cannot be used directly as a fuel. In the blanket, it transmutes to U-233 which is a good fuel.

Breeders may also be distinguished by the different types of coolants used to carry heat from the core to the generating side of a nuclear plant. Because the interconnections between the core and the generators are quite complex, requiring considerable engineering refinement, the choice of coolant defines conceptually different types of breeders as much as or more than the choice of fuel. Early in its program, the United States emphasized breeders with liquid metal (usually molten sodium) coolants and the reactor concept that evolved —the liquid metal fast breeder or LMFBR– has become the reference system for breeder research in other countries, representing more than 95 percent of the dollar effort devoted worldwide to breeders. Thirteen reactors using the LMFBR concept have been built, the most successful being the French Phenix reactor, and seven countries with major breeder programs (table 16) have all emphasized the LMFBR type. Alternatives are helium gas, molten salt coolants, and water.

Table 16.—Description of Milestones of Major Breeder Programs

	France	Federal Republic of Germany	Japan
Reactors	Rapsodie (24 MWt) Phenix (250 MWe) Super Phenix (1,200 MWe)	KNK-I (58 MWt) KNK-11 (58 MWt) SNR-300 (300 MWe) SNR-2 (nominally 1,600 MWe)	Joyo (100 MWt) Monju (300 MWe)
Milestones ► Planned	1. 1962—Rapsodie construction starts 2. 1967—Rapsodie goes critical 3. 1969—Phenix construction starts 4. 1973—Phenix goes critical 5. 1976—Super Phenix construction starts ►6. 1985—Super Phenix goes critical ►7. 1983—1987 Early Super Phenix orders	1. 1960—GFK, Karlsruhe project begins 2. 1964—Design study for 1,000 MWe LMFBR 3. 1966—SNEAK startup 4. 1975—SNEAK experiments for SNR 300 5. 1967—INTERATOM F.R.G. and BENELUX cooperation begins 6. 1972—KNK-I goes critical 7. 1976—KNK-11 goes critical 8. 1969—SNR-300 safety report 9. 1970—SNR-300 company established 10. 1971—SNR-300 revised safety report 11. 1972—SNR-300 sodium fuel pumps tested 12. 1973—SNR-300 construction begins 13. 1974—SNR-300 steam generators and IHX test 14. 1975—SNR-300 specification of fuel and cladding 15. 1980—SNR-300 goes critical[a] 16. 1974—SNR-2 company established 17. 1976—SNR-2 preliminary design[a] 18. 1981 —SNR-2 construction begins[a]	1. 1967-Joyo conceptual design 2. 1969—Joyo safety evaluation 3. 1970—Joyo construction begins 4. 1977—Joyo goes critical 5. 1968—Monju preliminary design 6. 1969—Monju conceptual design 7. 1973—Monju safety evaluation 8. 1978—Monju construction begins ► 9. 1984—Monju goes critical -10. 1986—Demo plant begins -11.1991 —Demo plant goes critical -12. 1988—Commercial plant 1 construction begins ►13. 1995—Commercial plant 1 goes critical -14.1991 —Commercial plant II construction begins ►15. 1996—Commercial plant II goes critical

[a]Schedule as of 1978. In 1980, the SNR program currently in flux. SNR-300 designed but not yet licensed. SNR-2 not Yet designed. Entire program will slip substantially, but the new schedule is not known at this time.

SOURCES: ***France:*** U.S. Energy Research and Development Administration, *The LMFBR Program in* France, ERDA 76-14, March 1976; M. D. Chauvin, "The French Breeder Reactor Program," 1976.
Federal Republic of Germany: U.S. Energy Research and Development Administration, *The /_ MFBR Program in Germany,* ERDA 76-15, June 1976.
Japan: Report of Ad Hoc Study Committee organized by Japanese Government Science and Technology Agency, October 1977.

SOURCE: International Energy Associates Limited, 1980.

	United Kingdom	United States	U.S.S.R.
Reactors	DFR (60 MWt) PFR (250 MWe) CFR (commercial size)	Clementine (25 kWt) EBR-1 (1.2 kWt) Fermi (200 MWt) EBR-11 (16.5 MWe) Clinch River (375 MWe) Fast Flux Test Facility (equivalent of 160 MWe) PLBR (commercial size) CBR (commercial size)	BR-5 (10 MWt) BOR-60 (60 MWt) BN-350 (1 ,000 MW) DN-600 (600 MWe)
Milestones ► Planned	1. 1953—first nuclear power program begins 2. 1964—second nuclear power program begins 3. 1963—DFR goes critical 4. 1984-PFR construction begins 5. 1972—PFR goes critical	1. 1946—Clementine goes critical 2. 1951—EBR-1 goes critical 3. 1963—Fermi goes critical 4. 1966—Fermi shuts down 5. 1983 -EBR-II goes critical 6. 1971–SEFOR (U.S. and F. R. G.) goes critical	1. 1958—BR-5 goes critical 2. 1965—BR-5 operates full core 3. 1969—BOR-60 goes critical 4. 1973—BN-350 goes critical 5. 1973—BN-600 construction begins 6. 1979—BN-600 goes critical 7. 1975—1,600 MWe reactor design underway

Table 16.—Description of Milestones of Major Breeder Programs (continued)

United Kingdom	United States	U.S.S.R.
Milestones ► Planned		
6. 1973—PFR shuts down for repairs	7. 1979—Fast Flux Test Facility construction complete	
7. 1974—third nuclear power program begins	8. 1975—Clinch River order components	
8. 1972—CFR design	9. 1977—Clinch River construction begins	
	► 10. 1982—Clinch River construction complete[a]	
	11. 1975—PLBR start conceptual design[a]	
	► 12. 1982—PLBR construction begins[a]	
	► 13. 1988—PLBR construction complete[a]	
	► 14. 1986—CBR-I construction begins[a]	
	► 15. 1988—CBR-I goes critical[a]	
	► 16. 1989—GCFR 300 MWe demonstration plant goes critical[a]	

[a]This was the U.S. program in 1978. Currently there is no planned PLBR schedule, pending final decisions on CRBR. CRBR is "in Construction," but has been in a holding pattern for 2 years.

SOURCES: United Kingdom: Prepared by IEAL from compilation of U.K. documents.
United States: U.S. Energy Research and Development Administration, *Liquid Metal Fast Breeder Reactor Program,* January 1977; U.S. Energy Research and Development Administration, *The LMFBR Program in France,* ERDA 76-14, March 1976; Ford Foundation, *Nuclear Power Issues and Choices: Report of the Nuclear Energy Policy Study Group, 1977.* Note on U.S. program: Items 9-15 refer to the program as it stood in April 1977, The plan and schedule has been in revision since then, but it is not yet available.
U. S. S. R.: United States Nuclear Power Reactor Delegation, "Report of the United States Nuclear Power Reactor Delegation Visit to the U. S. S. R., June 1-13, 1975," 1975.

SOURCE: International Energy Associates Limited, 1980.

As a source of centrally generated electricity, the breeder has been proven feasible at the pilot plant scale and at an intermediate scale but awaits demonstration at commercial scale —that is the 1,000-MW size of new conventional reactors. Its operating characteristics are expected to be similar to a conventional (light water) reactor, except that it will have higher thermal efficiency and therefore less thermal pollution. Breeders may also in principle be used for industrial process heat. The Russian breeder BNR-600 produces electricity and desalinated water.

The technology was demonstrated at a pilot plant scale in the United States in 1963, when a 10-MW reactor named EBR-II started producing electricity in Idaho. Between the 1960's and 1970's, technical leadership shifted from the United States to France. The Phenix which has produced electricity for more than 5 years at Marcoule, France, demonstrated successful scaling from 10 to 250 MW, but suffered some technical problems that required the plant to shut down for more than a year. Its breeding rate is considered too slow for commercial use, and some components (especially steam generators) are not extrapolatable to commercial size. France, together with the Federal Republic of Germany and Italy, is now building a 1,250-MW reactor incorporating an improved design – Superphenix – at Creys-Malville. Due to go critical in 1985, it will be the first commercial prototype breeder.

The time until commercialization of the breeder is 5 to 20 years depending on which breeder technology (French or U. S.) is meant. On the face of it, commercial readiness will occur in 1985, assureing success of the Superphenix. After that, France plans an aggressive program of breeder deployment, starting a new plant every 2 years for the rest of the century. * The French central utility (EdF) has already ordered the first two of these "commercial" plants. Progress on the U.S. plant comparable to the Phenix (the Clinch River breeder) has stalled, and its technology is outmoded in some respects. Some argue this intermediate plant step should be skipped to go to a commercial-size or nearly commercial-size plant.

*A reevaluation of these plans is apparently underway in France following the recent election

The leadtime for constructing a conventional nuclear plant in the United States is 12 years and design and construction of a full-scale breeder prototype under the same ground rules could take 15 years. Thus, U.S. breeder technology could be commercialized sometime in the 1990's, depending on the development sequence.

The major difference between the French and American technologies is whether the reactor vessel uses a "loop" or "pool" method of bathing the core with liquid sodium coolant. The pool method is simpler, has more thermal inertia, and is considered by the French to be an added safety factor. The loop method is more similar to conventional reactor technology and has been tested on an intermediate-scale U.S. breeder used for fuel development (the FFTF). Britain and France espouse the pool approach; the United States and Japan use the loop method; and the Soviet Union and the Federal Republic of Germany are testing both.

In principle, a U.S. utility could order a Superphenix reactor now for delivery in the early 1990's and in that sense the breeder could be said to be commercially available already. But no utility would invest in a central nuclear plant without reasonable assurance it would be reliable and could be licensed in this country. The licensability of the French technology is an open question.

The RD&D cost of commercializing the breeder is uncertain because the national policy for 1976-80 was to not deploy the breeder. It is also dependent on the demonstration strategy chosen (i. e., whether to go straight to a commercial prototype). Estimates made by the U.S. program managers in 1975 of $10 billion to $15 billion for commercial demonstration should still apply.

The obstacles that the breeder program must overcome before commercialization are not primarily technical. There is little doubt that a strong breeder RD&D effort could result in a reactor that utilities could order in a few decades. The questions are economic and institutional and generic to nuclear power. For the purposes of this discussion, the economic questions are less important. Unless the breeder costs are so high that it is uneconomic compared to other options, the major concerns are related to light-water reactors. These will not greatly affect the SPS decision.

Deployment of the breeder is predicated on the continued expansion of light-water reactors. The problems facing the industry are complex and difficult as discussed in the section on conventional nuclear reactors above. If these problems are not resolved, the fission option will be foreclosed, at least as a major energy source. Fusion may also be threatened. The breeder exacerbates some of these problems. Proliferation of nuclear weapons will be considerably harder to control if breeders are worldwide articles of commerce. While this might not have a direct bearing on a utility's decisionmaking process, the safeguards implemented to prevent diversion might be quite onerous, and public opinion could be hostile. Health and safety issues will be important because of the plutonium and the operating characteristics of the reactor. Waste disposal will not be qualitatively different, but the vastly greater potential of breeders to produce waste make the problem greater, especially if disposal sites are difficult to find. While these problems, individually or collectively, need not be overwhelming, they can all adversely affect a utility's inclination to order a nuclear plant. As long as a utility has a choice within a reasonable economic range, it is likely to select the less controversial options. Thus, while breeders could in principle supply all the electric power needed in the 21st century, they may in fact supply little or none.

4. Fusion.– Of the future energy sources considered here as competitors to the SPS, fusion is the furthest from realization. Fusion consists of nuclear reactions that are created by bringing together light nuclei at speeds great enough to exceed their mutual repulsive force. The result of this reaction is the creation of nuclear energy that is carried off by neutrons and/or charged particles, depending on the nature of the reactants. In order to create this reaction it is necessary to: 1) raise the temperature of the fusion fuel to very high

levels and, 2) confine the fuel for sufficient time. The criterion to be met by these two conditions is that more energy is released by the nuclear reactions than is used to heat and confine the fuel that is in a wispy, gaseous form (plasma).

Since the fusion reaction would be rapidly cooled by the reactor walls, containment by solid materials is not possible. Such an approach would quench the plasma. This difficulty, incidently, would also make a fusion reactor easier to turn off, making it safer than fission. Two alternate approaches are being taken: using a magnetic field in one of many possible shapes that have been proposed, (magnetic fusion); and using a laser or ion beam to produce a miniexplosion of the fuel in solid form so that confinement occurs by the inertia of the fuel (inertially confined fusion or ICF). The second approach draws on nuclear weapons work for some of its research and is partially classified. The discussion to follow will center on the magnetic approaches.

Among different magnetic confinement concepts—or types of "magnetic bottles" —the leading contender is a toroidal shape called the tokamak, after the Russian acronym given by its inventors. As a reactor, it would be considerably more complex than a conventional powerplant. The mixture of deuterium and tritium fuel planned for use in first-generation fusion reactors burns at a very high temperature, 100 million $^\circ$C. The natural current in a tokamak system is not sufficient for heating the fuel that hot, so additional and complex heating systems are required. The fusion core will be large enough that electrical losses in the magnets would be a significant drain on the output of the plant, unless superconducting magnets are developed specifically for fusion applications. Other complexities arise from the fuel requirements and operating requirements. Any fusion system must breed half its fuel (the tritium component), and tokamak systems currently under development must operate in a pulsed (few hour) mode rather than a continuous power mode.

These factors make fusion more complex than either conventional reactors or breeders. Particular difficulties in understanding the behavior of the fusion fuel in its very hot (plasma) state explain why scientists have had so much difficulty making progress in fusion research (which began in 1954).

Fusion is unique among future energy technologies because it has not yet been proven technically feasible—that is to say, no controlled fusion reaction has yet operated in a self-sustaining fashion or produced electricity even on a small scale. It has a broad range of potential applications, e.g., electricity production, high temperature process heat, synthetic fuel production, and fissile fuel production.

The fusion community can point to a recent string of successful experiments as evidence that fusion is on the verge of a scientific breakthrough. One of the goals is "breakeven, " meaning the achievement of positive net energy production. DOE expects breakeven to be achieved before 1985, and a recent review by the research oversight board of DOE[9] concluded that fusion was ready to move from the research stage to the engineering development stage. Nevertheless, the weaker understanding of the principles of controlled fusion compared to other energy technologies means that more emphasis is necessarily being placed on basic research. Consequently, the engineering-related considerations that influence commercial readiness and acceptability—that is the technical, economic, and environmental factors — are more uncertain than for breeders, solar thermal, PV or SPS.

Despite the high degree of uncertainty, much more can be said about the engineering features of fusion than was possible a few years ago, based on a set of thorough and detailed engineering studies. Using the tokamak as a reference system, a powerplant is likely to be in the range of 500 to 1,500 MW, with 1,000 MW being the nominal planning size. A tokamak fusion reactor would operate as a baseload plant, with capacity factors, construction leadtimes, plant lifetimes, land, labor, and materials requirements similar to

[9] ERAB Report (DOE), 1980

conventional nuclear plants. The high-technology core would constitute a substantially larger percentage of the total plant than for conventional (or breeder) nuclear plants, and fusion would have some unusual maintenance problems that arise from the character of the fusion reaction itself. Since the nature of the fusion core must be considered hypothetical until technical feasibility is proven, the economics of fusion is perhaps the most uncertain characteristic at this time. Two different engineering studies prepared at the University of Wisconsin[10] and Argonne National Laboratory" put the busbar costs at 75 and 44 mills/kW respectively (in 1980 dollars).

Because of the special character of fusion, estimates of the timetable to commercial readiness vary widely. A recent survey of opinion found the majority of estimates to fall between 2000 and 2025, with some as early as 1990 and a few extending to the 22d century or never.[2] It appears unlikely that fusion will be commercialized before 2010— the earliest likely date for SPS – and the present DOE program is on a schedule calling for "demonstration" in 2015, with the dates 1995 or 2000 considered possible at increased cost. The DOE program calls for two steps after breakeven in 1985, the first a fusion engineering demonstration in 1990 that produces thermal power but no electricity. Pending success with this plant, a fusion demonstration plant would be started by about 2000, that could produce 500 to 1,000 MW of electricity. However, more steps are likely to be needed prior to commercialization. Fusion research is in such an early phase vis-a-vis other technologies that it is difficult to determine reliably the path to "commercial" fusion.

To be commercialized, fusion must also find public acceptability. From an environmental, health, and safety standpoint, the principle advantage of fusion over fission power is that there is no conceivable possibility of a runaway reaction. But first-generation fusion plants will use relatively large quantities of tritium, a radioactive gas harmful to humans. Advanced fusion fuel cycles would greatly reduce the quantity of tritium that must be handled. To make fusion safe, the problem of handling industrial quantities of tritium without routine small emissions will have to be solved. There will also be a substantial waste disposal problem, because the "first wall" of the containment chamber for magnetic systems will have to be replaced every few years due to radiation damage. Since the replaceable "wall" may be up to 1-m thick, the quantity of waste could be high, measured in the tens or hundreds of tons per reactor per year. This material will be highly radioactive and will present a long-term waste disposal problem, though the radioactivity will not be as long-lived as conventional fission reactor wastes. The amount and lifetime of radioactive material can possibly be reduced substantially by using other materials for the first wall without changing the nature of the fusion reaction. Analogous changes for fission reactors are not possible since the waste material generated is an inherent part of the fuel element. Finally, fusion carries some proliferation risk because the energetic neutrons of the fusion reaction comprise a high quality source for producing weapons material. It is conceivable that unless proper safeguards are developed, a world full of fusion reactors could be highly proliferation prone. However, there are many other technologies that are available or could be available for the same purposes earlier, more readily, and more cheaply than fusion.

To a degree, fusion may also inherit the public acceptance problems of nuclear fission. Fusion is a different technology, with fewer intrinsic risks but greatly increased complexity. But since it is a nuclear technology, even if it turns out to be relatively benign compared to fission, it may remain associated with conventional nuclear power in the public mind.

The greatest uncertainty in the development of fusion remains the physics associated with

‘°” NUWMAK: A Tokamak Reactor Design Study, ” Fusion Engineering Program, Nuclear Engineering Department, University of Wisconsin UWFDM-330, March 1979

[1]“Argonne National Laboratory, *Starfire*

“”Chase Delphi Study on Fusion, First Round Results, ” Chase Manhattan Bank, September 1979 breakeven. Although many of these uncertainties can be resolved by small experiments costing on the order of $1 million to $10 million, complete resolution will still require a few large sophisticated experiments, costing in excess of $1 billion. It should be noted, however, that the nature of the fusion reactioc is such that a demonstration reactor would require very little increase in scale or cost from these large experiments. The total cost to develop fusion to the stage of commercial viability depends significantly on the cost of this "hardware" and is projected by DOE to be $20 billion to $30 billion. If more than two major steps are needed before a commercial prototype can be built, the cost will be somewhat higher.

5. Comparisons of Central Electrics. -Because each of these future electric technologies is designed for use in a central plant mode, they are best compared in the context of a utility company's needs. If each of the different technologies were at the same stage of development, comparison based on projected power costs would be the most powerful and appropriate method of analysis, particularly if all were close to commercial maturity. But the five are at quite different states of technical maturity — so much so that even the definitions used for "commercial" maturity used in the different programs may be qualitatively different. Lacking information that may take 5 to 20 years to acquire, a close look was taken at other characteristics, with particular attention to properties–such as complexity, health effects, and safety — which past experience has shown to be closely related to both capital and operating costs.

After costs, the most important issue the utilities must consider in deciding to risk capital on a particular investment in a generating technology is the way in which a plant is expected to function and its associated impacts. Can the proposed technology be successfully integrated in the grid and meet the associated requirements for reliability and capacity? These issues are discussed for the SPS in chapter 9. This section will highlight the factors most important for the other central base load technologies.

Scale of Power Output. – Plants must be designed on a scale that can be readily integrated into the existing grid at the time of deployment. Using the rule of thumb that no one plant should comprise more than 10 to 12 percent of the system's capacity to guarantee integrity of the grid during a plant failure, the largest plant that could be presently accommodated by a single utility in the United States would be 2,500 MW, and that only by the Tennessee Valley Authority. (See ch. 9. for a discussion of this issue.) Cooperative agreements among utilities on the same grid can expand the maximum acceptable size. Current baseload plants generate from 500 to 1,300 MW. Both fusion and breeder plants are planned to fit closely within this range. Very large powerplants (greater than 1,500 MW) were the rule in fusion planning several years ago, but encouraging new research results coupled with new interest in smaller powerplants allowed fusion engineering designers to direct efforts toward conceptual designs in line with present powerplant scales. Larger plants would mean improved economies of scale for the breeder (as it would for fusion), but for utility compatibility reasons (as well as licensability), the projected size of the breeder has also been kept below 1,500 MW.

Solar thermal and solar PV plants achieve their economies of scale at much lower outputs–100 to 200 MW maximum. Both can function economically at still smaller scales. Photovoltaics are modular and economic at a few kilowatts or less.

Only the reference system SPS appears to have economies of scale that make it impractical at a size that can be accommodated by the present utility systems. Whether it could be accommodated in future utility systems depends on the growth of future electric demand. Smaller microwave systems or a laser system would fit the utility grid more readily.

Reliability and Capacity Factor. – Prior to the demonstration of a technology, both its

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capacity factor and its projected reliability are subject to considerable uncertainty. However, it is expected that breeders will operate much as conventional light water reactors do today, with capacity factors of 60 to 75 percent and forced outage rates (that is, unplanned shutdowns) of less than 15 percent.

The steam and electric generation parts of fusion plants are expected to be similar to conventional reactors and breeders. But the fusion core will be much more complex than the nuclear parts of a conventional or breeder plant. One indication of this is that the fusion core is expected to represent a much larger fraction of the plant investment (50 v. 10 percent for nuclear). Because of the vast uncertainties surrounding the actual operating characteristics of fusion technology, it is impossible to predict what capacity factors and forced outage rates are likely to be. It is clear that to compete with breeders or light water reactors, fusion should be just as reliable and capable as they are.

Solar thermal is a steam technology, with a balance of plant that will be similar to, though smaller than, that for a conventional baseload plant. The solar-thermal part will be chiefly vulnerable to failure of the heliostats or the boiler. The heliostat fields could have tracking or maintenance problems, the boilers could have materials and integrity problems due to the high solar flux. Nevertheless, it is projected to operate with reliability similar to other steam technologies —60 to 90 percent.

Solar PV is the simplest technology, without steam systems or moving parts or (necessarily) high solar flux, if flat plate systems prove most economic. Because it is simple, the reliability of solar PV is expected to be very high (greater than 90 percent). There may be unsuspected durability problems with some solar PV cells, however. Although PV are an intrinsically simple technology, it currently has higher material and manufacturing costs than other alternatives. Both solar thermal and solar PV have an inherent limitation of plant capacity factor, due to the daily and yearly variation of ambient sunlight, which differs with latitude and climate. In the Southwestern United States, the capacity factor of a plant without storage would be 23 to 25 percent. Storage for a solar thermal or solar PV plant redistributes the collected energy to other times of day, but does not appreciably change the amount of energy Collected per year per acre of plant area.

The SPS would circumvent the 25-percent capacity factor limitation of terrestrial solar plants by being exposed in space to direct sunlight 24 hours per day all year (except for brief, predictable eclipses if located in geostationary orbit, or unpredictable cloud cover if a laser or mirror system). The question with SPS is not solar capacity but availability. As with fusion, it is impossible to predict just how reliable the SPS would be. As a system it is very complicated, involving a massive transportation system, untried satellite technology, and large ground systems. Reliability factors as high as 95 percent have been predicted for the operation of the satellite and rectenna combined,[13] but they have not taken into account the entire SPS system, including maintenance and repair. Research on transportation and space platforms will provide considerable insight into the expected reliability of the satellite.

Complexity. –Given the extreme range of physical requirements for a sustained, controlled fusion reaction, fusion is clearly the most complex technology under consideration, requiring a plasma hotter than the core of the Sun, powerful large superconducting magnets bigger than any yet built, and materials problems in a radiation environment more severe than that of the breeder. The reference system SPS is less complex than fusion, since it uses more nearly proven technologies. Nevertheless, the overall engineering and logistics problems of the SPS could make it an undertaking that approaches the complexity of fusion when all the technical hurdles are considered. It should be noted, for in-

[13] "SPS/Utility Grid Operations," sec 14 of Boeing Corp., report No D 180-25461-3

stance, that the SPS as it is described in the reference system could only begin to be assembled as a system after major breakthroughs in two other technologies—space transportation and PV — are achieved.

The breeder is considerably less complex than either the SPS or fusion, but is more complex than conventional nuclear systems. The main potential difficulties are the nuclear properties of the breeder core, the peculiarities of the liquid metal coolant, and the potential difficulties of the breeder fuel cycle. Although these factors are incremental additions to the complexity of a nuclear plant, they are the driving factors behind the projections that the breeder will cost 25 to 100 percent more than a light water reactor (LWR).

The solar thermal plant is also a steam system that has much of the complexity of other steam systems, such as coal or nuclear, mitigated by the reduced size of the plant and the modularity of the heliostat field. There may be special problems in having a central plant boiler at the top of a tall tower, but solar thermal plants appear to be less complex than nuclear, fusion or SPS technologies. Their complexity may be comparable to current base-load coal technologies.

Central PV plants have by far the least complexity of the alternatives discussed here, for two reasons. First, the basic technology is simple, modular, and should be manufactured cheaply if the experience with mass-produced semiconductor products holds as expected. Second, the additional technology needed for a central plant is electrical rather than mechanical or thermal, and is already proven at the appropriate scale.

Costs. –The cheapest acceptable technologies available in any future time period will be the ones deployed, so cost is the most important — and most problematic —factor. Two aspects of technology cost will be discussed. The busbar cost is the cost at which truly commercial versions of the various electric technologies will produce power. The opportunity cost is the total cost of RD&D for a technology from inception through the construction of a commercial prototype plant. It is the cost of lost opportunities in other areas for which the money could have been spent. A component of the opportunity cost is the cost of the commercial prototype itself, which is the demonstration cost.

The busbar cost is the actual cost of producing electricity with a technology when capital costs, fuel costs (if any) and operation and maintenance costs have been considered. For current technologies, these costs are well-known and therefore detailed comparisons between technologies are possible. However, even for current technologies the task can be difficult–witness the debate over whether coal or conventional nuclear is cheaper. For future technologies, the task is much more uncertain. Therefore, cost estimates of delivered electricity are of little use in deciding between technologies in early development stages. Furthermore, technologies reach commercialization at different times. Therefore, cost estimates for one technology are more reliable than for another, with the most fully developed technologies having the most thoroughly tested cost data. For example, coal plant costs are well known, but breeder costs are less so, and fusion costs are much less so. Though it is a current technology, the future costs of PV for onsite, central, or SPS plants, depend strongly on the future costs and efficiencies of PV cells and are consequently uncertain as well. A final note on busbar cost estimates is that as a technology matures, the projected cost may fall (as has happened with computers) but much more often rises. The maturation effect of costs during R&D has been particularly borne out in aerospace and energy technologies.[14 15]

Although busbar cost estimates are useful in the research phase to identify cost sensitivities and indicate preferred research directions to reduce costs, they become crucial at the

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[14] U S General Accounting office, "Need for Improved Reporting and Cost Estimating on Major Unmanned Satellite Projects,'" PSAD-75-190, 1975.

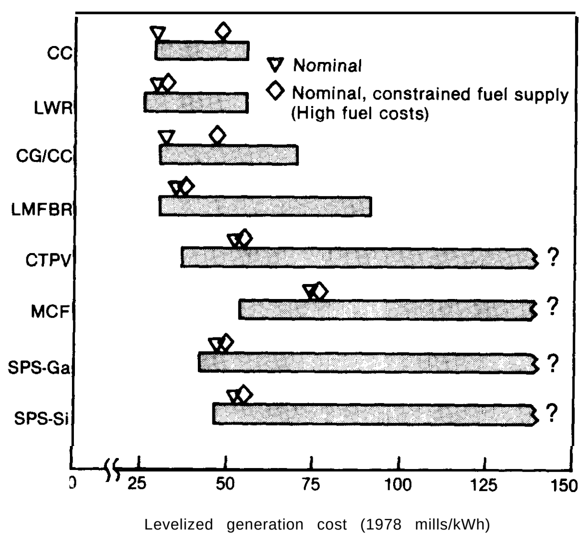
[15] F W Merrow, S W Chapel, and C Worthing, "A Review of Cost Estimation in New Technologies Implications for Energy Process Plants, " R-2481-DOE, Rand Corp , 1979

deployment phase. The DOE prepared cost estimates for coal, light water reactors, coal gasification systems using combined cycle systems, LMFBR breeders, peaking terrestrial PV plants, fusion and the SPS (fig. 28). ” The figure indicates the high and low ends of the range of estimates for each technology in 2000. It shows that capital costs do indeed increase with complexity, rising steadily for coal, LWR, LMFBR, fusion, and SPS systems. Costs are also relatively high for the terrestrial PV. Although it is an unlikely circumstance, the chart indicates that all could cost the same in 2000.

OTA prepared estimates that considered these future electric technologies including fusion (but not combined cycles), in terms of their busbar costs in 2010. **The results are given in table 17, using common financial considerations, equal capacity factors (65 percent in all cases), and the assumption of baseload operation for each technology. As noted above, these numbers may be indicative but are limited in their use because the uncertainty range represented by the range of costs means different things for each different technology. Factors that are small contributors to the estimated costs may have uncertainties that are substantial (such as nuclear waste disposal costs) but are difficult to identify and measure. Finally, baseload operation is not necessarily the most attractive operating mode for solar thermal and solar PV though it provides a basis for comparison.**

“Program Assessment Report statement of findings, SPS **Concept and Evaluation Program, DOE/E** R-0085, 1980

Figure 28.—Levelized Lifecycle Cost of Electricity



CC Conventional coal
LWR Light water reactor
CG/CC Coal gasification/combined cycle
LMFBR Liquid metal fast breeder reactor
CTPV Central station terrestrial photovoltaic
MCF Magnetically confined fusion
SPS Satellite power system
Si Silicon solar—cell option
GaAs Gallium arsenide solar—cell option

SOURCE: *Program Assessment Report Statement of Findings,* SPS Concept and Evaluation Program, DOE/E R-0085, 1980.

RD&D Costs. **–One of the most difficult tasks in choosing the wisest course for** RD&D **is to maintain the proper balance between the risks and the potential payoffs associated with a particular line of research. The goal is to minimize the risk and maximize the payoff. In energy research, the risk is associated with the expenditure of RD&D funds for a project that could conceivably fail. The hoped-for payoff is cheap energy. The associated RD&D funds required to pursue some of the future electric options under consideration are so great that it is likely that not al I can be pursued at an optimum rate. By according priority to some, opportunities for payoffs from others will be foregone.**

As the matrix of table 16 **makes clear, SPS will have the highest front-end costs by a considerable margin, followed by fusion and the breeder. The solar thermal and solar PV systems will have lower RD&D costs, in the range of $0.5 billion to $2 billion.**

The costs of the breeder will be large–in the range of $10 billion to $15 billion— assuming the United States does not change the present policy of developing domestic rather than foreign technology. But this figure is nevertheless comparable to the front end costs of other centralized energy technologies. Cumulative RD&D for light water reactors, for instance, is estimated to have total led $10 billion. Fusion’s costs will be the same or somewhat higher, estimated at $20 billion to $30 billion, including a commercial prototype

Table 17.—Summary Assessment

Technology	Prospective economic-cost range[a] (1 980 $) (mills per kWh)	Relative environmental costs	Scientific	Engineering/ technical	Commercial	Commercial readiness (year)
Satellite power system	80-440	Unknown	Proven[b]	Unproven	—	2005-2015
Solar photovoltaic with storage	65-86	Negligible	Proven	Proven	Unproven	Late 1980's
Solar thermal with storage. . . .	62-89	Negligible	Proven	Proven •	Unproven	Late 1980's
Breeder reactors	58-73	Substantial	Proven	Proven	Proven	*2000*
Fusion	44-75[c]	Moderate-substantial	Unproven	—	—	?
LWR-201O	58	Moderate-substantial	Proven ⁻	Proven	Proven	Operational
LWR-1980	47	—	—	—	—	—

aPlant startinq in 2010.
bEnvironmental impact still unknown, other aspects generallY accepted.
CN.t. this range reflects differences between two studies' estimates (footnotes 10 and 11 On p 120).
aMassive scale-up of known technologies.

SOURCE: OTA working paper.

plant. Fusion and the breeder may thus compete with each other for R&D funds.

The costs of the SPS will be substantially higher than for any of the other options, at an estimated figure of $40 billion to $100 billion.[7] The high number assumes all space development and plant investment costs are allocated to the SPS (see ch. 5), while the lower number assumes the total cost but allocates $60 bill ion to other space programs that could benefit from the same technical capability.

The SPS RD&D cost is so high that commitment to it could foreclose fusion or the breeder. As such, a decision at some point in the future to commit to the SPS would be a decision with potentially far-reaching consequences.

In fact, the SPS is the first proposed energy option whose RD&D costs enter the budgetary range that has previously been limited to very high-technology, high-cost national defense programs such as the MX missile system. That system, as proposed, will cost" $34 billion to $50 billion. Thus, from a policy point of view, the SPS is qualitatively different from any other proposed long-range energy solution.

Institutional Impacts. — Neither fusion nor fission requires much that is new institutionally because their size, health and environ mental impacts, and operating structure are similar to current LWR technology. As technologies used in the centralized mode, the solar technologies will not require different institutional attention than do any other peaking or intermediate plant. As dispersed plants, they are likely to be subject to a much different regulatory regime'[8] and utility structure that encompasses a much broader technological scope than is now the case.

SPS, however, because it is a space system requiring very high capital investment, would likely involve an institutional structure very unlike those in use today in the utility industry (see ch. 9). The main point is that the utilities are unlikely to want to invest directly in satellites, or perhaps even rectennas. It will create far fewer regulatory and capital problems for the utilities for them to buy power from a single SPS corporation and incorporate It directly into their grid. A national SPS monopoly would necessarily be federally, as well as internationally regulated (see ch. 7).

National Security Risks. – Both of the nuclear technologies u rider consideration (breeders and fusion) can be used to generate weapons material and therefore they carry some risk of increasing nuclear weapons proliferation. The terrestrial solar technologies

[17]OTA Workshop on Technical Optlons, December 1979

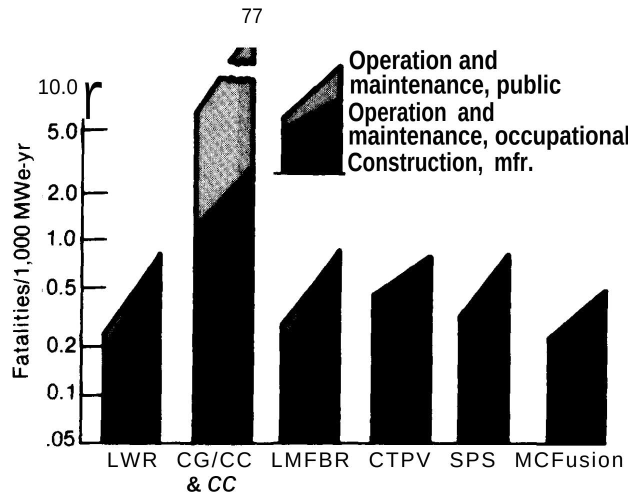
"Office of Technology Assessment, U.S. Congress, "Decentralized Electric Energy Generation Systems," upcoming report, fal I 1981 seem to have purely beneficial national security effects, however. They can be exported and used around the world for peaceful purposes. Because they would be used in relatively small units, they would be much less vulnerable than any larger unit and less of a military risk for a country selling the technology.

SPS would have indirect military potential, largely from the technology that would be developed for space transportation and space construction. However, the system itself would serve as a poor weapon. The question of vulnerability of an SPS system to nuclear or other attack is a different issue. On the whole it is little more vulnerable than any of the larger terrestrial electricity options (see ch. 7).

Economic Risks of RD&D Failure.— In general, the risks of failure are tied directly to the opportunity costs for the different central electric technologies. Therefore, the risks are higher for fusion and SPS than for any of the others. However, the financial risks of failure may be mitigated if some of the RD&D costs are recoverable for other uses. For example, the space spinoffs from developing the SPS could be significant (an upgraded shuttle, space platform technology, an orbital transfer vehicle technology, high powered microwave or laser transmission devices), which would reduce the economic risks. Here, as in the strictly research phase of an SPS program, it is very important to be cognizant of other space and energy programs that could benefit from dollars spent on SPS research and vice versa.

Safety and Health Risks. –OTA pursued no independent study of health and safety risks of the five technologies. This assessment has therefore relied on the work of Argonne National Laboratory that was funded by the SPS office of DOE.[19] The reader is referred to its report for a comprehensive treatment of the problem (see also app. D). The Argonne study attempted to quantify risks in terms of the number of fatalities that would occur per year for a specified plant output (see fig. 29). Some of the issues are unquantifiable, and for the

Figure 29.—Quantified Health Effects



SOURCE *Program Assessment Report Statement of Findings,* SPS Concept and Evaluation Program, DOE/ER-0085, 1980.

SPS and fusion, most of the issues are in this category. The difficulty of quantifying issues for SPS and fusion is a function of the uncertainties about the final configuration these technologies will take as well as the lack of experience with them upon which to base estimates of fatalities. This is an area that needs considerable further study, not only for SPS but in every other comparative study of energy technologies. The major needs are to put all the data on as common a basis as possible and to quantify risks where they are currently unquantified (see ch. 8 for a summary of SPS health and safety risks).

Environmental Risks. –As with health and safety risks, OTA attempted no independent analysis and has relied on the comparative assessment study of Argonne National Laboratory.[20] Table 18 summarizes the most important environmental effects for each of the technologies under study, plus coal. The nuclear technologies have been grouped together because their effects are common to all the nuclear technologies.

[19] G R Woodcock, "Solar Power Satellites and the Evolution of Space Technology," presented at A I AA Meeting, May 1980

[20] L J Habegger, J R Gasper, and C D Brown, "Health and Safety Preliminary Comparative Assessment of the SPS and Other Energy Alternatives," DOE report No DOE/ER-0053, April 1980

Table 18.—Major Environmental Risks

Coal	Nuclear	SPS
Air pollution Atmospheric changes (CO_2, particulate) Esthetic deterioration Land use	Catastrophic events Land use Thermal discharge waste disposal	Atmospheric changes Bioeffects from microwaves, lasers, reflected light Electromagnetic disturbance Land use

SOURCE: Office of Technology Assessment

Other factors. – How well would SPS compete with other baseload electric technologies? This question can ultimately be answered only in the context of overall demand for electricity, considerations that are taken up at the end of this chapter. However, if demand for electricity is such that SPS may be needed to supply a portion of that demand, then the competitive position of SPS vis-a-vis the other technologies will depend primarily on its being cost competitive, and presenting comparable health or environmental hazards to the other technologies. Other utility concerns such as its reliability and rated capacity factor have direct and obvious economic impacts that are subsumed in the condition of its being cost competitive. It is too early to tell whether SPS can compete effectively. What is clear, however, is that factors beyond the scope of control of an SPS program may determine more effectively whether SPS is competitive than the important concerns over costs or health and environmental effects. The effects of reduced coal useage are examined below. However, before the United States needs to decide whether it is prudent to continue or expand coal burning (c 2000), it must make a decision about the use of breeder reactors (c 1990). If we institute a strong breeder program, then SPS is less likely to be needed than otherwise, simply because breeders are apparently cheaper to build and operate than the SPS. They have the further competitive advantage that they strongly resemble LWRS, both in operating characteristics and in health, safety and environmental impacts.[21] Thus, utilities are more likely to purchase breeders than to take on a brand new technology whose major resemblance to terrestrial technologies is the fact that it produces electricity. However, perhaps more important is the fact that breeders could play a significant role in supplying electricity 10 to 20 years before the SPS, thus giving them an automatic competitive advantage.

Although the fusion program has not yet proven that it is possible to generate more energy than is fed to the fusion process, the fusion community is confident that the production of electricity from fusion is a matter of continued R&D. The costs are more uncertain than for SPS. However, fusion has a strong followlng inside and outside the fusion community. Furthermore, the utilities are already actively pursuing fusion studies. Therefore, if fusion's costs turn out to be competitive with SPS, it too may be chosen over SPS because it has a strong following and because beyond the first wall, it is similar to other nuclear options in the way in which it generates electricity. However, it may not be capable of making a significant impact on the supply of electricity until well after SPS, i.e., not until 2030 or later.

Because several proposed versions of the SPS are designed to use PV cells, a terrestrial PV system constitutes an obvious comparison to the SPS. The satellite or SOLARES ground site would receive continuous sunlight. A terrestrial system, however, receives constantly varying sunlight. Table 19 compares the peak and total annual insolation in space, at a SOLARES ground station and in Boston and Phoenix for an optimally tilted flat-plate, nontracking solar collector. Therefore, a terrestrial PV in Phoenix the size of a reference system rectenna would, in theory, be capable of producing as much electricity on a yearly basis as the reference satellite. However, the output of

"E. P Levine, et al , "Comparative Assessment of Environmental Welfare Effects of the Satellite Power Sytem and Other Energy Alternatives, " DOE report No DOE/E R-0055, April 1980

Table 19.—Terrestrial and Space Insolation Compared

	Peak insolation (per square meter)	Average annual insolation (per square meter)	Area needed to produce 1,000 MW (per continuous output on Earth (17-percent efficiency cells)
Space	1.3 kW	11,800 kWh	10 km^2
SOLARES GND Station (29° latitude)	1.3 kW	9,734 kWh	6 km^2
Boston	0.8 kW	1,430 kWh	44 km^2
Phoenix	1.0 kW	2,410 kWh	26 km^2
Equivalent rectenna area for reference system—35° latitude	—	—	28 km^2

SOURCE: Office of Technology Assessment,

such a central terrestrial system would be subject to short-term and seasonal variations in output due to fluctuations in insolation brought out by cloud cover. This effect is illustrated in table 20 for the Boston and Phoenix areas. The daily insolation for the month of December is 28-percent less than for the average month, resulting in 28- percent less PV output for the same sized array. Phoenix, by contrast, experiences average insolation values only 14 percent lower than the average in July, its month of lowest insolation.

Decentralized Electrical Generation

Although technologies that are capable of producing electricity in a dispersed mode may not be direct competitors of centralized technologies, they will compete for a percentage share of overall electricity supply in this country and the world. In 1977, the residential sector of the electrical market constituted 36 percent of this Nation's demand for electricity. If a significant portion of this demand as well as part of the demand for commercial and industrial consumption can be met by dispersed technologies such as solar PV, wind, and biomass at costs that are competitive with centralized electricity, then the demand for centrally produced electricity will drop. Low demand for centrally produced electricity will in turn reduce the need for new, large-scale generating technologies and place them in a poor competitive position with respect to proven technologies. Thus, it is of considerable interest to investigate the role that dispersed electrical technologies may play in the Nation's energy future.

Dispersed modes of generating electricity are first and foremost attractive in remote regions where the electricity grid has not yet penetrated. It is in these areas where windmills and PV, with storage, are now being installed even though their cost is high relative to the price of grid-supplied electricity.

As experience with these technologies grows, and their price decreases due to deeper market penetration and increased commensurate production, they are likely to penetrate areas that are now served by the utilities. Such a shift will be aided by the Public Utilities Regulatory Policies Act of 1978 (PURPA) that requires utilities to purchase electricity from renewable-based powerplants at their avoided cost of power. To date, State regulatory commissions have established prices that are equal to, or higher than, the retail price of electricity. If this practice should continue into the mid-1980's, onsite electrical generating systems will not only provide energy for their owner's use, but will become income generators as well.

This shift will be further aided by the attractiveness of modular units that allow a homeowner or community to become relatively self-reliant and independent of large-scale generating systems over which they have little control. Additionally, onsite systems can be erected rapidly and incrementally, allowing a close match of supply to local demand. Under such conditions, it can be expected that there would be a rapid increase in demand for small-scale systems.

The role of dispersed electrical generating technology in the Nation's electrical supply is

Table 20.—Terrestrial Insolation at Different Latitudes and Climates

Boston: Latitude 42.2

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
kWh/m²	3.4	3.7	4.1	4.0	4.4	4.3	4.6	4.4	4.4	4.1	3.0	2.8
kWh/m²/month	104	104	126	119	135	129	142	137	131	126	90	85

Total insolation per year 1,430 kWh/m²
Average daily insolation: 3.9 kWh/m²

Phoenix: Latitude 33.3

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
kWh/m²/day	6.0	7.0	7.4	7.5	6.6	6.2	5.7	6.2	7.0	7.3	6.7	6.0
kWh/m²/month	184	195	228	225	204	186	178	185	218	227	200	185

Total insolation per year 2,414 kWh/m²
Average daily insolation: 6.6 kWh/m²

SOURCE: *Solar Photovoltaics: Applications Seminar,* Planning Research Co

the subject of another OTA study that will discuss the full array of dispersed electrical technologies: wind, PV, and biomass. However, because much of the technology for constructing space-rated solar cells will be applicable to terrestrial applications and vice versa, this report explores the possible role of dispersed PV systems in filling part of this country's electrical needs in the time frame of the SPS.

Dispersed Photovoltaic Systems.—The most important single characteristic that makes PV of considerable interest for dispersed uses is their relative insensitivity to economies of scale for generating electricity because PV are modular, allowing considerable flexibility in their location. Economies of scale are very important in their production, however. The present high cost of PV (about $7/peak watt) is largely due to a very small production capacity. About 4.5 MW (peak) of terrestrial capacity were produced globally in 1979, by only a dozen manufacturers. Demand exceeds supply, however, even at $7/peak watt and thus the market will surely expand, especially as new manufacturing techniques allowing cheaper PV are developed. All indications are that continued reduction in price in line with DOE cost goals will accelerate the demand for PV cells for all applications and in particular for dispersed systems that are either connected to the utility grid or stand alone. Meeting this cost goal is important for the SPS, which in the reference design, is highly sensitive to PV array costs (34 percent of satellite costs).

The total penetration of PV and other decentralized energy technologies into the residential, commercial, and industrial sectors of the energy economy will depend on a number of interrelated factors in addition to cost. The following summary indicates the most important ones.

- *Average Available Sunlight.* –The best areas for dispersed PV are the same ones where centralized applications are most plausible, i.e., in desert climates such as the Southwestern United States. However, the variation of regional average insolation across the continental United States is less than a factor of two. Changes from year to year are considerably less. Both effects are smaller than variations in energy consumption and price patterns. Thus, regional or annual insolation variations are not likely to be a strong determinant of PV penetration. This will be even more true in areas where biomass and wind systems can work in complementary fashion with PVs.

- *Storage.* – Advances in storage technology could have a significant effect on the market penetration of PV systems, particularly for remote and stand-alone applications. It is generally agreed, however, that low-priced storage, if it is ever developed, is a decade or two away.
- The Use of Centralized Photovoltaic Sys*tems.* — Using PV for peaking or intermediate generating capacity will enhance the development of low-priced PV cells and the auxillary equipment (mounting panels, inverters, etc.) and speed the introduction of dispersed PV systems to marginal areas (i. e., areas where the centrally generated electricity is cheaper than onsite generation).
- *Conservation.* –Conservation has already resulted in important reductions in per-capita energy use. In the Washington, D. C., area for example, use of electricity is increasing by only 1.4 percent a year,[22] a sharp contrast to the 7 percent yearly increase in consumption that was common in the mid-1 970's. Continued price increases for energy will increase the desire to conserve energy and make the total needs of a residence, for instance, much less. The Virginia Electric Power Co., for example, reports that in its service area all-electric homes, used about 24 MWhr/yr in the mid-1 970's, but consumed only 19 MWhr for 1979,[23] a 20-percent drop. Decreases in total consumption make it more likely that PV systems can be sized to meet the needs of the residential sector.
- *Other Dispersed Sources of Electrical Power.* –The acceptance of wind and biomass for dispersed electrical generation, or as substitutes for electricity, may enhance the desire for photovoltaics as individuals and the utilities become accustomed to working with dispersed sources. However, if other sources failed to make a significant impact because they were expensive or because they didn't work well, they could have the opposite effect on the use of PVs.
- Cost of Photovoltaics. – Single-crystal silicon cells are highly energy intensive. Thus, the energy cost of producing them is high, and if energy prices increase, the cost of the cells will be higher than the DOE goals. New production techniques for amorphous silicon or other materials, however, may lead to less energy intensive cells, and the problem could be avoided.
- *Reliability y.* –One of the major reasons for preferring centralized power generation is the high reliability of electrical service. Dispersed systems must be reliable in order to capture a significant portion of the electricity market. The PV themselves are extremely reliable. However, the associated equipment is subject to a higher failure rate. Market penetration will therefore depend on a highly reliable product and effective, timely service to repair failures.
- *Institutional Effects.* – PURPA regulations will enhance the use of dispersed-systems. If these regulations are retained and if they are carried out effectively on the local level, then they will be effective in speeding the introduction of dispersed electrical capacity. However, a number of negative effects (e. g., low reliability, high costs, etc.), could cause such regulations to be repealed if they are found to work inefficiently.

In summary, it can be said that the future of dispersed electric systems, and PV in particular, is subject to considerable uncertainty. If cost goals are met, and the effect of the other factors is positive overall, then dispersed electrical systems could make a significant contribution especially in a future in which the demand for electricity is relatively low. As table 21 illustrates, the cost per kilowatt-hour for grid-connected PV systems, though subject to considerable uncertainty, is competitive with baseload systems. By combining several different kinds of dispersed sources of electricity (e.g., wind, PV, and biomass), the prospects for dispersed PV sales becomes even

[22] *Washington Post,* Mar 25, 1981, P D-9
[23] Washington Post, June 23,1980, p B-1

Table 21.–Costs of Onsite Photovoltaics (1980¢/kWh)

	Household				Industry			
	Without storage		With storage*		Without storage		With storage*	
	Boston	Phoenix	Boston	Phoenix	Boston	Phoenix	Boston	Phoenix
Roof replacement.	3.0¢	1.8¢	9.0¢	7.O¢	—	—	—	—
Flat on roof	3.9¢	2.3¢	9.9¢	7.6¢	—	—	—	—
Columns on roof or ground	8.3¢	4.9¢	14.7¢	10.4¢	8.O¢	4.7¢	18.9¢	12.9¢

NOTE: These costs were developed assuming photovoltaic arrays costing $35/m² and 17-percent efficiency in space (18 percent on ground). Further details of the assumed systems can be found in app. B.
● Assumed a 80-percent capacity backup generator,

SOURCE: Office of Technology Assessment.

stronger than when used alone. As in the case of the baseload technologies, these figures must be seen as indicative of the range of costs that may be attained and should not be used as a basis for comparison at this time. Considerable development will be needed to determine whether the various cost goals can be met.

Implications

Introduction

The discussions just completed illustrate that the future of the SPS, assuming it can be developed technically, depends on a variety of factors. These include the future demand for electricity and how SPS compares with other supply technologies. There are two questions to be answered: 1) is the SPS necessary at all? 2) if so, when do we need it? The section on demand showed that future electricity needs are highly uncertain and are dependent on technological developments that can profoundly influence the costs of various end use technologies. The section on supply contained discussion of several technologies that would compete, partially or completely, with the SPS to supply electricity for the long term. The section gave criteria for choosing between these technologies and the range of uncertainty about their potential success. From the discussion it is clear that a variety of factors beyond purely technical success will determine which supply technology(ies) will emerge.

To see this more clearly, OTA chose three hypothetical U.S. energy futures in order to examine possible future supply mixes. They were chosen to span a wide range of possible electricity demand scenarios for 2030. The lowest assumes no change from our present end-use demand for electricity, the highest uses the 1979 Energy Information Administration (E IA) high projection for 2020 extrapolated to 2030, and the mid-level is halfway between. These futures were chosen as an exercise to illustrate the way various technologies might be used and the constraints placed on this selection. OTA does not treat these demand levels as forecasts of what will occur, but as a plausible range of future end-use demand.

The extremes of the three scenarios are characterized by zero growth in electricity demand for the low scenario, and an average growth of 2.8 percent per year for the high scenario from 1980 to 2030. The growth in the high scenario is not steady, however, but starts at 4.1 percent in the 1980-95 time period, and declines to 1.9 percent by the end of the scenario in 2030.

The low scenario represents a conservation-oriented energy strategy, in which the increases in industrial output and residential and commercial space are offset by improved efficiency of electricity use for industrial processes and drives, and residential and commercial heating, air conditioning, lighting, and appliances. The end-use electricity level in the low scenario, taken from the CONAES scenario A, assumed electricity demand at a constant level of 7.4 Quads for 1980 to 2010, and extrapolated the same constant level to 2030. That level is very close to the actual end-use electrical consumption in 1979 which was 7.6 Quads. The total primary energy consumption in the CONAES scenario A is 74 Quads, compared to actual use in 1979 of 78.9 Quads.[24]

The high scenario represents a major expansion of the use of electricity in all sectors. The scenario is taken from the E1A Series C projection from the Long-Term Energy Analysis Program. The total primary energy use in this scenario is 169 Quads. The scenario projects a major shift in residential fuel use, with electricity supplying 60 percent of all residential needs and 55 percent of residential heating. (Water and space heating alone are projected at 8 Quads end-use electricity in 2020.) Electricity is expected to provide 70 percent of the commercial energy demand in 2020. In this project ion, EIA forecasts that the industrial sector will grow faster than any other sector, and that industrial use of electricity will triple or quadruple by 2020. Total energy use in the industrial sector in the scenario is 63 Quads in 2020. Electricity's share of the industrial energy sector rises from 11 to 20 percent. The dominant supply technologies in the scenario are coal and nuclear, with coal providing 60 percent, nuclear 33 percent, and hydro and other renewable the remainder. The E 1A scenario was extrapolated to 2030, using the same electric growth rate as assumed in 2010 to 2020, namely 1.9 percent. According to the extrapolation of this scenario, the total energy use in 2030 is 196 quads and the total electricity use is 30.2 Quads (end use).

The middle scenario is chosen to be the midpoint between the high and low scenarios at each of the decades projected. The end-use figures for each of these three scenarios are given in table 22.

OTA does not suggest these demand levels as forecasts of what will occur. These futures were chosen to illustrate the way various technologies might be used and the constraints that might be placed on their selection.

To characterize the mix of supply technologies possible under these scenarios, a number of questions was addressed. Among these questions were the numbers and kinds of technologies that would contribute to the supply mix under the various scenarios, the maximum reasonable SPS contribution under each scenario, the most likely technologies to replace SPS were it not deployed, and the relative implementation rates of the various technologies under different demand conditions. The exercise carried the simplifying assumption that one technology could be substituted for another These questions cannot be answered precisely, but their discussion leads to interesting insights into the potential role of SPS.

[24] *Energy in Transition*, op c I t

Table 22.—Range of Energy Demand in 2030

Scenario	End-use electrical (Quads)	Primary total energy (Quads)
High	30.2	196
Mid	18.8	135
Low	7.4	74

SOURCE: Office of Technology Assessment.

Low-Demand Future

For this case, end-use energy demand for electricity is selected to be 7.5 Quads (today's level). A zero electric growth future is likely to be the result of substantial conservation – probably resulting from high energy prices — and the failure to develop end-use technologies that use electricity at a lower net cost than technologies using liquid or gaseous fuels and direct solar. The principal feature of this future is that electricity demand can be satisfied without SPS, fusion or breeder reactors. The supply potential of coal, hydro, ground based solar (including wind) and conventional nuclear would be more than sufficient to meet demand Even if coal were to be phased out due to negative findings about the CO, buildup, its share could probably be absorbed by other sources. Zero growth in electricity demand gives the nation considerable time for developing new technologies.

In this situation utilities would only need to replace retiring plants. Therefore they would have considerable latitude in choosing technologies. Further, a zero growth rate would not favor large plants because they would add too much capacity at one time. Therefore, small-scale, dispersed technologies may play a major role in this future. If any of the new technologies under discussion are introduced they will have to appear in relatively small increments in order to maintain system reliability. For example, one could expect SPS to provide no more than 1 to 2 Quads at any given time to the 7 to 8 Quad total. This would act strongly against an SPS the size of the reference system since it would only require 7 to 15 units of 5,000 MW at a 90-percent capacity factor to supply this much energy. Therefore deployment of any SPS would depend on an international demand for electricity and/or the development of much smaller units than the reference system (perhaps on the order of 500 MW). A similar argument could be made about fusion and breeder reactors, although current development plans show the size of eventual commercial plants to be 1,000 MW or less.

In summary, a scenario that shows little or no increase in electricity demand for the next several decades does not appear to be attractive for accelerated SPS development, particularly of the reference system. At the same time, development of other central, baseload supply options ultimately competing with SPS could also be slowed. The choice among these, if needed at all, would primarily depend on which ones could most economically be developed in smaller sizes.

Middle Demand Future

In this case net electricity demand reaches about 20 Quads in 2030 representing about a 2-percent growth rate per year that is close to that which the Nation is now experiencing. Although this is about 2.5 times current electric energy demand, it too could be met without using the SPS, fusion or the breeder reactor. For example if two-thirds of the 20 Quads were produced by coal, it would require a tripling of present yearly production, which is within the Nation's capability. Current estimates of domestic uranium reserves are sufficient to supply another 6 Quads in 2030. In addition, a major contribution from terrestrial solar (wind, onsite PV) can be expected to help meet increased intermediate and peak load demands that coincide with solar peaks (space heating and cooling). If growth continues past 2030, this mix may be insufficient. Yearly coal production could probably not be expanded too much beyond this (tripled) level without straining other sectors of the economy, and by 2030 the Nation may be near its uranium resource limits. Therefore, to ensure supply beyond 2030 and to replace retiring nuclear plants, some level of new, centralized technologies would probably be needed.

If coal and conventional nuclear remain acceptable, it is not likely that all three of the major centralized technologies under development would be needed. The contribution they could make by 2030 would be small because of the time needed to bring them on line and the fact that they would be starting from a zero base sometime near 2010. A 10-percent contribution to 20 Quads would require anywhere from 60,000 to 100,000 GW, depending on capacity factor. Unlike the low demand future, this would allow SPS units of up to 5,000-MW size to be added if continued growth past 2030 is expected. A 2 percent per year growth rate means about 0.4 Quad/yr added at that time. This could be supplied by three SPS plants per year at the reference design size, in addition to baseload units to replace retired plants. This is still a small enough increment that smaller SPS plants appear to have an advantage. In addition, this demand increment is still not too large to rule out its being met by onsite solar, wind, and centralized solar. All have much lower energy densities than fusion or breeders, however, and eventually their contribution will be limited by available area. About 25 m^2 are required to supply a continuous kilowatt of solar electricity assuming PV conversion efficiencies of 20 percent. The entire 0.4 Quad could be supplied by about 125 mi^2, not an unreasonable area.

If coal is not acceptable because of $C0_2$ then there will have to be a substantially larger contribution by the newer technologies. In this case it is plausible that all three, plus substantial ground based solar, would be needed. Such a replacement could be achieved with these new technologies but it would be a sizable effort. If coal supplied just half of the electricity in the case discussed above, about

10 Quads of new electric energy would have to be found, requiring 300 to 500 GW. If new plants were on the order of 1,000 MW in size, a construction rate of 15 to 25 per year would be needed assuming they were first available in 2010. Under this future of constrained coal, then, there would appear to be sufficient demand for al I technologies to be introduced at a rate that would pay for their development in a reasonable period. Also, it is not likely that any one technology would be relied upon to supply the entire 10 Quads at the end of this 20 year phasing-in period. An even three-way split, for example, would mean that SPS would supply about 100 to 150 GW by 2030.

High-Demand Scenario

This future assumes a final demand for electricity of 30 Quads (about four times the current level), meaning a growth rate of about 2.8 percent per year. At that rate, about 0.8 Quad/yr would be added in 2030. If one assumes an increase in net conversion efficiency from today's 29 to about 35 percent and an increase in capacity factor from 42 to 55 percent, then this total demand could be met by an installed generating capacity about three times today's figure. Efficiency and capacity factor will almost certainly have to increase if a 30-Quad demand is to be met. Total system capacity would be in the range of 1,200 to 2,400 GW (1,800 GW at 55-percent capacity factor).

To be able to supply this much electric energy, all technologies would probably be needed. Further, larger plants are likely since a demand increment of 0.8 Quad/yr would require about 40,000 to 50,000 MW of new capacity per year. Therefore, addition of plants ranging from 1,000 to 5,000 MW would not cause any significant short- or long-run overcapacity problems. Because of the large amount of capacity needed, conventional nuclear and coal will probably be able to supply only about two-thirds of the total (i. e., about 1,200 GW) before they reach the limits discussed above. Thus, about 600 GW must be supplied by hydro, ground based solar, geothermal, and some combination of SPS, fusion and breeders. Breeders are likely to supply the bulk of this by 2030, provided they are acceptable, since they are the closest to commercial readiness. Even so, as much as 200 GW of SPS could be needed by 2030. The SPS development would have to be accelerated if it is to meet a goal like this. The same holds true for fusion, which could also be required to supply around 100 GW by 2030.

The mix of technologies will be determined substantially by constraints such as environmental concerns, capital, land and water availability, materials limitations and labor requirements. For example, limited water would favor SPS and ground-based solar PV. Limited capital, however, would favor the least capital-intensive technologies such as coal and act against the SPS. In any event, these constraints will be very important at this demand level because of the large number of powerplants needed

If coal must be phased down or eliminated then even larger demands will be put on the new technologies. For example, if coal and conventional nuclear could only meet one-third of the demand, an additional 600 GW of capacity would be needed. In this case it is probable that an all-out breeder program would be needed. This should not affect the SPS– in fact, more satellites may be needed– but it could actually reduce fusion's contribution since it is a competing nuclear technology. The terrestrial, onsite solar contribution will have to be large in either case but is very unlikely to be able to supply even one-half of the 30 Quads. Even 20 percent of the demand would require a very large deployment of PV systems — nearly 400 GW of dispersed generating capacity.

Conclusion

The size of future electric demand will be the major determinant in the amount of SPS capacity installed, assuming successful development and competitive price. Table 23 shows estimates of the upper range of SPS capacity available for each future for the case of full coal development and coal phaseout.

Table 23.–Upper Range of SPS Use (in GW)

Future (Quads)	With coal	Without coal
7.5	0	0-30
20.0	0-60	100-200
30.0	100-200	100-200

SOURCE: Office of Technology Assessment

In addition to determining the upper range of the contribution of SPS the demand level and rate of growth will also determine the preferred unit size. For the low scenario, smaller plants would be preferred since overcapacity problems caused by adding too much at once would probably more than offset gains made by any economy of scale. For the upper future, however, for even the largest SPS proposed plant size, it is unlikely that too much can be added at once for any reasonable construction schedule. The mid scenario, however, gives somewhat ambiguous results, although the smaller size SPS systems appear generally to be more desirable.

For the first two scenarios it is unlikely that all three major, centralized supply technologies will be needed simultaneously, even if coal cannot be used. Onsite, dispersed solar will be able to make up a larger percentage of the needed capacity and could eliminate the need for any new centralized technology in the low demand case. In all cases, coal can be the dominant source and continue in that role for several years past 2030. Finally, as the demand for electricity increases, decisions about capacity mix will become more and more dependent on physical and labor constraints because of the sheer size of the capacity requirements.

THE EFFECTS OF SPS ON CIVILIAN SPACE POLICY AND PROGRAMS

The effects of SPS development on the U.S. civilian space program would be great, though their precise type and magnitude would depend on the kind of SPS built, the overall speed of the development program and the status of space capabilities at the time. An SPS program would stimulate more rapid development of space transportation, large-structure assembly and manned-mission capabilities, and automated operations. SPS development would also have a bearing on national space policy and institutional structures, both Government and private sector. The following discussion will examine four areas: 1) space policy, 2) current and future space projects, 3) institutional structures, and 4) indirect effects and "spin offs."

Space Policy

The Nation's space policy is a reflection of broad national goals. The principles guiding the U.S. civilian program were first enunciated in the 1958 National Aeronautics and Space Act, and have been periodically reaffirmed with minor modification and changes of emphasis. The 1958 Act states that "activities in space should be devoted to peaceful purposes for the benefit of all mankind," to promote the "general welfare and security of the United States " The Act specifies that civilian activities shall be directed by NASA, and military/defense operations by the Department of Defense. The specific aims of the space program include: expansion of knowledge, improvement of space transportation, "the preservation of the role of the United States as a leader in aeronautical and space sciences," and cooperation with other nations. NASA was established to "plan, direct and conduct aeronautical and space activities. "[25]

These general goals and this framework have been reaffirmed subsequently, most recently in the "Directive on National Space Policy" and the "White House Fact Sheet on

[25] "Natlonal Aeronautics and Space Act of 1958, as Amended, " in *Space Law, Selected Basic Documents,* Senate Committee on Commerce, Science, and Transportation; U.S. Government Printing Of flee, 1978; pp 499-503

U.S. Civil Space Policy," both issued in 1978. In these documents the Carter administration committed the United States to increase scientific knowledge, develop useful commercial and Government applications of space technology, and "maintain United States leadership in space technology. " Establishing and maintaining satisfactory relations between the civil and military programs was recognized as a priority issue, and the National Security Council was charged with providing coordination for all Federal agencies involved in space. Cooperation with other nations, including joint programs and the development of a stable legal regime allowing all nations to use outerspace for peaceful purposes, were emphasized as important goals. The investment and direct participation of the private sector in space activities was addressed in the context of remote-sensing systems. NASA's responsibilities for the operation, as opposed to research, development, and testing, of applications systems have yet to be clarified.26

The U.S. civil space program can thus be said to have an ongoing set of policy goals:

- scientific — increasing knowledge,
- political — maintaining U.S. preeminence, and
- economic–developing useful commercial applications.

It also has a continuing policy framework:

- separation of civil and military programs (with various mechanisms for coordinating different efforts),
- cooperation with foreign countries and agencies, and
- separation of NASA R&D and prototype development programs from commercial applications (an unclear relationship).

Would an SPS program alter the basic thrust of U.S. policy? I n terms of goals, an SPS program would be primarily an applications effort for commercial purposes, and hence would further the economic goals that have been emphasized in recent policy proclamations.

The political end of U.S. preeminence in space, though no longer stressed as strongly as during the Apollo program, would also be served by commitment to an SPS. (This assumes that the project would be successful; failure of such a high-visibility effort could be extremely damaging to U.S. prestige. International cooperation might tend to mitigate this danger.)

The SPS program would not be focused on increasing basic scientific knowledge, but much of the research and experimentation required would provide some scientific gains; in addition, the infrastructure for SPS (e. g., platforms, transportation vehicles) could be used for a multitude of scientific projects in space. There is some danger, though, that focusing the national space program on such a major applications project as SPS would divert resources and attention, at least temporarily, from scientific missions.

The effects of SPS on the U.S. policy framework will depend on how it is financed and managed. Civil-military relations could be altered. Although the SPS is not technically suited to be used as a weapons system, much of SPS technology and infrastructure, especially the transportation vehicles, would have military uses (see ch. 4). Furthermore, it is unlikely that a project with the scope and impact of SPS could be approved by Congress without at least the tacit consent of the Department of Defense (DOD). In the foreseeable future, DOD requirements for aerospace expertise and facilities will be great, and SPS may be seen as a competitor for scarce resources unless direct defense benefits can be realized. Although an SPS program would not be run by the military, it might be necessary for the civil and military sectors to be more closely coordinated than has previously been the case.

Foreign cooperation and joint ventures might be encouraged not only by the desire to improve international relations but by more direct economic considerations. (see ch. 7). These considerations would be strong enough

[26]" Description of a Presidential Directive on National Space Policy, June 20, 1978, " and "White House Fact Sheet, U.S. Civil Space Policy, Oct 11, 1978," in *Space* law, pp 558-564.

to provide for a greater degree of shared responsibility than in any equivalent U.S. program to date, unless U.S. military involvement proves an insuperable obstacle. International participation might be such that the project could no longer be run as a U.S. venture with limited foreign cooperation, but would become a truly multinational effort with no dominant U.S. role.

The relation between public and private participants would be a major issue in any SPS program. Policy in this area has not been clearly established, though there is precedent for detaching applications projects, such as satellite communications and Landsat, from NASA after development is completed. NASA has conducted all U.S. civilian launches on a reimbursable basis; it is unclear what would happen if private firms wished to build and/or launch their own vehicles, as has been suggested for the shuttle. If, as is presently the case, a Federal SPS program were managed by DOE or some other agency besides NASA, NASA might be responsible for only a limited part of SPS development and NASA restrictions and policies might not apply.

Current and Projected Space Projects

SPS would be strongly affected by current space programs and capabilities, and in turn might also determine what many of those programs would be. However, since an SPS development decision is unlikely to be made before 1990, and may not be possible until 2000, (see ch. 4), SPS will not shape NASA projects conducted during the next decade (though it may affect long-range planning).

Historically, NASA has devoted the major portion of its resources to a single major project, first the Apollo lunar-landing program, and then the Space Shuttle. However, there are currently no plans for a similar "centerpiece" project to follow the Shuttle; the White House Fact Sheet asserted explicitly that: "it is neither feasible nor necessary at this time to commit the United States to a high-challenge space engineering initiative comparable to Apollo." Instead, present plans call for a number of smaller scale operations and scientific missions centered around use of the Shuttle and other components of the Space Transportation System (STS). The lack of a single, clear, overriding project goal for the civilian space program has been criticized for squandering NASA and contractor capabilities, and leaving the United States without a visionary and profitable use for the new transportation capabilities under development. This problem will undoubtedly be addressed during the 1980's, but jurisdictional and philosophical differences, as well as budgetary constraints, may make consensus difficult to achieve.

For the next 5 years, NASA plans to concentrate on a number of areas: those most directly relevant to SPS include:

1. Transportation and Orbital Operations: Transportation efforts will concentrate on meeting shuttle schedules but also include other elements of STS: the inertial upper stage, for placing payloads in geosynchronous orbit (CEO) (under development by the Air Force); Spacelab, for manned and unmanned experimentation (joint program with ESA); development of orbital transfer vehicles such as an electric orbit transfer vehicle (EOTV); systems to handle payloads outside of the Shuttle; and free-flying platforms. Each of these programs will be important for improving our capability to move and work in space, and hence directly relevant to SPS. The key element is the Shuttle, which must work and work well if these projects are to proceed during the 1980's. Delays in Shuttle operations, or in building additional orbiters, will not only retard these projects but also might prevent SPS-specific research flights as envisioned in one of the policy Options from taking place in the late 1980's (see ch. 4).
2. *Immediate Applications:* In this area, space processing experiments to be conducted on Spacelab could be important in determining the proper kinds of materials for SPS construction, as well as prospects for direct processing of raw materials in orbit. Communications and remote-se ns-

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ing development will involve work with microwave transmission, lasers, and mirror systems, as well as detailed studies of the upper atmosphere,[27] which will be vital in determining the environmental effects of launch effluents and energy transmission beams.

3. Solar Radiation: The Solar Maximum Mission (launched February 1980) and the upcoming International Solar Polar Mission, scheduled for 1983, will study solar radiation and its effects on the near-Earth space environment. Such information could be important in designing SPS solar cells and in adding to our knowledge of the effects of radiation on SPS workers: ionizing radiation in CEO is a potentially serious obstacle to human effectiveness and could be decisive in determining the optimal "mix" between automated and human-controlled operations.
4. Humans in Space: The studies of Shuttle crew performance as well as specific Spacelab experiments will provide a basis for determining the long-term effects of weightlessness and cramped quarters, and for designing appropriate equipment to improve manned performance. 28

The above projects are already underway and are those for which funding or explicit planning are in place. NASA has also outlined other, longer term plans that would be important to SPS. NASA's Office of Space Transportation Systems' long-term goals are predicated on the assumption that "the growth of U.S. civilian space programs in the 1990's will probably continue to be moderate and evolutionary, rather than rapid or 'Apollo-like,' " and that "space projects will increasingly have to demonstrate significant economic return or perform essential services to obtain approval." The specific goals are: 1) routine operation of the STS by the mid-1 980's; 2) routine operation of unmanned large low-Earth orbit (LEO) platforms by the mid-1980's; 3) a permanent manned facility in LEO for research, construction, and operations, by the end of the 1980's; and 4) a permanent facility in GEO, eventually manned, by the late 1990's. Meeting goals would involve:

- augmenting the Shuttle's thrust, perhaps via a liquid booster;
- developing EOTVs, such as the low-thrust ion-propelled Solar Electric Propulsion System (SEPS) for service to geosynchronous orbit;
- equipping the Shuttle and its modules with a 25-kW add-on electrical power system; and
- carrying-on a ground and space-based effort to fabricate and assemble precision structures in orbit .29

All of these projects could have direct bearing on SPS and on any future decision to proceed with SPS development. Some of the longer term aims, such as SEPs, might overlap with an SPS development program, that would provide a strong impetus for their completion.

NASA is not the only body with plans for space. DOD goals, though largely classified, include large platforms, orbital microwave radars, and space-based lasers. DOD requirements could drive NASA projects such as Shuttle thrust augmentation, or lead to separate development of SPS-useful equipment.

Other long-range projects have been suggested by many individuals and organizations, in and out of government. In the transportation area, these include very large fully reuseable launchers; laser-propulsion; 30 lightsails, to power low-acceleration transfer vehicles or deep-space missions; [31] and mass-drivers to lift material off the lunar surface, or as a solar-powered propulsion system for space vehicles.[32] Other than the building of full-scale permanent colonies, SPS is the largest space project proposed to date, in

[27] National Aeronautics and Space Administration, *NASA Program Plan, Fiscal Years 1987 Through 1985*, 1980, p 107

[28] Ibid, pp 3-5

[29] Ibid pp 190-205

[30] A Hertzberg, K Sun, W Jones, "Laser Aircraft, *Astronautics and Aeronautics*, March 1979 p 41

[31] K Eric Drexler, "Spinoffs To and From SPS Technology: A Preliminary Assessment," OTA Working Paper, June 1980, p. 9

[32] G. O'Neill, G Driggers, B. O'Leary, "New Routes to Manufacturing in Space," *Astronautics and Aeronautics*, October 1980 pp 4651

terms of expense, returns, timeframe, and amount of people and materials placed in orbit; if developed it would be a spur to all forms of cheaper space transportation.

SPS's effect on space projects would depend to some extent on the type of SPS that would be developed, the size of each unit, and the size of the entire system (as well as the scope and type of space program in place at the time). A geosynchronous microwave SPS similar to the reference design would require extensive transfer vehicle capacity and hence lead to accelerated development of EOTVs, chemical-powered personnel vehicles, and manned GEO construction stations. A laser-SPS in LEO, on the other hand, would require relatively little LEO to GEO transfer capacity. A mirror-system might need even less upgraded lift or construction capacity in order to be fully deployed (see ch. 5).

A large SPS system consisting of many satellites would tend to have greater economies of scale, leading to the development of more and different sorts of vehicles, and greater mass-production and automation. In-orbit processing of lunar or asteroidal raw materials would also be feasible only if a very large system were built, to justify the front-end costs of lunar mining and orbital processors.

Institutional Structures

Would an SPS program require a change in current national institutions? The completed SPS Concept Development and Evaluation Program[33] was a joint DOE/NASA effort, with DOE providing most of the management and NASA providing technical support. A decision to have further SPS research, development, and demonstration efforts managed by DOE would likely prove awkward, since the bulk of the up-front development costs would be for space systems; hence DOE would have to pass most of its SPS funding to NASA, or attempt to develop its own contractor relations and in-house space capability, which would be time-consuming and wasteful. SPS would require a much clearer and stronger coordinating mechanism than currently exists for national space programs, since not only NASA and DOE but a number of other departments and agencies would be involved. 34

Extensive NASA involvement in SPS would require clarification of NASA's appropriate role in commercial applications ventures, and perhaps modification of NASA's charter. Both underlying policy— i.e., to what extent NASA should operate applications systems, such as Landsat and communication satellites—and specific procedures for turning over patents, technology, and hardware to private industry or other Government agencies, have been subject to continuing controversy. *

It is probable that a separate public or quasi-governmental body would eventually be set up, outside of NASA and DOE, to manage an SPS program. Such a decision would be influenced by, among other things, the desired mix of public and private funding, and the degree of international involvement. Possible forms such a body might take are discussed in chapter 9, Financing Ownership and Control, and in chapter 7.

Indirect Effects and "Spinoffs"

There would be three kinds of indirect effects of SPS development:

- technology and hardware developed for SPS that could have other uses (and that otherwise would not be developed or would be developed at a much slower pace),
- uses of the SPS itself other than providing terrestrial baseload electric power (and that would otherwise not be provided for), and
- economic/technological changes and basic shifts of national attitudes

SPS developed technologies and hardware: Most, though not all, of these spinoffs would relate to space capabilities. We have already

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[33] "Satellite Power Systems Concept Development and Evaluation Program, "Program Assessment Report Statement of Findings," November 1980, DO E/E R-0085

[34] DOE report on SPS and Government agencies – in press

*See OTA assessment, *Space Policy and Applications*, in preparation seen that NASA's transportation plans include many elements directly useful to SPS, which SPS development would tend to accelerate or modify. Although the reference system calls for heavy-lift launch vehicles able to carry 400 tons to LEO, and a 5,000-ton payload EOTV, the exact types of vehicles needed cannot yet be specified. The proper mix between size, numbers, and types of vehicles depends on many unknown factors, including the type of system, its location, and the number of satellites to be built.

The combination of improved and cheaper transportation, robotics and teleoperation, possible new construction materials (such as graphite composites), and human expertise, would make possible many commercial space activities. Large communications platforms, scientific and industrial research facilities, processing plants for chemical and raw materials —these are a few possibilities. Past experience teaches that commercial exploitation follows in the wake of the development of new capabilities, and cannot be accurately foreseen. 35

Space industrialization could be greatly enhanced by the use of extraterrestrial raw materials. SPS could lead to lunar or asteroidal mining by fostering the development of transport and robotics capacity, as well as by providing a major market for processed products such as aluminum, steel, silicon, and oxygen. The most detailed studies have examined mining the lunar surface, and launching raw materials to orbiting processors via an electrically powered mass driver. Others have suggested mining or capturing a small asteroid, preferably a carbonaceous-chondrite asteroid rich in carbon and high-grade iron/nickel ore.[36] Establishing such facilities, which might be done in the later stages of SPS development, could considerably reduce the costs of transporting material to high orbits.

On the ground, SPS would require large-scale automated production of solar cells; some of this technology could also be used for ground-based solar projects.

Space or ground-based industries using SPS-developed technology or hardware could, at least temporarily, compete with SPS for scarce resources. A mechanism for allocating priorities might have to be established to resolve competing claims.

Alternative SPS uses; Depending on the electromagnetic environment (i.e., on the type of system used and the amount and type of shielding available), the SPS platform, whether in (GEO or LEO, could be used as a station for a variety of communication and remote-sensing equipment. A GEO SPS would be especially useful, due to the relatively small number of positions available. Remotely operated optical astronomy devices could be placed near or on SPS as a way of escaping the interference faced by Earth-based telescopes. Given a large amount of space traffic associated with increasing industrial and military space flights, the SPS station could become a focal point for local storage, refueling, and rest and relaxation for crews – a kind of spaceport. Living quarters for maintenance crews and construction workers could be expanded and upgraded into occasional (and, initially, very high-cost) tourist accommodations.

SPS electricity could be used in orbit, either at the satellite itself or at remote sites equipped with receiving antennas, to provide power for industrial activities. Processing, especially of extraterrestrial raw materials, could require large amounts of electrical power that might be more efficiently supplied by a central SPS than by building specific electrical capacity.

Some SPS designs, especially the mirror-systems, might produce enough power to be used for local climactic modification. This would require more precise understanding of weather systems than is now available. Orbital mirrors have also been suggested as a way of providing nighttime illumination of cities and/or of cropland to enhance growth. 37

[35] Woodcock, op cit , p 12
[36] Drexler, op. cit , pp 10-11

[37] Woodcock, op cit

Special mirror surfaces that reflect only specific wavelengths would need to be developed for such purposes.

Generic economic and social effects: A successful SPS could be instrumental in provoking an economic upsurge by stimulating new production in the aerospace and energy industries, and new industries altogether in space fabrication, solar cells, antenna construction, and so on. Specific technical advances necessary for SPS and likely to provide economic spinoffs have been mentioned. The likelihood of a revolutionary new product, comparable in effect to the transistor or microchip, resulting from SPS is unpredictable. Estimates of the aggregate economic and technical effects of large research and engineering projects, such as Apollo or nuclear reactors, vary enormously. Some credit a large portion of the U.S. economic vitality and technical leadership in the 1960's, especially increases in research, engineering, and project management skills, to Federal investments in the Space program.[38] SPS might prove equally stimulating. Others argue that these resources would have been available anyway, and could have been used in more efficient ways.

Arguments about long-term social vitality also often revolve around the Apollo experience. The optimism and vision that characterized the "Apollo decade" are contrasted with the pessimism, uncertainty, and sense of limits of the post-Apollo 1970's. Skeptics, however, argue that Apollo represented a misguided effort to escape from more pressing social and political problems, and that the space program lost public support when this became apparent[39] (see ch. 9). Whether the United States will regain some of its former enthusiasm for large high-technology projects will depend partly on the success of current efforts, such as the Space Shuttle, and on the magnitude and type of benefits that such projects offer.

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[38]Drexler, op cit , pp 8-9

[39]Klaus Heiss, "New Economic Structures for Space in the Eighties, *Astronautics and Aeronautics,* January 1981, p 17