NSF-RA-N-74-013

Workshop Proceedings

Photovoltaic Conversion of Solar Energy for Terrestrial Applications

Vol. I. Working Group and Panel Reports

October 23-25, 1973
Cherry Hill, New Jersey

Organized by
Jet Propulsion Laboratory
California Institute of Technology

Sponsored by
National Science Foundation
RANN—Research Applied to National Needs

Grant No. AG-485

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PREFACE

The Workshop on Photovoltaic Conversion of Solar Energy for Terrestrial Applications was held in Cherry Hill, New Jersey on October 23, 24, and 25, 1973, under the sponsorship of the National Science Foundation—Research Applied to National Needs (NSF-RANN) program. The meeting was called in recognition of the pressing need for the exchange of information among researchers in this field and to promote a dialogue between the researchers on the one hand and representatives of manufacturing, marketing, government and utilities on the other. Considerable effort was devoted to obtaining participation from a broad representation of the manufacturing, marketing, and user fields having an interest in large-scale photovoltaic application for our national energy needs. All attendees showed enthusiasm by their participation and cooperation in preparing the Workshop summaries for publication. There were about 135 participants at the Workshop. The meeting was also intended to aid NSF in planning resources and in developing reasonable goals and milestones for the photovoltaic program within the constraints of expected funding.

The proceedings of this Photovoltaic Workshop have been published in two volumes. The first volume covers the introductory remarks by NSF, the working group summaries and discussions, and the panel discussions. Volume II encompasses the five sessions of technical presentations and discussions. The agenda for the entire three-day workshop and the list of attendees can be found at the back of Volume I.

The questions, answers, and comments following each presentation were transcribed as completely as possible from tape recordings. The names associated with the questions and comments were deleted, since not all could identified. Some editing was employed to improve readability. The prepared papers have been printed as received, although some changes may have been incurred in the process of editing galley proofs.

These Proceedings were prepared and published by the Jet Propulsion Laboratory under Contract 382-10-00-00-28 from the National Science Foundation. The contents of the papers and the opinions expressed in the discussions are those of the participants and do not necessarily reflect the views of the Jet Propulsion Laboratory or of the National Science Foundation.

This publication represents the results of one phase of research carried out at the Jet Propulsion Laboratory, California Institute of Technology, under Contract No. NAS 7-100, sponsored by the National Aeronautics & Space Administration.

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INTRODUCTION

INTRODUCTION

INTRODUCTORY REMARKS

H. R. Blieden
Advanced Energy Research and Technology
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National Science Foundation
Washington, D. C. 20550

October 23, 1973

It is a pleasure to welcome you today to the NSF Workshop on Photovoltaic Conversion of Solar Energy for Terrestrial Applications. I want to thank all of you for attending. This will be a working meeting and, I hope, a productive one that will be of value to you. We will try to keep the sessions informal; however, the tight agenda will require a certain discipline on everyone's part if we are to achieve our objectives.

The purpose of the Workshop is fourfold:

(1) To assess the present status of photovoltaic conversion and the ultimate impact it will have upon the national energy picture.

(2) To determine requirements of manufacturers in the semiconductor, power equipment, and related industries, as well as those of expected users, such as the power utilities, the building construction industry and others.

(3) To provide a forum for active interchange between researchers, industry and potential users.

(4) To assist in the formulation of a national plan for the photovoltaic conversion of solar energy.

The National Science Foundation established a research and development program in terrestrial applications of solar energy in FY 1971 in the Research Applied to National Needs (RANN) program of the Research Applications Directorate. The major responsibility for the solar energy activities in the RANN program resides in the Division of Advanced Energy Research and Technology and the Office of Public Technology Projects. Forty projects are presently being supported – more than double the number of projects funded a year ago. The funds estimated for FY 1974 are $13.2 million, a considerable increase over FY 1973 ($4.0 million) and FY 1972 ($1.7 million).

The general objectives of the solar energy program are: (1) to provide the research and technology base required for the economic terrestrial application of solar energy and to foster the implementation of practical systems to the state required for commercial utilization; (2) to develop at the earliest feasible time the potential of solar energy applications as large-scale alternative energy sources; and (3) to provide a firm technical, environmental, social, and economic basis for evaluating the role of solar energy utilization in U.S. energy planning. These objectives are based upon the recommendations of the Solar Energy Panel, organized and funded by NSF and NASA in January 1972 under the auspices of the Energy R&D Goals Committee of the Federal Council for Science and Technology. This Panel's purpose was to assess solar energy technologies and to propose a research and development plan. In addition to NSF and NASA staff participation, about 35 solar energy experts from universities, industries, and other government agencies became working members of the Solar Energy Panel. The Panel's report*, issued in January 1973, became the basis for a five-year U.S. solar energy research and development program organized into the following areas:

(1) Heating and Cooling of Buildings

(2) Solar Thermal Energy Conversion

(3) Bioconversion

(4) Wind Energy Conversion

(5) Ocean Thermal Energy Conversion

(6) Photovoltaic Conversion

Five-year objectives and plans and five-year budget projections to implement these plans have been formulated for each of the solar energy program areas.

The five-year goal of the Photovoltaic Conversion Program is to undertake component and subsystem proof-of-concept experiments on the fabrication of low-cost solar cells and solar arrays. The initial objective is to reduce production costs by a factor of ten under present costs of less than $50 per watt of silicon solar cell output. In this technology area, the NSF/RANN solar energy program is supporting eleven projects at: Boston College, Brown University, University of Delaware, Harvard University in cooperation with Tyco Laboratories, Inc., Rutgers University, Southern Methodist University in cooperation with Texas Instruments, Inc., Stanford University, University of California at Berkeley, Boston University in cooperation with Esso Research and Engineering Company, American Cyanamid Corporation, and the Jet Propulsion Laboratory.

In FY 1974 component and subsystem proof-of-concept experiments will be initiated to evaluate the quality and costs of photovoltaic arrays and systems. Also alternate approaches for fabrication of solar cells and for new solar cell materials will be undertaken. An analysis will be initiated for photovoltaic systems for a variety of applications, e.g., residential power, remote power stations, and special commercial power needs.

In order to achieve the goals of the Photovoltaic Conversion Program, a detailed plan has been prepared which will be discussed later in the meeting. In the working group sessions you will have an opportunity to provide your own input to this plan. The summaries of the working groups will be presented later in the program and will later be published in the Proceedings of the Workshop. These recommendations constitute an important part of the output from this meeting, so please take an active part in their preparation.

I would like to take a moment to acknowledge the fine job that John Goldsmith, Dick Stirn, and Ralph Lutwack of the Jet Propulsion Laboratory have done so far in organizing this Workshop. The Cherry Hill Lodge offers an attractive setting, and if the meeting proceeds as planned, the next three days should be most stimulating and informative for all.

*This report can be obtained from the National Technical Information System (NTIS, Department of Commerce, Springfield, Virginia 22151, Document PB-221659, ($2.75).

INTRODUCTION TO WORKING GROUP SESSION

H. R. Blieden
Advanced Energy Research and Technology
Research Applied to National Needs
National Science Foundation
Washington, D.C. 20550

October 24, 1973

These working groups have been organized in order to obtain your assistance in formulating a national plan for photovoltaic conversion. The general objectives of the solar energy program are: (1) to provide the research and technology base required for the economic terrestrial application of solar energy and to foster the implementation of practical systems to the state required for commercial utilization; (2) to develop at the earliest feasible time the potential of solar energy applications as large-scale alternative energy sources; and (3) to provide a firm technical, environmental, social, and economic basis for evaluating the role of solar energy utilization in U.S. energy planning. The current five-year objectives of the photovoltaic conversion program are given below:

(1) To reduce the cost of single-crystal silicon wafer solar cells by a factor of more than 10 (to about \$5/peak watt).

(2) To provide the research base for alternate solar cell technologies; i.e., CdS, GaAs, thin film silicon, etc., showing low-cost potential.

(3) To conduct systems and applications studies for low-cost fabrication of cells and arrays.

(4) To identify a system proof-of-concept experiment (Phase 0) projecting power costs a factor of 10 lower.

Now, how would *you* propose to accomplish these goals by implementation of specific programs? Your consideration of this question is very important. I am sure that you have all come prepared to contribute to the answer!

As a guide, assume several parallel efforts as indicated earlier. What initial 5-year program would you propose to fully explore all aspects of the development and utilization of photovoltaic conversion? What must be done to insure at the earliest date substantial commercialization of photovoltaic conversion for the production of electricity in a variety of applications? What must be done to have substantial impact (at the earliest date) on the national requirements for electric power generation?

I would like to suggest that you consult with your working group leader to answer any questions that may arise. We will reserve for the session tomorrow afternoon a detailed presentation of the NSF/RANN current program and proposed plan in photovoltaic conversion now under consideration in the ten-billion-dollar 5-year plan for a national energy R&D program.

WORKING GROUP RESUMES AND DISCUSSIONS

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WORKING GROUP RESUMES AND DISCUSSIONS

SINGLE-CRYSTAL SILICON

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Presentation Summary

A. Group Makeup

This workshop consisted of twenty-one members with a balance of University (5), Government (2) and Industry (14) people. There were six materials people, six device people, and two solar cell production people from leading silicon companies. More than half of the group could be considered top experts in his field. So, it would appear that the group's findings would be worthy of considered attention. Time was the only limitation! The five hours spent were insufficient for a full and complete discussion of the subject. The timing and costs of technology developments discussed here would probably need considerable additional study.

B. State of the Technology

The advantage of the single-crystal silicon solar cell approach is evident as shown in Table I. High efficiency, abundant material, theory and technology well understood, and proven reliability are agreed-on advantages. The only present real disadvantage is the high relative cost of such cells. Possible improvement in efficiency over 18% is likely with further experimental work in materials and cell design.

It is important to keep in mind that if silicon solar cells (10% efficient, 8 mils thick) were to give us 1% of today's electrical power needs (2×10^9 W), it would require about 3 times the annual U.S. production of single-crystal silicon or about 1500 tons. It is therefore of importance to consider the total cell production from "sand to cell" – economies of scale and uniform product should take place faster than expectations in the semiconductor industry today.

Most of the group's time was spent discussing ways and means of getting to low-cost cells in the ten- to fifty-cent per peak watt range. This report will primarily treat that discussion. To get from peak watts to average watts, a factor of 5 is reasonable.

C. Cost Reduction

1. Raw Silicon

We consider here the polysilicon starting material that is used to make single crystal. It was pointed out that three high-temperature cycles are presently used whereas one might be possible, and that special purity considerations are needed for present-day applications. For the large amounts of silicon being considered in this study, one doping level should be satisfactory. Also, if the silicon does not have to be highly purified, it should be cheaper. It was predicted that a savings of from 3 to 5 over the \$60/kg price paid today could be expected. Trichlorosilane ($SiHCl_3$) costs \$6/kg based on silicon content. Silane (SiH_4) might be used and integrated into the single-crystal process.

In order to achieve this result, a study should be undertaken, immediately followed by an experimental pilot plant costing about \$6 million. A factory to produce the silicon for annual production of 5×10^8 peak watts by 1985 would cost \$50 million.

2. Single-Crystal Manufacturing

Three approaches were considered with the following cost reductions predicted considering the high-volume solar cell business:

Czochralski: factor of 2

WEB: factor of 5

EFG: factor of 10-100

It was pointed out that Czochralski crystal could be grown in 5-in.-diam ingots, but cutting and polishing losses and costs were too severe to allow more than a factor of 2 cost reduction. Float zone was considered briefly and thought to be comparable to Czochralski in the long run.

WEB dendrites could be scaled up in crystal growth speed and geometry, perhaps to 10-cm width. A cost reduction factor of 5 was considered possible. A $1-2 million R&D program would be needed to determine the potential of this cost reduction. Unless a factor of over 10 in cost reduction is probable, the WEB material would not lead to the low cost cell we are looking for.

The EFG process shows promise of large cost reduction, not only because of speed of crystal growth, but also because of the possibility of multiple growth. Two companies have achieved results. The key problem is finding a die material that withstands the temperature without interaction with silicon over a long time (a tall order). It is estimated that about $5 million of R&D at several companies will be required. Another $15 million might be needed to scale up the process, with about $30 million needed to develop a factory. For example, 7 square miles of cells (to give 2×10^9 peak watts/yr) would require 560 EFG crystal growers, each growing ten 3-in. ribbons at 6 in./min, operating for 12 hours a day the whole year. This assumes 100% crystal and cell yield. (Tyco's numbers of 20 ribbons simultaneously, 2 in. wide at 2 in./mm, indicate one would need 1260 EFG crystal growers.)

The EFG process is the key process to low cost silicon cells and the die problem is the key technical difficulty that has to be overcome. It requires early support since the lack of a solution here would be a "show stopper" and other silicon investments would not be warranted. It was pointed out that sheet crystal in rectangular form was also very important to low cost fabrication.

3. Process Technology

An evaluation of the best junction fabrication approach is necessary. It could be diffusion, ion implantation, or epitaxial growth. Each of these processes could give high quality solar cells and is capable of being scaled up.

A continuous manufacturing process is indicated – the input might be sand and the output an encapsulated cell. As a minimum, the single-crystal sheet would go into the machine and all junction formation, contacts, etching, etc., would take place automatically. It may be possible to perform automatic testing so that rejected cells would go back into the starting position to undergo reprocessing.

A reliable, reproducible, high-yield production-prone process has to be worked out. Process definition is estimated to cost $6 million. Developing a pilot plant of size consistent with full operating economies for the automation would cost about $12 million, and a factory to turn out cells is estimated to cost $80 million.

4. Packaging

The question of packaging came up and it was suggested that, for a 20-year life, some form of cell encapsulation would be required. This has to be determined and tested and would cost about $1 million to develop. There is much passivation technology to borrow from in the semiconductor industry. Some form of encapsulation must be made an integral part of the continuous cell fabrication process. Questions regarding arrays in this connection have to be resolved.

D. Improved Cell Characteristics

Supporting development of $3 million to $5 million per year is required to back up the whole program. A goal of the program should be to improve the conversion efficiency of solar cells through increases (if possible) in fill factor, short-circuit current, and open-circuit voltage. It would be desirable to decrease resistivity of the bulk silicon to, say, 0.01 ohm-cm. Lower resistivity gives higher open-circuit voltage and permits the use of cheaper silicon. The problem is to decrease leakage current at the higher doping level. Therefore, study mechanism of excess current. Also seek to increase short-circuit current by antireflective coatings which are matched across the spectrum.

Efficiencies up to 20-22% should be possible, although in the mass-produced low-cost cell, we are estimating only 19% AM1 efficiency. Efficiency is important since it reduces the area of land cover and also reduces the cost of the array – both may be considerably expensive. Some of this work could be undertaken at universities.

E. General Conclusions

We believe that the program suggested could lead to a 50¢/peak watt cell by 1985 with a volume of 5×10^8 peak watts available and that by the year 2000, considering scale up and learning curve expectations that a 10¢/peak watt cell at 5×10^{10} peak watts would be possible. The problem areas are summarized in Table II.

Key elements in the technology development program are shown in Table III. These have all been discussed.

The required dollar resource is shown on Table IV. Note that a $250 million investment is estimated to achieve the 1985 goal. The investment beyond 1985 would be less. The industry, manpower, and materials resources are well within reasonability.

The proposed milestones are shown on Table V. These are keyed to the resources and the knowledge of what it takes to develop the technology and scale it up. If this paper is to be taken seriously, a number of studies should be undertaken immediately.

Discussion

Q: What probability of success is attached to the fifty-cent a watt price goal?

A: That's a speculative thing you are asking me about. I think the probability of success is very high. You're really asking me what the probability of success of the EFG process is. We believe it's quite high. I think that's the absolute key part of it.

Q: I'm not sure of the cost data, but you had on your chart 1974 costs of $5.00 per peak watt, and that's sort of like the base line from which you ----?

A: It's really not. I don't care if it is $20 a watt at the present time. We feel that prices now are artificial because demand is too limited.

C: I guess the point I want to make is that I understand that the number possibly is based upon concentration multiplied by ten, and that was the number we were struggling over the first day of the conference, trying to understand how the numbers have gone down so quickly from the space program, and that's one explanation.

A: When I came to the conference today, I felt the number was something like 50 to 60 dollars by the major solar cell manufacturers and about $20 a watt from some of the more venturous operations using low-cost silicon.

Q: So isn't the number somewhere between $50 and $20 a watt if we are talking about unconcentrated solar energy?

A: Yes

Q: How did you arrive at that market curve – the volume with price?

A: It's very interesting to see how market plans are made. I have taken part in some of these for my own corporation, and I will say that the exercise is not that far different from what we have been through last night.

A: It is really a compendium of information from a number of different people.

Q: Did you mention what efficiencies you are going to be shooting for at 50 cents per watt?

A: I think around 20 percent by 1985. I think that is conservative considering how the field has been moving.

Q: Do you think that you can simultaneously achieve a 20 percent cell and still have that cell at 50 cents or 20 cents per watt, or do you feel that you may want to compromise?

A: I think the 20 cents a watt is going to come from the scale-up of production and the learning curve. We have incorporated a supporting development program with substantial funding – it starts out at $3 million, ending at $5 million – on a continuing basis to back up this program. We had some discussion as to how large such a program should be, but I see coming out of supporting developments the technology that will lead to the higher efficiency. I do want to point out that I think we are proposing a modest improvement in efficiency. So I think we are being very conservative.

Q: Can the developments already be identified that lead to the 20 percent? In other words, do we know why we are down to 13 or 14 percent now and what we are going to do?

A: We can identify some things. For example, leakage current in the junctions, which could be solved by getting better mechanical or impurity perfection in the crystal, or by going to higher doped material, so that we could get higher voltage. We have a couple of orders of magnitude to go. If we knew how to dope the material a couple of orders of magnitude higher while keeping the mechanical perfections and the lifetime as maintained, we certainly could get into this efficiency range. For example, we are saying 0.01 ohm-centimeter with 10-microsecond lifetime could give open-circuit voltages in the eight-tenths of a volt range. I really don't think that's insurmountable by any means, and we know which way to go.

Q: Is that reasonable for EFG-grown sheets – when we have the recombination center concentrations that we have?

A: Now that's a good question. We are really at a very early stage with respect to the EFG material. We did discuss this to a certain extent, and they convinced me that the material was not under that much strain, that it really isn't that much different from ordinary Czochralski; so I came away from our meeting thinking it is more possible than, perhaps, I felt at the beginning.

C: Even if that process gives you ten percent rather than 20 percent cells, resulting in a dollar a watt, it is of significance.

Q: Dr. Schwuttke, can you answer the question about the potential perfection of the EFG material as compared to Czochralski?

A: Tyco and ourselves have produced by EFG small sections with excellent specs. So based on these preliminary results, I believe that the EFG in the long run with very hard work can equal the Czochralski effects. There is a good chance that this can work.

C: We are talking about a really substantial investment in that material, and we see more than one company being in it too. If all these areas are going to be funded to the extent we are recommending, it is going to invoke competition and multiple operations.

Q: May I ask what impact EFG could make on other devices in volume and dollar costs?

A: You recognize that if the hour were not so late, we would have tried to make an impact chart. The one impact item that we did get is that if this program is successful, it is going to have a major impact on the silicon semiconductor business; there is no question about that. We are talking about large cost reduction factors, and as you know, this is very important to the semiconductor industry. I don't know whether the fabrication of device technology will have as much fallout, because we envision this processing technology as something that is very specific to making a solar cell. It may have no use for anything else except making the solar cells, and we think that's important. We can see that a very specific technology will be developed that will be able to crank out just solar cells, and maybe have very little flexibility in terms of anything else.

Q: I wonder what you mean by impact on the silicon industry, when the price has come down on the chip which did cost something like $20, $30, to 20 cents now; how much further---?

A: It's not there yet.

C: It depends upon what kind of chip we are talking about.

A: Ten or twelve dollars is the cost of some chips in production quantities. The materials cost of that is only a dollar or two, but there are a lot of devices that use more silicon, i.e., power transistors and automotive power switching devices. Also, power switching becomes important. They all use a lot more silicon, so integrated circuits are the worst example to use, though your point is well taken.

Members of Single-Crystal Silicon Working Group

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Table I. State of the Technology

Proven efficiency 12-15% (AM1)
Theory well understood
Technology understood and well developed
Direction for improvements understood
Materials abundantly available and safe
Reliability proven
Energy economy is good
High cost but room for improvement
Multi-kilowatt arrays have been built

Table II. Problem Areas

1. Cost	
a. Materials:	Raw material need factor of 3-5 cost reduction Thin single-crystal growth Die problem with EFG
b. Fabrication:	Automation to reduce cost
2. Vertical Integration:	Sand in, cells out
3. New process technology needed for very high production:	Techniques and machinery to make low cost, reliable, reproducible cells with high yield
4. Encapsulation needed to give 20-yr life	
5. Scale up problem	

Table III. Key Elements of Recommended Technology Development Program

a. Low cost poly
b. Low-cost single-crystal <u>sheet</u> silicon (could be the show stopper)
c. Automated manufacturing
d. Basic studies to support program and improve efficiency to 20% (AM1)

Table IV. Required Resources in Millions of Dollars

Task	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985
Reduce silicon poly cost	0.5	0.8	1	1.5	2	◁—	50	—	—	—▷	
Silicon ribbon:											
Technology	1.5	1.5	2								
Machinery		1	2	3.5	5	◁—	—	30	—	—	—▷
Cell manufacture:											
Process definition	2	2	2								
Encapsulation, reliability	0.25	0.5	0.25								
Automation				3	4	5	◁—	—	80	—	—▷
Supporting developments	3	4	4.5	5	5	5	5	5	5	5	5
Total	7.25	9.8	11.75	13.0	16.0		◁—	—	195	—	—▷
								Total $250 to 1985			

Table V. Milestones

FY	75	76	77	78	79	80	81	82	83	84	85	2000
Cell cost/peak watt					\$2.50						\$0.50	\$0.10
AM1 efficiency	13-15%				16-17%						20%	
Production rate peak watts/yr					6×10^6						5×10^8	5×10^{10}
Low-cost polysilicon		▲ Select process			▲ Pilot plant				▲ Large-scale plant			
Single-crystal ribbon			▲ Tech devel		▲ Multiple growth			▲ Pilot plant		▲ Large-scale plant		
Cell fabrication automation process development			▲ Define process		▲ Plant design		▲ Pilot plant			▲ Large-scale plant		
Encapsulation pkg and reliability			▲ Design complete									

POLYCRYSTALLINE SILICON

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Presentation Summary

I. Introduction

Ingots and films of polycrystalline silicon have been used for the fabrication of solar cells. However, no major efforts have been directed to the development of polycrystalline silicon solar cells, and the present state of technology is rather primitive. Large grains are obtainable in polycrystalline silicon ingots, and solar cell efficiencies up to 6% have been obtained. The reported efficiencies of polycrystalline thin film cells were less than 1%. Since the manufacturing cost and electrical energy required for the fabrication of polycrystalline cells are many times less than those of single-crystalline cells, the use of polycrystalline solar cells is a promising approach for terrestrial applications. Although polycrystalline cells will not be able to compete efficiency-wise with single-crystalline cells, a worthwhile lower efficiency cell will provide a unit power cost many times lower than that of single-crystalline cells. The major problems limiting the development of polycrystalline silicon solar cells, the breakthroughs in technology necessary for the production of these cells, and a program for the development of low-cost polycrystalline silicon cells are discussed below.

II. Major Problems

The development of low-cost solar cells from polycrystalline silicon ingots or films appears to be limited by the following factors:

(1) The grain boundaries in polycrystalline silicon reduce the carrier mobility and lifetime, thus limiting the cell efficiency.

(2) The grain size in polycrystalline silicon is usually small and not reproducible.

(3) The present high cost of materials and processing limits the use of polycrystalline silicon ingots for solar cells.

(4) The lack of suitable substrates and ineffective absorption of radiation limit the use of polycrystalline silicon films (5-20 μm in thickness) for solar cells.

(5) Polycrystalline silicon p-n junctions usually have soft current-voltage characteristics, thus low efficiencies.

III. Technology Breakthrough

Several breakthroughs in technology are necessary for the production of low-cost silicon solar cells from polycrystalline ingots or films.

(1) A new technology to produce solar cell quality silicon must be developed to reduce the cost of polycrystalline silicon wafers by a factor of 10.

(2) The grain boundary effects in silicon must be reduced significantly in order to obtain a solar cell efficiency of 5% or better.

(3) Low-cost substrates ($0.50/m^2 or less), compatible with silicon in properties, must be developed.

(4) Large area (1 m^2, for example) junction formation technology, either p-n junctions or Schottky barriers, must be developed.

(5) The configuration of large area devices must be optimized, and the contact and encapsulation technologies must be refined.

IV. Objectives

(1) The feasibility of producing polycrystalline silicon solar cells with 5% efficiency should be demonstrated in 5 years.

(2) After achieving the first objective, a pilot plant with a capacity of 10,000 m^2/year will be established in 8 years.

(3) The efficiency of polycrystalline silicon solar cells will be increased to 10% in 10 years.

(4) Production facilities of polycrystalline silicon cells with a capacity of 100,000 m^2/year/line will be in operation after 10 years with a cost objective of less than $0.50/W.

V. Schedule, Principle Milestones, and Resource Requirements

Because of the primitive state of the art and the complexity of the problems involved, several parallel efforts should be devoted to the research and development of low-cost polycrystalline silicon solar cells. The approximate schedule, principle milestones, and resources requirements are shown in Table I.

Discussion

Q: I would like to know some more about this ten percent efficient polycrystalline cell you are talking about. What are the assumptions that are going into that? What is the grain size?

A: We do not know. It could be polycrystalline material in the form of sheets with very large grains, or it could be polycrystalline thick films with the effects of grain boundaries, either limited or reduced. As I have mentioned earlier, there have been very few reports concerning polycrystalline silicon solar cells. The reference with the six percent cell was a Russian article. I usually take it with a grain of salt.

Q: Large grains or small grains?

A: Large grains. I remember the picture in the paper. The area of the cell is only 14 square centimeters, and clearly shows very large grains. I assume that the material was obtained by passing one zone – one molten zone through the polycrystalline ingots. Of course, there are other possible techniques for obtaining large-grain polycrystalline sheets at much lower cost. Even the present day cost of polycrystalline silicon is $60 per kilogram. For solar cell purposes, we do not need such high purity. The metallurgical grade of silicon costs around 50 cents per kilogram. We want silicon with a quality somewhere in between.

Q: I was going to comment on the difference between your study and our study with respect to the starting material. You are expecting a factor of ten reduction in cost and we are expecting a factor of three, but I didn't point out that we are not able to use, we don't think, the low-grade silicon because of the efficiency. On the other hand, it seems to me that you are cranking in all of the benefits that you could possibly get out of low-cost technology, but when you start asking for 5 or 10 percent from your films, I start becoming very skeptical. Let me ask you how thick these cells are – the 5 or 10 percent cells. What thickness are you envisioning for silicon?

A: This depends. If we can eliminate or reduce the effect of grain boundaries, we will probably be using the thin film approach with a thickness of, say, tens of microns.

C: I have just compared some curves and at ten microns, it seems to me that your short-circuit current is down to one-third of what it would be at full thickness, and at one micron it's about one-tenth. There are other losses that would have to be considered in the final device besides the short-circuit current.

A: As far as films are concerned, the cost is not much different whether you have 50 microns or 100 microns, because, on the manufacturing basis, CVD can be a very economic process, and the conditions are less critical, less stringent, than the epitaxial technique used in production today.

C: The French about ten years ago at the French Phillips organization developed a 6 percent polycrystalline cell.

Q: Did you say you are going to get that with a fraction of a micron?

A: No. Yesterday we were debating what kind of thickness should be used in the polycrystalline thin film cells. We thought maybe about 5 to 20 microns, and, as I mentioned earlier, the cost of manufacturing them doesn't really depend that much on the thickness of silicon, because trichlorosilane is relatively cheap – 60 cents per kilogram – whether you put down 20 microns or 50 microns, the cost will be in terms of pennies.

C: The lifetime is not really a constant parameter if you think of very thin films, and we would like to just work with, say, one quarter of the solar input at the first 10,000 to 20,000 angstroms. We can live with pico-second lifetimes then. And so if a thin film is used and a large part of the spectrum is given up, such as half of it, the lifetime required is cut way down.

Q: Would you use EFG to make polycrystalline silicon?

A: I would say it is a very expensive way to go. I think one necessary requirement for low-cost silicon is not to go through a melting process. For example, the power required to grow or to melt and solidify a four-kilogram charge could be several hundred kilowatt-hours.

Q: Then that seems to question the whole basis for the single-crystal presentation. I guess I'm not asking you that; I'm asking the single-crystal people to reply to that comment. Let me make one other comment: It seems to me that the single-crystal and the polycrystalline programs have been sort of artificially separated by the way this is set up. They seem to me to be – at least at the beginning they seemed to be – in competition, but, in the end, one of them has got to win over the other one. You are not going to make pilot plants for both of those, are you?

A: That's right. We will have a decision point at the end of the five-year program.

C: It seems to me that the whole sort of get-together has got to come early in the game. When you talk about resource allocation, and this is a problem for the RANN people, you needn't worry about two separate pilot plants. That part, it seems, has got to be decided.

C: I can only speak for myself because there are other people involved in it. If one can obtain ten percent efficient cells from a thin film silicon technology, then I would say scrap the single-crystal work, but I think the probability of success for that is 10^{-6}.

A: That remains to be seen. However, if we can achieve a five percent efficiency with the cost considerably lower than your 20 percent efficiency with single crystal ---.

C: I raise the probability of that to 10^{-2}.

C: Surface passivation of single-crystal silicon has been a problem that has been with us since the beginning, and it hasn't been really resolved. And when you get to the polycrystalline material, all you've done is to take the surface and put it into the bulk, because every grain boundary is a surface and you have the same problem now, only it is throughout the entire material. And it's not clear, at least from the discussions here, that from any work now going on in the semiconductor industry, you are going to be able to get rid of that very high recombination velocity at these grain boundaries, unless you attack that problem first and resolve it.

A: Yes. And that is one of the major problem areas where ---.

C: But there have been twenty years of research on that already without much success.

A: I don't think anyone has really worked on the grain boundary effects, and as far as the surface passivation of silicon I don't think it is a problem anymore. If you are talking about III - V compounds, it's a different ball game. No one can passivate any of these III - V compounds, but the passivation of silicon devices is a cinch these days.

C: Just one comment: The multiple-growth electric path consumption is not significant and is not a large fraction of the cost to manufacture, which is another way of stating that if you compute the return economics of how long you have to run the solar cells to get back the electric power, people have calculated that it can be as little as three weeks or at most six months. So I don't think it is a valid criticism of either approach, but you have to use some electric power to make the silicon.

C: As far as the probability of success goes, I suggest that we are looking into a polycrystalline ball.

C: I would like to answer an earlier question. There may be two pilot plants because we are talking about rooftop power, where single-crystal high efficiency is a necessary goal, and solar farm power, where cheaper one percent cells may end up to be the ultimate choice. We may have to separate the market.

C: I really think that one percent is going to be out of the question. Real estate is not that cheap, even in Arizona, and I can't see going to photovoltaics then.

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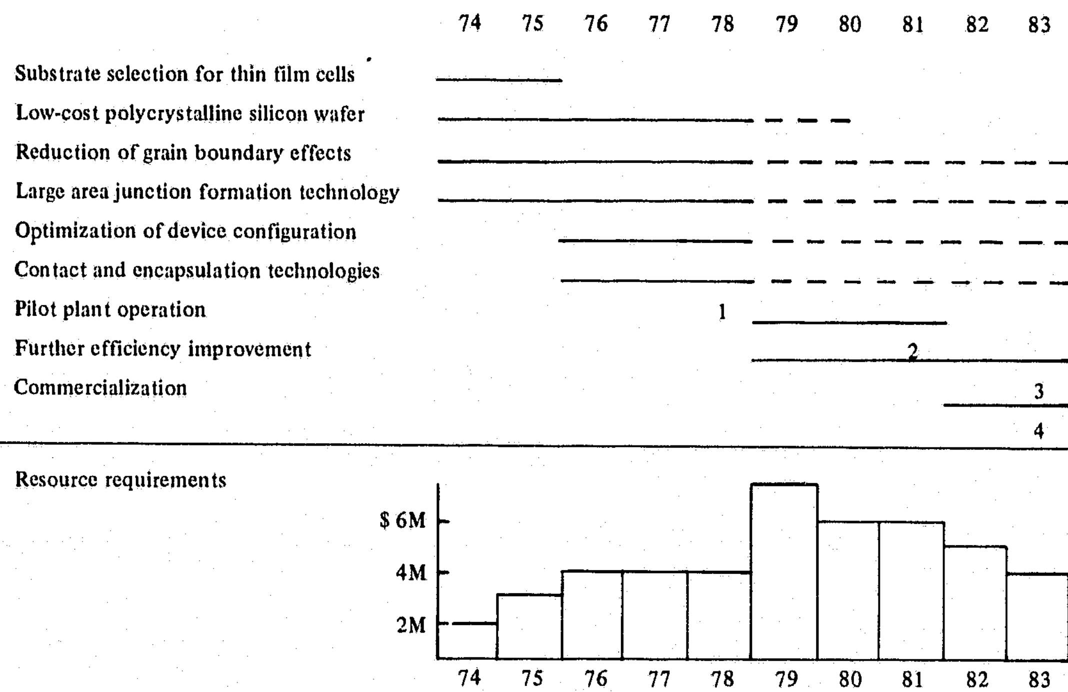
Table I. Schedule, Milestones, and Resources Requirement for Polycrystalline Silicon Solar Cells



Milestones:
1 Decision point, 5% efficiency demonstrated.
2 Completion of pilot plant operation with a capacity of 10,000 m^2/yr.
3 10% efficiency demonstrated
4 Completion of production facility with a capacity of 100,000 m^2/yr/line.

CdS/Cu_2S THIN FILM CELLS

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Presentation Summary

CdS/Cu_2S solar cell hardware is presently available with up to 8.3% conversion efficiency at air mass one and at 25°C. Many thousands of 3 × 3" mylar-covered cells with efficiencies in excess of 5% have been made. Most of these cells degrade in ambient air rapidly; however, when protected from oxygen and water vapor and when properly electrically loaded, there are indications that a lifetime in excess of 15 years can be expected, as deduced from accelerated life tests. Moreover, there are currently several cells in existence which have survived in excess of seven years under ambient conditions without apparent degradation.

The degradation mechanism is currently not understood; however, there are indications that retardation of this degradation can be achieved by controlling the composition of the copper sulphide and by proper doping of the CdS as documented by SAT (France). So far only a laboratory pilot production of modest output has been attempted. The yield of acceptable cells must be improved.

The production methods are amenable to mass fabrication. Since these cells are thin film solar cells, they are today the only ones which have already been developed to the point of economical feasibility. The following projections are based on our improvement of the current technology and do not need any technology breakthrough.

The projected cost estimates indicate a ramp-price of less than 20¢/watt at a production level of about 10^7 ft^2/year. These estimates include semi-detailed technology assumptions and employ reasonable industrial planning methods. CdTe thin film solar cells have been made about 10 years ago, and were further developed by SAT and Battelle/Frankfurt. These cells achieved conversion efficiencies between 5 and 7% at air mass one and at 25°C.

Recent material developments indicate feasibility to significantly improve the efficiency. CdTe and CdTe/CdS cells show promise of increasing stability compared to solar cells containing copper. There are other II-VI combinations possible which show promises and should be further investigated.

Problem Areas

Cell conversion efficiencies, life expectancies, and production yields must be improved. Mass production methods must be developed. A better understanding of the solar conversion mechanism in these cells must be achieved.

Potential

These II-VI compound thin film solar cells currently show the highest potential of any known solar cell for large-scale terrestrial photovoltaic solar conversion commercialization.

Summary

CdS/Cu_2S solar cells are currently available; however, these cells are far from being prototype cells and their method of production is far from being acceptable for mass production standards. Extensive research and development is necessary to bring about processes from which an optimum process (or several similarly attractive processes) can be selected.

It is the consensus of this group that several research and development centers of overcritical size should be funded, each with several satellite projects, in order to achieve the goals in the given time frame. These goals are to achieve commercialization of low-cost solar cells by 1985. The given goal-milestone chart (Fig. 1) could be achieved with a total government support of $185 million. This compares to a probable $1 billion/year market before the end of the 1980's.

The assessment and conclusions reached above and all recourses defined are made unanimously by the group. The given estimates are felt to be conservative. They do not include any technology breakthrough, nor do they include marked increases in conversion efficiencies. However, it is very likely that much further improvements would indeed be achieved. A more general estimate for the year 2000 indicates that a production cost of 5¢/watt may be possible.

Discussion

Q: For easier comparison with the first two groups, will you quote an efficiency figure which is more of an average for some reasonable yield?

A: Yes. We have assumed that in the beginning our efficiency is in the neighborhood of five percent, and that yields of the cells are in the neighborhood of sixty percent. We feel safe because many years ago, figures very close to that have been achieved. As we go out in time, we think that a seven percent cell, which has been achieved time and time again by the German group and the French group and some cells which have been made here, is a realistic figure with a production yield again of sixty percent. We are quite comfortable with these numbers and feel that they are conservative. Many of us feel that one could do much better. I know that we all are pushing for much higher numbers, but we want to make absolutely sure of what we are saying here, so we can feel safe in terms of commercialization.

Q: When do you think we will see our first stable cadmium sulphide solar cell in the United States?

A: I think that with the results we have, this has been done about eight years ago. I asked for a cell from NASA Lewis for calibration of our solar simulator. They said, "We will send you a cadmium sulphide cell. They are very stable if you keep them from seeing light and oxygen and water vapor together." Now, obviously, we don't want to cut out the light, but it is easy to cut out the oxygen and the water vapor if we accept glass, which we do. We have quotations for thin glass envelopes which are less than twenty cents a square foot, and so we are feeling very comfortable with this. I should say on the other side we have an array of one thousand two hundred cells, all in series, on the roof top of Solar One. It is hydrogen flushed and has been in operation since the beginning of July. We haven't seen any marked drop in the output that we could measure. So I think stability questions are important, and you have to deal with the cells properly.

Q: Is that only when you are using forced cooling on cells?

A: Yes.

Q: Don't you feel that the problem with increased degradation rates at higher temperatures is really a very significant problem in terms of large-scale utilization, since the average home owner, if he had the forced-air solar arrays on his roof, might want to go away occasionally and problems in systems for developing the forced cooling equipment would markedly increase the basic cost?

A: Yes. As a matter of fact, it is markedly increased and that's what forced cooling means. We have in our system a chimney effect. It is very simple to put behind the panel. It is slanted and put behind the panel with ducts. That's all that is necessary, and you force air through this with very few fins, which keep the temperature at 190°F. We know that a 150° we are safe, but we are not yet sure how safe we are at 190°. Additional work needs to be done, but that, of course, is a failsafe system. We see from our accelerated degradation curves that we don't have to feel worried very much, and also we see from the environment we have at our Institute, cells which have been deployed now for fifteen months, that if we don't do anything but let it sit there between two thick glass layers and stabilize at ambient temperature, the extrapolated lifetime is eight years. And that's not so bad to start with (without fins), and so we hope that with fins we can increase this lifetime very markedly; just putting a little air channel behind it would improve that. So forget about the two-horsepower motor which pushes air through; this is not necessary. We are sure about that.

Q: Have the accelerated life tests that you use with the cadmium sulphide been proven with other materials?

A: No. Accelerated life tests are always something which I can only be convinced with after I have lived that long and will really see whether the projections are all right, because you extrapolate the curve into the future and you don't know whether you get a kink or not. The extrapolated life tests in cadmium sulphide/copper sulphide are especially difficult because there are two phase transitions, one in the neighborhood of 100°C.

On top of that, you have stresses, so that research needs to be done. So we cannot really say. This is the reason why we were so very careful about our projections about lifetime. And we can only say that there are cells which have survived eight years, and I need only one cell which has shown this to tell you that it's true, that it is not against the second law of thermodynamics; and then we just have to learn how to make it more reproducible. I can also tell you that it is not one cell which we have, but many many cells which have survived.

Q: SAT has been running a pilot line in their laboratory for some years. To the best of my knowledge it is running over 500 French francs per watt; they want to go below 100 French francs per watt. How does this fit with your estimates?

A: As you know the SAT program is not a terrestrial program; it is for space, Air Force, or balloon operations, and the requirements are completely different. And, as you know, differential thermal expansion in cadmium sulphide/copper sulphide, cadmium sulphide-base material, and cadmium sulphide-grid are very problematic; so in order to solve all this, one has to do a much more careful job for space than for terrestrial application. There is a marked decrease in our grid cost, and the cost calculation we are planning here is based on accepted industrial practices and a quite detailed analysis.

Q: I have a question, but it doesn't pertain particularly to your subject. It's sort of a general question for everybody. We have been talking about dollars per watt of price output in preparing polycrystalline silicon, single-crystalline silicon, cadmium sulphide and cadmium telluride cells, as well as other materials. I was thinking it might be more meaningful to talk in terms of dollars or pennies, hopefully, per watt device output per area of device. This to me would be a more meaningful unit to use.

A: Certainly the deployment costs are important. When you compare a five percent cell with a twenty percent cell, four times the area is needed and, hence, four times the deployment cost. All of this has been very carefully figured into some kind of business plan and we have gone even one step further. Let us take into consideration the cost of mounting and theorize the amortization. Let us take into consideration the insurance costs that have to be paid for as part of deployment, that additional taxes have to be paid – this is on top of a rooftop of a house – and, since the house is made more valuable, there will be increased taxes (and let's lump all this together with maintenance costs, etc.), and we come up with a figure which is 17 percent. Now let us convert this to a cost per kilowatt in the Delaware area (not in Arizona), where we have only an average of five hours of sunshine over the year per day. Let us then calculate what the price of a kilowatt-hour would be. The price of the kilowatt-hour calculated from that calculation is 3.5 cents in 1973 dollars. We feel that this is a hopeful price because the increase of energy probably goes up more steeply than other commodities in the future.

Q: You referred to the possibility of wanting to produce CdS cells in more than one way, such as sprayed and evaporated cells because of different markets. Could you explain that more, and from a resource manager's point of view, how might one decide between the processes if only one is desired by the time we get to the pilot plant stage?

A: Obviously, this should be done with the help of markets. So I think the way to do it is through interim markets to final markets. Of course, we know already; we have some feedbacks on what kinds of markets there are, and it is just inconceivable that sophisticated devices such as cadmium sulphide solar cells will have the same specs. They will have different price tags attached to them. Now if it happens to be that the cheaper cell will also be the better cell in all respects, certainly you scrap the other one, but you still should not go with a sole supplier. I think that is not sound in a free economy. So for that reason I think it is important, and I think for the silicon people the same things should hold. It is important not to put all your eggs in one basket, but to get several centers financed to develop alternative solutions, and, of course, alternative markets.

Q: I notice that the proposed goal from NSF was to get down below fifty cents a watt, and possibly down to twenty-five cents a watt. I also notice that the figures here project that, by 1985, everybody is at about fifty cents or less a watt, and by the year 2000 below twenty-five cents a watt. I wonder how much influence these goals of NSF have had in coming up with these numbers?

A: Obviously we look not so much at directives obtained from some place, but at what nature or business dictates to you. If you couldn't come down below a dollar a watt, forget about solar energy for photovoltaic large-scale commercialization. So you have to come down and you ask yourself how honestly you can get down below these figures, and the answer is yes in the cadmium sulphide field.

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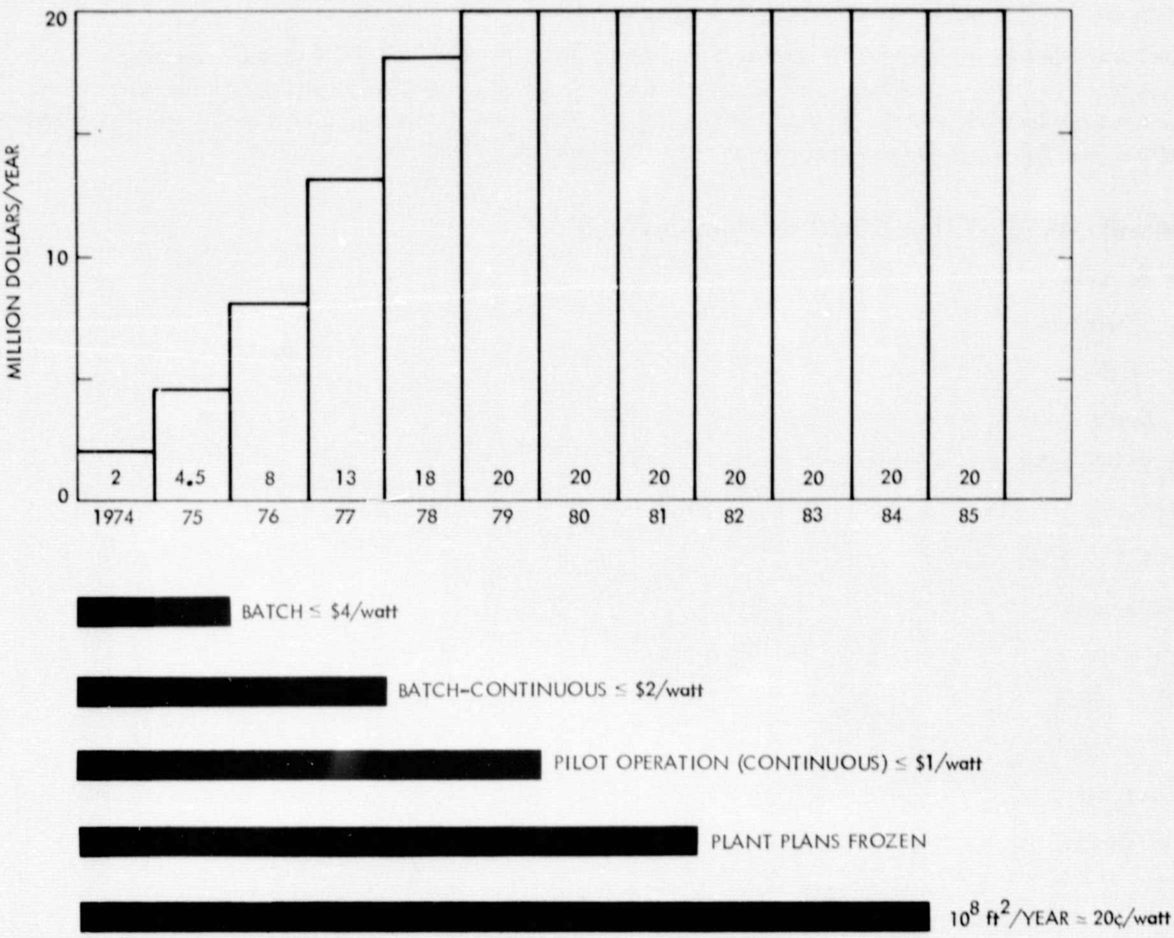

Fig. 1. Goal-Milestone Chart

OTHER MATERIALS AND DEVICES

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Presentation Summary

I. Introduction

Of the twenty-one persons in this group, seventeen were affiliated with universities, two with government laboratories, and two with industrial research laboratories. No two persons in the group were working on the same "other material" or "other device." Other materials represented included materials with a photovoltaic history like GaAs, CdTe, Cu_2S, CdS (in other devices) as well as newer possibilities like the inorganic materials BiS, $Pb_{1-x}Cd_xS$, $CuInS_2$ and $CuInSe_2$ and the organic semiconductors like tetracene, pyrene, and photosynthetic pigment. Other devices included p-i-n structure, solar thermal-photovoltaic converters, devices for photo-electrolytic decomposition of water, semiconductor-electrolyte systems, electromagnetic wave energy converters, vertical multijunction cells for high-temperature (produced by concentrators) operation, improved transparent conducting electrodes, etc. There are probably yet other materials and devices which are possible candidates for solar energy conversion; those represented by members of the working group constitute a sample of possible candidate materials and devices.

In the course of the discussion, the group expressed a strongly held view that the Photovoltaic Conversion Program should maintain a healthy segment devoted to exploration of alternatives to silicon and cadmium-sulfide solar cells because of the possibility that cells based on these materials might fail to meet the large-scale terrestrial-utilization requirements of low cost, long life, and acceptable efficiency. The view was expressed that proof-of-concept programs for any of the materials and devices described above would require significant investment of effort and resources. It was conceded that some of the systems were not likely to be brought up to a level where they could be ready for large-scale introduction in less than five years, and that such concepts should be supported at a lower level. However, others of these systems could probably succeed; indeed there was a strong current of opinion that some of the systems discussed by the group were as likely to succeed as silicon or Cu-CdS. There was little sentiment for launching a shotgun-type materials research program to identify new photovoltaic materials. It was felt that those programs should be selected for support where proposers offered strong evidence that the system would satisfy the needs. It was felt that a weeding-out process would be necessary. In most cases, the question of whether a material was likely to serve the needs of the program could be answered within about three years. This suggests a program in which certain investigations are being pursued at a low level because they have a strong long-term potential but are obviously not ready on a short term in parallel with intensive investigations of materials and/or devices which are closer to realization. If materials and/or devices being subjected to this intense investigation pass the test, funds from other less promising avenues (including silicon or Cu-CdS, if they are less successful) would be diverted to it. If the materials and/or devices falter, new ones would replace them, provided that the new candidates are justified on the basis of promise.

II. Objectives of a Program on Other Materials and Devices

It was the sense of the group that there are two general objectives for this program.

(1) To identify and develop *at least one* new photovoltaic solar energy system to serve as an alternative to the three currently most intensely studied systems (silicon single-crystal, thin-film Cu-CdS and polycrystalline Si systems), all of which are beset by problems which may prevent achievement of the objectives associated with large-scale terrestrial solar energy conversion.

(2) To identify and develop improved photovoltaic systems for large-scale terrestrial conversion. "Improved" means higher efficiency at "same" cost; lower cost at same efficiency; systems made from more abundant materials; systems of comparable performance made from less toxic substances; systems of potentially longer life, etc.

III. Summary

(1) There are a number of alternate promising paths in various stages of development:

(a) Cells based on other semiconductors

(Direct gap, 1.0 eV $< E_g <$ 2.5 eV, abundant constituents, organic semiconductors)

(b) Cells based on barriers other than p-n junctions

(Schottky barriers, MIS structures)

(c) "Novel" systems

(Solar thermal photovoltaic; semiconductor-electrolyte systems; photosynthetic barriers; electromagnetic wave converters, et al.)

(2) None of these alternatives is clearly so superior that it should be pursued to the exclusion of the others. Various levels of exploratory research should make possible elimination of some of these proposed alternatives.

(3) Some of the "novel" systems require better estimates of their probable solar energy conversion efficiencies and more detailed descriptions of configurations, as well as a clearer explanation of how they can be incorporated into large-scale energy systems.

IV. Conclusions

A. Semiconductor Solar Cells

(1) Sound guidelines based on theory provide a basis for selection of semiconductors with a high potential for solar energy conversion (direct gap, 1.0 eV $< E_g <$ 2.5 eV).

(2) Because single-crystal cells are likely to be more expensive than thin-film polycrystalline cells, no new programs based on single-crystal cells should be initiated unless a "breakthrough" in single-crystal cost of the material is evident. Of course, work on single crystals may be necessary as a prelude to work on thin films of the substance.

(3) In thin-film cells, grain boundary effects are likely to prevent the use of diffusion for barrier formation. Schottky barriers, MIS, or other barriers formed at lower temperatures are more suitable to such polycrystalline cells.

(4) Organic semiconductors do not promise a short-term solution to the photovoltaic solar energy problem, but their long-term low-cost potential continues to make them attractive.

(5) There is some question about the availability of Ga, In, perhaps even Cd for large-scale (a few thousand square miles of cells) systems. Clarification of this matter should precede any commitment to new solar cell systems based on these elements.

B. Novel Systems

(1) The potential for large-scale solar energy conversion of semiconductor-electrolyte systems has not been evaluated, even though such systems exhibit an inherent promise of an interesting efficiency and low cost.

(2) The electromagnetic wave converter has a promise of efficiencies in excess of 50%, but this system does not appear to be a short-term solution to the solar energy conversion problem.

(3) The theoretical efficiency of solar thermo-photovoltaic systems which, of course, require use of concentrators and combine energy storage with conversion is higher than that of currently available solar cells even with concentrators.

(4) It should be possible to produce and extract photovoltaic power from bimolecular lipid membranes containing photosynthetic pigments. There may be other possible "imitations of nature" which should be explored.

(5) Other as yet unidentified methods of photovoltaic solar energy conversion probably exist.

V. Recommendations

(1) Research should be funded on solar cells made from selected semiconductors having a high potential for solar energy conversion. These include materials with a photovoltaic history like Cu_2O, GaAs, CdTe, as well as materials previously untried in this application like Cu_2S, I-III-VI_2 materials ($CuInS_2$, $CuAlS_2$ etc.), II-V compounds like Zn_3As_2 etc. Emphasis should be on thin-film solar cells which may include both single-crystal and polycrystalline cells. Extensive materials research programs should be avoided; rather, advantage should be taken of materials synthesized for other reasons, at least in initial stages of this program. Materials research may be required after initial successes with devices suggest materials-research problems whose solution is necessary for further progress.

(2) Research should be funded on "novel" systems provided that sufficient preliminary work has been done to ensure that obvious requirements of large-scale terrestrial conversion are met by the system (acceptable efficiency, available materials, susceptible to large-scale mass production, etc.).

(3) At the end of, say, three years, a "weeding-out" process should take place. Those systems which are exhibiting an attractive near-term potential should be supported at an increased level. At the end of five years at least one system should be selected for development. This program is intended to satisfy *Objective (1)*.

(4) At the end of, say, three years, programs with a high potential for "improved" solar cells should be identified and retained in the program. Those which have not worked out by this time should be discarded. This program is intended to satisfy *Objective (2)*.

(5) An openness to new concepts, new ideas should be maintained. A fraction of funding in the program should be ear-marked for supporting promising new avenues.

(6) A national thin-film diagnostic center (or centers) should be established and maintained in support of this program.

Proposed Funding Level for Other Materials and Devices

	1974	1975	1976	1977	1978
Solar Cells Based on New Semiconductors	\$1.2M	\$1.5M	\$1.8M	\$1.8M	\$1.9M
"Novel" Systems	\$0.7M	\$1.2M	\$1.3M	\$1.4M	\$1.5M
Total	\$1.9M	\$2.7M	\$3.1M	\$3.2M	\$3.4M

If one of these systems "comes through" at any stage in the program, additional funding would be needed to exploit it, i.e., a pilot plant, etc., would be required. The budgets proposed above do not include such funds, because funds for this purpose might be directed from those called for in the silicon and Cu-CdS programs.

Other resources required: a "National Center for Thin-Film Characterization."

Discussion

Q: What happens to these costs if you are successful?

A: Let's say that the funds will be transferred over – I mean, it's possible you may turn up with something like a xerography process which sort of took off very rapidly. Obviously, if there is a breakthrough in one of these systems – if one of these systems is clearly superior – you have stability, you have efficiency in excess of five percent, and a much simpler system; then you should increase its funding.

You are not going to try to put down a cost projection for any one of them. I might have put down twenty such sets of projections; I think it would have been unrealistic. Rather, this is a situation where if one of these things comes through and the other ones fail, then you go on with the one. If, on the other hand, the other ones are working okay, and, of course, if in subsequent years, in the fifth year of the program, either the cadmium sulphide or silicon is working, then this activity is based on improved systems rather than on trying to find an alternative.

C: It just seems to me that the number of programs per dollar is very large. If there are different programs at different institutions I would guess that they would be subcritical. There are eight programs for 500,000 dollars.

A: I know. These are university programs that we are talking about, and I found that the manpower required was a matter of a few manyears to test certain of these systems. I think that on that basis, this is about what people are talking about. How I interpreted what they meant is based on my experience of about what it costs to run a program in a university.

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INSOLATION, TESTING, AND EVALUATION

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Presentation Summary

I. Introduction

This working group was composed of twelve dedicated and enthusiastic people. There was nearly equal representation from industrial corporations, universities, and federal agencies. The problems of obtaining the necessary insolation information and of testing solar cell arrays and the use of various techniques for solar cell and array evaluation were discussed. Several recommendations resulted from these discussions, and a plan for implementing the recommendations was made. The conclusions drawn in this report represent the concensus of the entire group.

II. Insolation

The lack of adequate insolation data hinders the design of solar cell arrays or other solar power systems for terrestrial use. The presently available information is of dubious accuracy and is limited primarily to the total horizontal irradiance. Errors in the data may be as much as ±20%. The direct component of insolation is obtained at only 4 of about 88 stations operated by the National Weather Service. The Smithsonian Institute has set up at least four world-wide insolation stations that obtain limited spectral distributions of sunlight in addition to the total and direct components of irradiance. The lack of direct insolation information was viewed as a major problem area for most solar power systems. However, the real needs of the various solar power systems have not been clearly defined beyond the desire for accurate values of total and direct insolation. It was also suggested that the available insolation data should be thoroughly analyzed for trends, averages, and 3-σ limits to provide a general statistical data base.

A second problem area is the need for a standard data acquisition system. The sensors presently in use should be evaluated and the need for new sensors determined. Also, the desirability of using narrow bandpass sensors for specific applications (e.g., a solar cell for photovoltaic applications) should be studied. Once the needs of the various solar power systems have been outlined, then a station capable of fulfilling these needs should be designed. Factors to be considered include choice of sensor systems, the ability to track continuously, the time interval of data acquisition, acquisition of spectral information, the need for auxiliary meteorological information, system maintenance, and the desirability of having a remote station. The possibility of using a space station or satellite for monitoring the insolation at the Earth's surface should also be seriously considered.

Finally, the problems inherent in managing and distributing the abundance of data should be carefully examined. The data output format most generally useful to the users should be established. Care should be taken that neither too much nor too little data are produced or disseminated. Also, the problems of data reduction, handling, and retrieval should be studied.

III. Testing and Evaluation

In the testing and evaluation area, the variability of terrestrial sunlight is a major problem. This variability takes two forms: first, the lack of continuous sunlight for testing purposes, and, second, the variation in the solar spectrum as the air mass and atmosphere changes. These two areas are interlocked. The first area suggests the need for terrestrial sunlight simulators for reproducible, reliable testing conditions. The spectral output of such a simulator must be representative of "terrestrial sunlight." Therefore the air mass (e.g., air mass 2) and the spectrum of the simulator must be established. Thus it is clear that the insolation and testing areas are closely interlocked, and it is clear that the spectral distribution of both the direct and the indirect component of terrestrial sunlight must be known. The reproducibility, accuracy, and stability of these terrestrial sunlight simulators must also be determined.

These considerations suggest the need for standard solar cells whose current output in terrestrial sunlight of a given air mass (or air masses) is accurately known. These cells would be used to allow reproducible settings of the terrestrial solar simulator as well as to serve as standards for real sunlight measurements.

Standardized test conditions should be established. For example, the data obtained, the performance measurements made, the environmental data, and the test location should be uniform. If a simulator is being used, then most conditions can be controlled; however, for outdoor measurements, the temperature, wind speed, and solar spectrum cannot be controlled. Finally, it may be desirable to set up accelerated environmental test facilities to allow timely selection between alternative materials or systems. The problems of setting up accelerated tests are formidable and the reliability of such tests questionable. However, this area represents a real need and should be examined.

There currently exist a variety of tools for evaluation of photovoltaic systems. These range from sophisticated instruments for research evaluation of specific solar cell device problems to diagnostic and evaluation measurements on solar cell arrays. There is a need to coordinate the various tools and to perhaps consolidate them into one focused laboratory facility so the full weight of their capability can be brought to bear on key problem areas.

One additional area was discussed in this working group that perhaps does not belong here. However, it apparently was not discussed by the other groups. In order to use solar cells terrestrially, they obviously must be either attached to buildings or to other types of framework. However, where construction is concerned, especially with homes or other buildings, building codes enter the picture. These codes can present major problems, and investigation should begin so that these problems can be minimized.

IV. Recommendations

As a result of the problem areas briefly outlined above, the following recommendation can be made. First, it was concluded that obviously there are insufficient insolation data available. However, the sensor technology base for acquiring the necessary information appears to be adequate. Therefore it is recommended that a center or a group responsible for determining terrestrial insolation be established. This center or group would have the responsibility for determining the insolation system requirements in terms of the users. They must then design both the hardware and the software for obtaining, reducing, and distributing the insolation data. They must implement the establishment and installation of this new sensor/data handling system. They must also see to the maintenance and operation of the system. In addition, the possibility of using a space platform for monitoring insolation should be investigated.

In the testing and evaluation area it was concluded that there are no standard testing conditions, no terrestrial sunlight simulators or calibrated solar cells, and there are no accelerated environmental tests for solar array or other solar power system components. Finally, there are many sophisticated tools for the evaluation of solar cells and systems that are not now being applied in a focused effort. Thus the following recommendations are made. A center responsible for system testing and evaluation should be established. The systems tested should not be limited to solar cell systems alone, but, rather, all types of solar power systems should be tested. Insofar as possible the testing conditions and performance data obtained should be made as uniform as possible to allow intercomparison of results between laboratory and actual test site data. In support of the testing conditions the necessary artificial terrestrial sunlight sources should be constructed and tested, and the supporting calibrated solar cells or other standards should be developed. Accelerated environmental tests and a testing facility should be set up to permit timely selection of materials and systems. A group should also be established for overseeing the efficient utilization of the available diagnostic tools. It was felt that all of the objectives in both the insolation and the testing/evaluation areas could best be met by establishing a national laboratory or center responsible for all of these areas. By coordinating all the groups into one area, an efficient interchange of needs, ideas, and information could be accomplished.

V. Resources

Finally, the milestones and resources necessary through FY 79 implementing these recommendations are detailed in Figures 1 and 2. No significant funds were allocated for construction of buildings. It was felt that existing laboratory facilities would provide a base for beginning operation in the shortest possible time. The program proposed has a short time frame, but it was felt that the milestones are achievable with dedicated effort. The total 5-year funding for the insolation area totals \$9.8 million, and for the testing/evaluation area the total is \$9.2 million. No funds were included for operation of the insolation network, as the form or format of this system was not known.

Discussion

Q: It seems to me that maybe a natural for this testing is the National Bureau of Standards.

A: Yes, that was talked about and the feeling was that the National Bureau of Standards is not the unit to maintain a system such as this. They can assist vitally in establishing it, because of the background information they have, but in terms of keeping it operational ---.

Q: Can you say why?

A: I'll defer to the members of the committee. They were fairly strong in their opinion on this.

C: The feeling is that to develop or to assist in the development depends on political decisions made on the method by which one would make the measurements and develop the standards – be they procedures, materials or devices – that one would use to carry out measurements. But the daily maintenance and operation of the equipment in the field is an area where we could give assistance, but where we couldn't be directly involved. An example is the way we maintain the weights and measures of systems for the various states. We give them sets of weights and it is for the states to set up their procedures for making certain, for example, that your gas station pumps gives you correct measure.

Q: May I ask a question as a bit of information? What significance does air mass two have? Is that the water vapor in the atmosphere?

A: No, air mass two is simply a definition that comes about from the angle of the sun above the horizon. Air mass two is roughly thirty degrees above the horizon. This is the quantity or the amount of air between you and the sun. Air mass one is defined as the standard atmosphere at sea level observed between you and the sun. Obviously, from the cosine law, as you go down, you get thicker and thicker layers of air.

Q: But clouds and moisture don't enter into this definition?

A: No.

Q: Before people came upon this agreement that air mass one would be such and such, was it because a more precise definition of optical path length needed to be considered?

A: Well, I think that air mass is basically the pathlength.

C: Not quite. Certain processes of absorption are of the scattering type, and therefore they tend to saturate. You can't say the cosine of 60° is one half.

A: That's right, because you have refraction effects and things like that, but basically air mass is geometric. It does not concern the constituents in the atmosphere.

C: But it isn't simply the cosine.

A: I know it.

Q: Would air mass one, say, in Delaware, be different in January than in summer time where the thickness of the atmosphere is changing with the seasons?

C: Air mass one was measured by Charles Abbott from the National Bureau of Standards, who did a lot of study on the solar spectrum. I remember he used to go to Mt. Kalama in Chile for the original measurements of the solar spectrum and the definition is for an atmosphere that has no clouds, where there is not moisture in the atmosphere (it was dry in Chile), and where there is no dust in the atmosphere.

Q: Well, that would bring it up somewhat above sea level.

A: There are fundamental physics problems that we are getting into there that are real.

Q: You were going to mention about the laboratory, then you kind of backed off of it.

A: We feel that there is a need to establish a national laboratory probably both for insolation and test and evaluation; these could be combined under one roof. The group was not terribly strong – I don't know whether it was exhaustion at that point or what – they were never very strong in suggesting that we have a laboratory for testing and evaluation and a laboratory for insolation. They also had the feeling that these could be combined into one group. I do not make that recommendation, but that is what we came up with eventually.

Q: Does your program include all the requirements for any solar power system, like solar thermal, for example, so that the budget and everything else would include the documentation, needs, and so forth, for the other systems?

A: Yes. We did not want to structure this just for photovoltaics, but for all solar energy systems.

Q: It seems to me your suggestion of maybe thirty monitoring stations is on the sparse side ---.

A: We had a hundred monitoring stations and we thought it might go up well beyond that.

Q: That is still only one every 30 or 40 thousand square miles. Do you have some candidate places for these laboratories? Do they have to be built from scratch or are you thinking of some present NASA centers as a place to put the ---?

A: We didn't go into that detail.

Q: It seems there are something like a thousand meteorological stations. Aren't these logical places for at least collecting the data that you want and using the personnel associated with the Weather Service? Perhaps a lesser number of places for analyzing the data – do you have objections to that?

A: Currently the insolation network that is run by the Weather Service has about 88 stations scattered around the country. We don't object to using them at all, but they are going to have to be modified with sensors, and as soon as we start changing the sensors, we come up with the real question of who cares what you get for Charlotte, North Carolina, or wherever. We've got to define the locations and that's why we chose not to go into the details of it; rather, this is the responsibility of the organization.

C: I think that you can find at least three government agencies which will bid for operating this thing: the Weather Bureau, maybe NBS, and NASA. As an industrial contractor, I always like to see at least three bidders. I would like to suggest that the test part of your program is very important because we are starting out in a business where we have no "Good Housekeeping Seal of Approval," no NASA test facility, or whatever might be used to standardize environmental packaging of these systems. And I think that one of the biggest variables the customers are going to find in the beginning is varying degrees of survivability. We know that our silicon solar cells can last a long time, but maybe something is going to attack the contacts, the plastic or glass is going to degrade, and so on. I think we need some standards pretty badly in this area along with meteorological data.

A: We agree 100 percent.

C: About a definite recommendation as to where the center should be, political realities are such that I think it is premature for us to state that it is going to be one agency or another. It's something that would involve people who, as the saying goes, would like a piece of the action. And, also, the views of the Office of Management and Budget will be important.

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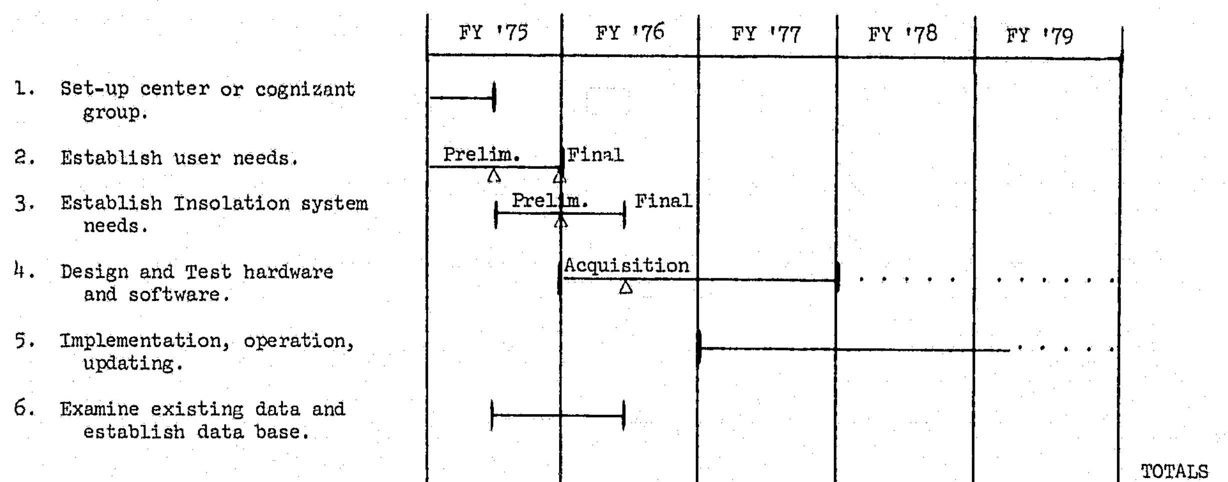
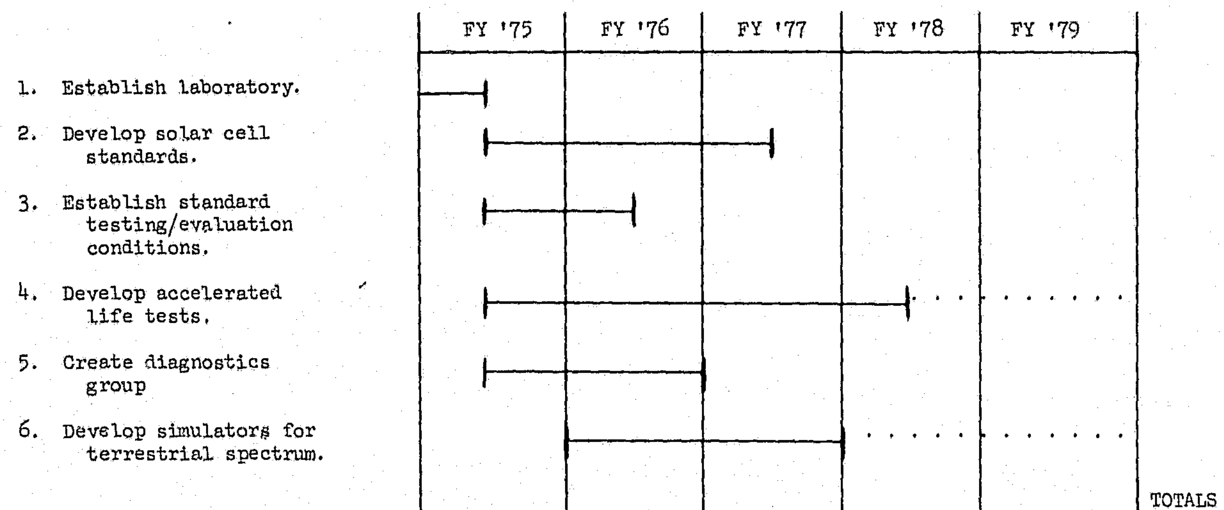


		FY '75	FY '76	FY '77	FY '78	FY '79	TOTALS
RESOURCES:	R&D	$1.0M	$2M	$1.5M	$1.5M	$1.0M	$7.0M
	Hardware-30K/unit ~ 100 units	0.1	0.2	0.5	1.0	1.0	2.8
	System Operation			?	?	?	?
							9.8

Fig. 1. Insolation Milestones/Resources



		FY '75	FY '76	FY '77	FY '78	FY '79	TOTALS
RESOURCES:	R&D	$1.0M	1.0	1.0	0.8	0.7	$4.5M
	Hardware and facilities	0.2	1.0	1.5	1.5	?	4.7M
							9.2

Fig. 2. Testing and Evaluation Resources/Milestones

SYSTEMS

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Presentation Summary

Introduction

The systems working group consisted of five representatives from government agencies, four from utility companies, four from aerospace/systems companies, four from solar subsystem industrial suppliers, and three from universities. All of the subjects that could possibly come under "systems" were not discussed, and an effort was made to address only broad problems associated with photovoltaic system applications. For example, technical problems involved with incorporating cells into arrays were not addressed.

Present System Costs

The present commercial selling price for small photovoltaic systems is about $20-30/watt. This price includes battery storage and is presently competitive for remote power systems. Long-term life data is not yet available for these systems. It is expected that a 1-kW system, involving some concentration, will be available on the market next year at $5/watt. This system includes the complete costs of the array and tracking system but does not provide any power conditioning or energy storage. Large inverters and storage batteries are presently being developed for utility applications. Large DC to AC inverters have a present price of $30/kW. Large batteries for load leveling that are capable of 3000-6000 cycles and 80% efficiencies are expected to be available on the 1980 market at a price of $80/kW plus $2/kW for each hour of desired storage.

It appears that it would be more appropriate to use photovoltaic array cost figures ($/watt) rather than solar cell costs. When low cost cells are made, it is most likely that encapsulation and incorporation into arrays will be an integral part of that mass production process. Also, with systems using concentration, it is only the array cost per watt that is important and not the cell cost.

Major System Unknowns

Photovoltaics, like other solar systems, needs more complete insolation data before meaningful system studies can be completed as to the applicability of using different types of systems in different sections of the United States.

Although there is a great deal known about the life of a silicon cell, very little data is available on terrestrial array lifetimes. Since the final configurations and characteristics of cells are not likely to be much different from present cells, it would seem likely that arrays could be presently fabricated and meaningful testing started.

A major unknown is the cost goal that a photovoltaic system has to achieve to be competitive. This can only be estimated after a rather sophisticated system study evaluating the various alternatives of how a photovoltaic system would be incorporated into existing systems. It also involves a study of what effect environmental and fuel constraints will have on competing systems.

Conclusions and Recommendations

System studies should be started now in order to feed back information to influence component development.

Complete insolation data and array life test data are needed badly and should start immediately.

Concentration advantages and limitations need to be investigated and can be started immediately.

The initial demonstration photovoltaic system may likely be for a commercial complex or a utility substation because of the advantages of load averaging and decreased requirements for storage compared with lower power applications.

At array costs of $0.50/watt, the array still dominates the system costs for large-scale systems. It would then appear that major attention should not be given to the development of storage or inverter devices in the photovoltaic program, but rather the adaptation of these technologies to photovoltaic systems.

There is a need for either government or utilities to establish and stimulate a market base over the next few years upon which a photovoltaic industry could progressively develop competitive systems.

There is a need, especially from the user's view, for a rather large (100 kW) array to be built and tested in a field environment in order to obtain real system data.

Milestones and Resource Requirements

Figure 1 indicates the schedule and minimum resource requirements that the systems study group recommends. It is felt that projections beyond five years is not very meaningful, because what is learned during that time will direct the efforts required in the systems area. The resource figures shown are considered to be the minimum amounts from a government agency which would hopefully be supplemented by utility contributions. It also presumes that many of the studies being done under other solar programs can be used and would not come out of a photovoltaic budget.

The major effort that needs to be started immediately and continued is that of system studies. These studies are needed to identify the quantitative advantages of different size systems meeting various requirements. How does a residential application compare with a commercial complex or utility substation application? What are the alternatives of combining a photovoltaic system with a thermal supply system or hydrogen generation system? How would a photovoltaic system interact with an electrical grid with or without storage? Would load following be possible with limited storage devices? What type of storage is best coupled with photovoltaics? What are the social and environmental impact and "worth" of these systems? What are the relative advantages of using existing roof areas from the public acceptance as well as the economic view point? How sensitive on life cycle costs are system lifetimes and degradation rates? These and many more questions can be investigated somewhat independent of cell development, but could be just as important for bringing photovoltaics into the terrestrial market.

The second effort that should be started right away is to identify the advantages and limitations of concentration for photovoltaic systems. This is definitely a systems problem, can start immediately and would complement the programs investigating cheaper cells.

The third effort that could be started now is the building of field test systems which could act as system experiments for data collection. It was felt that these arrays should produce about 100 kW of power so that the information could be scaled down a factor of 10 for residential systems and up a factor of 10 for utility systems. At least two of these systems should be built in quite different sections of the country and should be incorporated into an existing electrical grid to provide experience and information on an actual working system. These systems would not be designed to be economical, but, if successful, could be turned over to the user for continuing operation after the initial testing is completed.

Based on the experience gained during the building and operation of the 100-kW units and the advances made in other photovoltaic research, a preliminary design could be started for a large prototype photovoltaic system in the megawatt power range.

Discussion

C: I'm a little concerned about the 100-kilowatt size being large enough to really be a proof of concept. In my discussions with some of the utilities – I guess there are enough here that they may be able to answer this – a substation beyond 20-megawatt or 10-megawatt size looks like more of a good building block insofar as a proof of concept.

A: Yes. This experiment is not a proof of concept of the economic feasibility. It is really only a data collecting experiment. The thought of 100 kilowatts was that it is within a factor of two of being scaled down for a home-type use, and within a factor of ten of being scaled up to a sort of substation in the megawatt range. The utility members did think that that would be a meaningful size.

C: I'd like to add a little more to your answer. The question of size in a real-life plant is very important, but when you are talking about showing how it works or putting all of the pieces together, it is not as critical. A hundred kilowatts is actually a quite large system when you compare it with the cost. Recently, Public Service Electric and Gas of New Jersey installed three 12.5-kilowatt fuel cells in a substation in Newark, and ran these things for a couple of months as a demonstration that you could put all the components together and run them. This program was developed by the utilities in conjunction with manufacturers. So it's not necessary to go to 20 megawatts.

Q: Did I understand you correctly to say that you plan to have quite a bit of your plant done and built by 1976?

A: That's correct.

Q: If so, do we have the productive capacity to produce the cells?

A: Yes, we have a solar cell supplier that would be very happy to take our order.

C: I think the solar system, as opposed to a thermal system, does not need a size of a megawatt to be economical. If you have a one, five, or ten-kilowatt system sitting on a rooftop of a house, it may be as economical as a 100-million-kilowatt plant in the desert. I think both concepts need to be studied very carefully so that one has something which is proof of concept as soon as possible. We have Solar One already established and we are taking data. We hope to get financed to use this in a bit more sophisticated way than we currently do, but I think one has to be careful not to push for too large a project at the beginning. It is very important to get data from both sides at an early time.

Q: It seems to me that somewhere the array problem has fallen through the crack. We thought you were going to take care of that. For example, you say fifty cents a watt still dominates the systems cost. Does that include the array? We're talking about fifty cents a watt for the cells, and it seems to me that one of the studies that will have to be made is array cost versus efficiency. There is an efficiency tradeoff with implications that low efficiency might be a problem. Some people think that the cells might be only a fraction of that – it depends on the price range. But has anybody looked at the array problems and should not that be a recommended program?

C: You should have explained that we just picked one small area last night and by no means did we cover the systems problems entirely. We just took one subprogram and approached that. We didn't have the time to go into all of the other programs that could be developed. The problem of a home with one or two kilowatts on it is a vital problem, for instance. The array cost is a systems problem, but we did not get a chance to investigate that.

C: I presented a figure of \$550 per kilowatt installed yesterday, and I am rather pleased to observe that the systems panel has already taken \$150 off that figure. The one figure that I would like to get some verification of is conversion cost, because that does sound somewhat unrealistic to me – \$30 per kilowatt. We have taken a figure of \$100 and that's pushing it.

A: This would certainly depend upon the size. We were taking all of these costs for storage and for inverters in large size systems – in the megawatt range.

C: The \$5/watt system that was quoted for delivery in 1976 – that unit is a selling price breakdown of this hardware. Over fifty percent of the money is involved in things that have nothing to do with the hardware: things like overhead, transaction costs, and so on. So the labor, material, and things like that that are in that array will run \$2500 of the \$5000 down to approximately \$1600 or \$1800, depending on the quantity built. Now if you took the number \$1600 as a hard cost for an array of this design, approximately \$500 of that is in solar cells. They are fifty cents a watt, but if you go to the State of California, you are going to find out what kind of wind loads there are on freeway signs, for instance, or other kinds of things that blow down. You will find that there are a lot of costs associated with the structure. It is proportional to the square feet involved and the wind loads that are expected. As I said, the square footage problem is a very important thing. You don't hold it up there for nothing, even on the roof of a building. Now these loads cost money per square foot; we have material-labor type costs that are in the $5000. Only one third of this is in solar cells and the other two thirds are things like encapsulation, interconnection, structure, etc. Five hundred dollars per kilowatt then sounds to me like fifty cents a watt.

Q: Can we ask Dr. Böer to comment on this?

A: On what?

Q: If you have a fifty-cent a watt solar cell, what will the array cost? How much more must you add to integrate these cells into an array?

A: The factor we have there is about three. The cost per cell of one dollar per square foot comes up to about $3.20 or $3.30 per square foot of array – encapsulated, sealed, and installed. Now we are subtracting from this square foot price whatever is normally on the roof, so we come up with $2.30 or so in our quotations.

C: I would like to make you aware that when the Washington Gas Company was studying proof of concepts for fuel cells, they chose to install them in some model homes so that the public could see them – not on a large commercial office building. I don't think you should rule out the residential market for proof of concept.

A: I think that public exposure is an important thing, certainly. A place where people can visit and see a meter on a dial and that sort of thing was talked about to some extent, but the thought was that it is perhaps more meaningful to see a meter move on a large power dial and still have a place that people can visit and see.

C: I'd like to refer to the Westinghouse study that came out with $550 per kilowatt. As a result of that, 6.4 mils per kilowatt hour was calculated as a power cost and any calculation that I could make is way off, even though that number might be correct in certain applications. I come up with at least 60 mils per kilowatt, and this is based on 1972 dollars. In a power plant structure of any size, an attempt to include the escalation and construction costs is not made. Yet in the conventional system, this adds 25 to 30 percent of the cost, depending on the time of construction. So lots of things were left out, and it makes me kind of curious as to the basis for the calculation for the 6.4 mils per kilowatt-hour.

A: I think the whole costing area is pretty difficult to assess until you know how the photovoltaic system is going to be utilized.

C: I'd like to read this data, which is the only data we have that's been presented that I recall about the relative size of these different elements for building a power source of significant output. These figures are approximate, since they add up to 110 percent. But the cells are on the order of 20 percent of the total system cost. Now we have been talking about spending hundreds of millions of dollars, focussing on this obviously critical area. But it seems to me that you've got to keep in perspective the fact that the cell is only 20 percent of the total cost of the finished system; otherwise, you're fooling yourselves. As a little example of what this can mean, if you compare two systems, one with five percent efficiency and one with ten, or one with ten and one with twenty, you immediately see that the construction cost and support cost is going to double if you halve the efficiency. Or it will go down by a factor of two if you double the efficiency because it takes half as much area. That means that you can pay almost twice as much for the cells if it has twice the efficiency and still end up with the same total unit cost. So one of the things that concerns me is that I don't see how you are going to be able to use cells of low efficiency in significant size power installations. I just don't think it's going to be practical.

C: The ten cents does not represent peak watts in the Westinghouse calculation, it calculates over twenty-four hours; and, secondly, this was very much of an order of magnitude type of a calculation. All I was concerned with is to show that we are in some sort of a ballpark range. Differences in estimates on storage, conversion costs, etc., make a difference in the final system cost.

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July 1, 1974	FY 75	FY 76	FY 77	FY 78	FY 79	July 1, 1979
Effort 1	$2M	$2M	$2M	$2M	$2M	
Effort 2	$0.25M	$0.25M				
			Complete			
Effort 3	$0.5M	$2M	$0.25M			
	Fix Design	Complete Construction	Continue User Operation			
Effort 4				$1M	$1M	
			Start Preliminary Design			
Totals	$2.75M	$4.25M	$2.25M	$3.0M	$3.0M	$15.25M/5yrs

Effort 1 - system studies

Effort 2 - Concentration data and limits

Effort 3 - Design, test and operation of at least two 100 KW systems

Effort 4 - Preliminary design of megawatt prototype system

Fig. 1. Milestone and Resource Requirements

PANEL I.
INDUSTRIAL ASPECTS OF LARGE-SCALE PHOTOVOLTAIC UTILIZATION

PANEL I. INDUSTRIAL ASPECTS OF LARGE-SCALE PHOTOVOLTAIC UTILIZATION

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Having attended this session for three days, I'm rather heartened by the fact that we have several paths for solving this photovoltaic problem. And I have the feeling that probably if man is going to stay comfortable on this planet in future centuries, he is going to have to use the sun. I also have a basic faith that, coming from industry, if we once solve the problems that we still have, if we characterize the product that we want built, industry will be able to make the cells, and fabricate them on an automated basis. Now this is pure mechanics from that point on, but the mechanics cannot be solved until we have completely characterized the product that we want to put into the machines. We are not going to characterize the product before we go through quite a few adaptation exercises and determinations of what the product is going to be, or which one of the systems is going to prevail — and that in turn means that we've got to get started now. There is a lot of work to do in the next five years. It has been said that there is a lot of money needed, which is absolutely true. That means there are a lot of decisions that have got to be made right now on a relative basis.

I believe that industry can do its job. I think that the speed with which it will do so is going to be determined somewhat by how we go at the marketing. If it is going to be a natural marketing process, there's going to be a lot of reluctance to use the product because it is different. If you are talking about houses, it's completely foreign to the architect who is used to a lot of other materials. His profession doesn't even know what its all about, and he isn't going to necessarily think of it as a natural building block in terms of building houses or factories. It is going to take some time before we can ever get to this point. I think if you are talking about central systems, there is bound to be some reluctance on the part of utilities also, because it is getting into strange areas where they have to do some betting, rather than working with facts with which they are very capably operating today. On the other hand, if it is important enough, then the government has a basic decision to make, I suppose, if they want to really operate on a mobilization basis. If they do, then it is certain, up through the first five years at least, that there will have to be some decisions made rather rapidly. They will have to serve as a customer and thereby provide incentives to users or become users. There have been socialistic experiments, or so claimed, in the TVA area and in other areas — the building of highways was mentioned. There have been various analogies where the government has felt strongly enough to become the customer, and I think that it all depends on how much stress we have on finding a solution. Certainly when we hand these things over to industry, they will rise to the occasion, and they will get their "E" as we did in the last war. But it's going to take a lot of money and effort.

Do we need a mobilization effort, or do we go at it by whatever competitive means and market sales we may have? I think these are decisions we need to make. We've talked about whether we want them in houses or central. This is way out, but it is the type of thing we ought to think about — we can think about using waste space for our power in the West, and so forth. I'm not certain that power in those places is the best place because we have a large transport cost. Therefore, why not grow the food out there? That land will produce, and you can store and ship food very nicely, and then use the area for our power where we are now growing food close to our cities. Secondly, I don't think that we necessarily have to talk about energy storage. If we will turn this whole thing around, let your primary source be the sun, even if it is in a much smaller amount. But use it when it's there and your storage then automatically becomes your system like the other present nuclear or fossil fuel systems, or whatever it may be. A third point might be that one of the easiest ways to use the power with no conditioning at all, practically speaking, would be by resistance loads, and I suspect in industry we've got enough furnaces, ovens, etc., that we could use direct resistance. I don't think the resistance oven cares whether it's got an ac or dc input. I think that we have great possibilities if we will do a little thinking along these lines.

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The history of solar energy has been one of speculation to date. Recently, the preponderance of this speculation has been of a technical and cost nature.

Proponents of different technical approaches for generating electric power can be funded and given a program plan. They should be able to show progress or failure within a reasonable time. Failure will usually be the result of underestimation by a university or individuals of the task to be accomplished, or, if the failure is by a major reputable corporation, the result is usually due to the true difficulty (or impossibility) of the technical task.

Of those solar power generation schemes that prove capable of reliably generating electric power from solar energy, some tests must then be made to determine costs. These costs must somehow be related to what price the electric utility companies or individuals will pay for electric power in the future. At the present time, the question of what the costs of large photovoltaic systems will be in the future is open to considerable speculation. Proponents of the systems that do not work very well claim that if they can break through, their systems will be "very cheap". Cadmium sulfide systems have been in this category for 15 years, with little perceptible change. Proponents of systems that work well and are in extensive use, such as silicon, are generally projecting large cost reductions from the current market price of $35 per watt. Some of these cost reductions are claimed to be due to design improvements, and additional reductions will accrue due to automation of production as a result of eventual production in the very large volume required.

It is obvious that for photovoltaic systems that do not work very well and are not bought and sold in the marketplace at the current time, a decision will have to be made as to whether to continue or discontinue additional funding on an individual basis. Systems that work well and are in common use, such as silicon single crystal cells, should be expanded to the next state of testing; that is, the testing of the thesis of large cost reductions. I would suggest that this be done in several stages, but that little time be wasted in establishing the lowest cost possible for any given volume of product and verifying that these costs can be met.

There is another side to the cost picture that relates to the price the electric utility companies or individuals will pay for electric power in the future. The answer to this question has not been determined and is not likely to be determined with any degree of precision within the next two years. However, we do know that prices paid for oil in the Middle East have soared. The cost of fuel as a part of generating electricity goes from insignificant to a very significant factor larger than the capital investment cost of a fossil fuel power plant. It is my opinion that during the next few years, we can expect the cost of electricity generated by burning oil to rise to somewhere between 5 and 10 cents per kilowatt hour. Some people might say that nuclear fuel reactors will prevent this. However, a rise in the cost of fossil fuel electric power will give the atomic proponents an opportunity to raise the prices from the presently quoted levels of $500 per kilowatt installed to a point somewhat closer to Ralph Nader's overall cost estimate of close to $1,800 per kilowatt. As a result of the honest costing of fossil fuel, which is an irreplaceable resource, we will have honest costing of the total program required to generate power by nuclear sources. The result will be an environment that encourages and motivates companies to look for alternatives, and one result will be a great upsurge in the use of coal or gassified coal to generate electricity.

Motivation such as this price increase is the key factor in encouraging private industry to invest. However, investment decisions are of an individual nature, and are rarely made when the market situation is not stable. This problem of providing an environment in which investment decisions can be made over the long haul is the key problem facing the management of the United States of America. If stability cannot be created and higher prices cannot be tolerated by politicians, then a crisis of indecision will result, and no large investment by private industry will be made until the investor is sure of a payoff. Thus, if oil from oil shale can be produced at $8 per barrel, because of the enormous time required to build up an oil shale plant, nothing will begin as long as the threat of Arab oil prices being dropped to $7.50 can occur. At the present time, our government is encouraging low prices to placate homeowners and other voters.

Photovoltaic electric power is a higher risk than other electric generating systems currently available, such as nuclear and coal. Large-scale investments are required to produce enough solar cells to generate a significant amount of electric power. Small solar power businesses will be generated and grow slowly, but the current environment characterized by indecision, fluctuating prices, political rhetoric, will not generate the confidence required for a privately funded photovoltaic program. There is no question but what silicon solar cells can be made to generate electricity for capital cost of less than 50 cents per watt by 1990. However, for this to start happening, the present photovoltaic business must be doubled each year in size for at least 15 successive years. Even at that time, the annual rate of production from this new industry would provide only 5 per cent of the nation's power requirements. The major problem facing the photovoltaic community is recognizing the size of the industry that must be generated to build even a very small portion of the U.S. electric power generating capacity. The City of Los Angeles alone uses more than 1.35 billion kilowatt hours of electricity per week.

There are three routes that can reduce the cost of silicon cells to competitive levels. The utilization of any one of these three approaches can potentially produce electric power at prices competitive with other forms by 1985 to 1990. These three design changes include low-cost polysilicon material, continuous growth of single crystal silicon material, and concentration of sunlight. Improvements in two of these areas will assure the financial success of silicon photovoltaic converters in the long run. It is essential that the Government not only continue funding of potential breakthrough technology, but that continued funding be provided to accomplish some specific cost goals and expand the average size of photovoltaic electric power systems deployed in the field. This work must be done in conjunction with electric power companies for the express purpose of helping them understand how to relate the cost of solar electric power systems to conventional and nuclear power plants. None of this work has begun yet. At the present time, electric power companies who are interested in developing this source of energy have no way of comparing it to baseline plants, nuclear reactors or gas turbine peaking plants.

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The use of single crystal silicon solar cells for large-scale photovoltaic conversion holds considerable promise. Silicon supply requirements for this application, however, are very large; the following discussion is designed to illustrate the magnitude of this requirement.

Consider one square mile of 12% efficient (under practical operating conditions) solar cells. In the U.S.A., on the average, each square foot will generate 2 W, so the one square mile generates about 50 MW.

Assuming the cells are 8 mils thick (for self-support and low-breakage processing), the amount of silicon for one square mile = 1200 tons.

Present U.S. electrical generating capacity equals 400,000 MW. Therefore, one square mile $\approx 1/80$ of 1%. To replace present capacity would require 8,000 square miles, or an area approximately 90 × 90 miles. To replace 1%/year of current U.S. electrical generating capacity would require 80 square miles/year, or about 100,000 tons of silicon/year.

Assume \$1/W; the cost per square mile equals \$50 million. To replace U.S. electrical generating capacity at 1%/year would cost \$4 billion/year. At this rate, the solar cell industry would be comparable to the total semiconductor industry.

To reach the \$1/W range, new methods for obtaining single crystal silicon in sheet form will have to be developed; growth of ribbon directly from the melt is a promising concept. Assume growth of silicon ribbon, 3 inches wide, 8 mils thick, at 6 inches/minute; this would be considered a challenge considering the rather stringent requirements on silicon for efficient solar cell fabrication. In one year, such a ribbon growth machine would pull 1/500 of a square mile. Therefore, one square mile/year requires 500 ribbons growing; to replace 1% of U.S. electrical generating capacity would require 40,000 ribbons growing at the same time.

One square foot of silicon, 8 mils thick, weighs ≈ 50 gm. = 1/20 kg. Raw polycrystalline costs \$60/kg, so one square foot × 8 mils would cost \$3. It is expected that raw silicon costs can be reduced by a factor of 5 to 10 (very high volume, single specification of fairly low resistivity), so the silicon cost for one square foot should be expected to fall to ≈ 45¢.

This leaves $\approx$ \$1/square foot for (loaded) production costs, so continuous processing is a necessity. This appears feasible. A rough estimate for the equipment costs (not including development costs for the equipment) would be \$100,000/ribbon. Therefore, one square mile/year would require \$50 million capitalization; to replace 1% of U.S. generating capacity/year would require \$4 billion capitalization; i.e., capitalization $\approx$ annual sales.

The storage problem could be sidestepped by a worldwide solar generation system, all tied together. This would require long-distance (i.e., 10,000-mile) transmission; the development of liquid H_2 temperature superconductors would seem to make this practical.

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Most companies are looking at short term projects for their new enterprises. Executives are being measured on their ability to demonstrate solid achievement in two to three years. When they are presented with a twenty-to-thirty-year payoff, or even ten-year payoff, they become very uninterested. Long range projects present great risk and investment capital will be scarce unless there is to be a very large return in the major line of their business – a business that they know. Does this say that it will be more reasonable for an energy company or a utility to invest in a solar cell plant, a technology that they are unprepared to go into, or rather that semiconductor companies that can do the technology should get into the power business? Companies will undergo major change only under crises. Industry has to take chances, but cannot take failure. Failure can very often take other very important projects under. There is a regenerative effect that takes place that increases the risk. A short time ago the gas and oil industry was abandoning badly needed research and development because of high risk, high cost, and low return on the investment. We have a chicken and the egg problem here, where we need industry to make a large commitment to prove feasibility, but the decision to go with it may be negative, depending on many extraneous factors. What is needed? I have outlined a number of problems here.

First, the problem has to be defined very carefully. I think that the purpose of our meeting here is an attempt to do that. The direction needs to be established and feasibility shown. The degree to which this takes place will set the interest level on the part of industry, for then the risk is lessened.

Second, the government must take several important positions. A long-range commitment is required. No one wants to tool up for massive production only to find that in three to five years the interest has waned. The government will have to establish strong financial and moral support to reduce the risk and convince top executives that they mean business. It should be pointed out that under most conditions, a company's financial contribution is great even under very heavy government financial support.

Third, the incentives must be substantial so that future profit is assured. This bears on the amount of regulation imposed on the industry and what the government expects in return for its investment. Patent considerations and tax incentives have to be thought out.

Lastly, the long-range nature of the program must be assured, independent of political factors. Perhaps the creation of a Department of Energy in the Federal Government structure would solve this one. Industry-wide planning and cooperation is needed to solve this problem. Pooling of interests in independent energy enterprises should be considered. This could involve government, industry, and universities. National laboratories, similar to those that the AEC or NASA has developed, may be necessary if all else fails.

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In covering the items to be considered by the panel I shall give my opinion on these matters in the order appearing on the original agenda.

1. Materials: for those solar cells upon which most research and development has been done, namely silicon and Cds-Cu_xS, materials should present no problem given a probable installation rate extended over a period of years.

2. Automated/mass production techniques are under development; some were described in the paper presentations.

3.-4. Recycling and waste recovery are necessary parts of the automated mass production and should present no serious problems.

5. Capitalization: the total electrical generating capacity of the U.S. is estimated to be over 400 million kilowatts at the present time (it was 359.6 million kilowatts in December 1970). A conservative estimate of its replacement cost (at $250/kilowatt) would exceed 100 billion dollars. The total investment in the electrical system of the U.S. is of course much greater since the cost of the distribution system is not included in the above totals. Of the total installed capacity nearly 80% has been privately funded, and the current rate of private investment is much higher than that of the government.

The electrical generating capacity of the U.S. has been approximately doubling each decade. This rate of increase obviously cannot continue indefinitely, but a NSF-NASA study predicts a 5.4-fold increase over 1970 by the year 2000.

Between 1969 and 1970, 27 million kilowatts were added to the national generating capacity at a cost of approximately 7 billion dollars, of which 84% was investor financed.

What is significant is that private industry has been able to finance our present generating and distribution capacity through its period of growth with little difficulty.

6. Role of government: the difficulty that does exist in our present situation, namely, the necessity to develop solar sources to replace fossil fuels, is that industrial managers (of which the speaker is one) find it difficult to face the risk of an extremely costly development with the earliest possible return scheduled a decade or several decades ahead.

I have totalled up the suggested development budgets presented at this conference, and it exceeds 400 million dollars to be expended over a period of 12 years.

With a variety of short-term approaches to the energy problem confronting him, together with the risk and long period before return, it is difficult to see industrial decision makers investing in the necessary research and development to make solar energy a reality. This is the case, even though the more thoughtful of them know that in the long term, the use of solar energy is inevitable for a variety of reasons known to everyone here.

In view of the above, it appears that only government is in a position to finance the solar energy effort at this time.

When it has been demonstrated that it is technically and economically feasible, I have no doubt that private industry will be eager and able to finance its adoption as they have the formation of our present power system.

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A common characteristic of all methods of utilizing solar energy is that they require the interaction of this energy with matter.

The efficiency with which this interaction takes place has a direct bearing on the efficiency and practicality of the overall system.

For many systems, single or multilayer optical films of thicknesses of the order of the wavelengths of sunlight can be used to control the interaction between solar energy and matter. Examples would be: reflectivity enhancing coatings for concentration reflectors, selectively absorbing coatings for thermal receptors, antireflection coatings for windows, and perhaps the photovoltaic systems themselves.

Typically, coatings of this type are obtained by evaporation or sputtering in a vacuum chamber.

It is therefore appropriate in this gathering to review the state-of-the-art in large scale deposition of thin films.

In 1967, my company started design and construction of a very large multilayer automatic coater to which we applied the acronym MAC. In this machine, racks of glass up to 2½ X 4 feet are cleaned by an automatic washing machine and fed by a conveyor through an entrance lock into a vacuum chamber over 100 feet long. This chamber contains glow discharge and heating sections and six specially designed electron beam evaporation sources that are capable of continuously depositing metal or dielectric materials at carefully controlled rates. The racks of glass pass sequentially over the sources, receiving up to six layers of coating material before leaving the machine through an exit lock. All machine elements are designed to operate for at least a week without service or maintenance, and coating rates as high as ten square feet per minute can be achieved.

MAC has now been in production for almost four years. It is operated three shifts, seven days a week, producing a variety of products, and even after allowing for scheduled and unscheduled downtime is producing coatings at the rate of about 1½ million square feet per year. Some of these coatings sell for as little as two to three dollars per square foot, which is an order of magnitude less than can be obtained with conventional batch coaters.

Our confidence in this approach to high volume production is such that MAC II is already under construction. MAC II will be larger than MAC I, will have more coating sections, and will be capable of more sophisticated multilayer coatings.

In addition, we are proceeding with plans and research programs directed towards MAC's III and IV, which we expect to follow closely behind MAC II.

I feel confident that when the solar energy system requirements for optical thin films are defined the technology for economical large scale production will be available.

G. W. Wiener
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Westinghouse Electric Corporation is a major supplier of electrical equipment for the generation, transmission, and distribution of electricity. To maintain a leadership position, we must be at the forefront of any technology likely to be required and used to meet the growing demand for electrical power. Consequently, we have supported and will continue to support research in promising areas that are likely to help satisfy the energy needs of people. One of these areas that deserves major attention is solar energy.

At the risk of some oversimplification, we can roughly divide our technical requirements into two parts: that which is absolutely essential to be accomplished between now and 1985, and that which is likely to have impact beyond 1985. In the first category, we suggest that it is of utmost importance to develop a successful demonstration breeder reactor and the necessary coal gasification system to provide clean fuel from coal. We believe that a sound technical and economic basis exists for both developments. Vigorously pursued, we can expect that both the breeder reactor and a coal gasification combined cycle plant will be available to the electrical industry as we enter the 1980s. At the same time, looking toward the period beyond 1985, we consider it essential to do the necessary research and development to explore the potential of nuclear fusion and solar energy. In the case of fusion, both scientific and economic feasibility are yet to be demonstrated, whereas solar energy for power generation is still unproven economically. If solar energy is to be used for electrical power generation, much research remains to be done. To gain some perspective of the magnitude of the economic problem, it is important to understand the present economic situation. The total capital cost of a 1,000 MW nuclear light water plant is projected to be between $500 and $550/kW in 1981 dollars. The nuclear steam supply, turbines, generators, and associated electrical equipment represent only a fraction of the total installed cost. For a combined cycle plant with a coal gasification unit, we believe that the costs will be less than for conventional coal-burning plants with stack gas scrubbers. Furthermore, the potential exists for significantly higher efficiency in the combined cycle plants. Both uranium and coal are available in the United States in abundant supply; thus, electrical energy is assured for years to come. Even though both uranium and coal can meet our needs, it is important to maintain as wide a base of technical options as possible. This country cannot afford a technological lag, and solar energy may be a viable source of energy in the future. However, to make photovoltaic solar cells a factor, the emphasis must be on reducing the cost of the cell. Present-day costs of approximately $100,000/kW for a solar cell are completely unrealistic for any large-scale application for the generation of power. A cost reduction of three orders of magnitude is required. While improvement of efficiency is desirable, the emphasis must be on cost and reliability. Our own research is directed toward these objectives. Furthermore, we believe the research emphasis must be on the cells, and diversion of effort toward demonstration plants using photovoltaic devices would be premature and of questionable value on the basis of any cost/benefit analysis. On the other hand, initiation of system studies to identify potential problems is appropriate.

While this workshop has been concerned primarily with solar photovoltaic devices, we also support efforts on thermal collectors and systems involving heat engines. At this stage of solar energy research, it is necessary to pursue this approach with appropriate effort. Thermal conversion has been used for some applications around the world, principally in Japan and Israel for water heating, with some degree of technical and economic success. It is important to learn if thermal conversion can be used to generate electricity economically. Though solar energy may someday be used on a large scale for the generation of electricity, it is not possible to predict with any certainty as to when it is likely to be accomplished. Furthermore, the investment required is uncertain. These conditions strongly suggest that the Federal Government must fund solar energy research since the business risk is high. It is also reasonable to expect that the Electric Power Research Institute will take a role in the program. The private sector can be expected to develop capabilities, both people and facilities, to carry out research and provide for the implementation of successful developments as they occur.

R. Larson, Guest Speaker
House Science and Astronautic Committee
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I would like to discuss four different areas: (1) something about my background, (2) some recent solar history in the legislature, (3) a bill that has just been introduced, and (4) a request for your help in preparing for the next item that will come out of the Science and Astronautics Committee – the Solar Incentive Act.

I am the IEEE Congressional fellow and there are six of us in Congress – three from IEEE, two from the American Physical Society, and one from ASME. I have been on this job for four weeks, so I am not much of an expert. The point I want to make is that Congress needs a great deal of help. There are not very many people working in any technical area, much less solar, and if any of you have any influence over professional societies and can find ways to give Congress free help, I would urge you to do so.

The second point is on recent solar legislative history. You are probably aware that the major energy concerns in the Senate are handled in Jackson's Interior Committee. He did not take up any solar matters in that Interior Committee. The present leader in the Senate, however, is Senator Cranston from California, and he has introduced a bill almost identical to a bill I will be talking about.

The Office of Technology Assessment (OTA) is another branch of the Legislature that is coming along very rapidly. It will have its first board meeting on November 1, 1973. The director will be announced at that time.

In the House, energy is all over the place. It is in the House Interior, Commerce, and Ways and Means Committees, but certainly a good part of it is in the House Science and Astronautics Committee. The chairman of that committee is Representative Teague from Texas. The chairman of the subcommittee, the man I am essentially reporting to, is Representative Mike McCormack from the State of Washington. About two years ago he initiated an energy task force. I am sure many of you are aware of that. The result of the hearings was to set up a separate subcommittee. It moved out of the Science Research and Development Subcommittee and became a separate subcommittee. He has been holding hearings extensively, and the first two pieces of the legislation are a Solar Heating and Cooling Act and a Geothermal Act. Concerning OTA, many people think that one of the things that they will want to look at, since it is fairly noncontroversial, is solar energy, and you might want to try to have an impact on, or an input into the decisions of that group.

The present bill for the Heating and Cooling Act is HR-10952. This has the support of all but one member of the House Science and Astronautics Committee, twenty-seven out of twenty-eight, and has about another seventy-some cosigners. It is very unusual to get this many cosigners on a bill, and it is indicative, I think, of strong Congressional enthusiasm for the whole area of solar energy. This bill may, in fact, not be supported by Congress; there are a lot of political ramifications, and the Office of Management and Budget will get into helping make that decision. But the present bill would authorize 50 million dollars over five years to produce four thousand home units, half heating and half heating and cooling, somewhat phased with, certainly tying in with the present NSF studies. Now, you cannot be too optimistic about the passage of any bill, and yet I think on this particular one there is very good reason for some optimism.

One point that I want to make is about going metric. This same committee should take up a metric bill on October 29, 1973. Most people have not talked metric here today, so I would hope that the solar industry could begin as a metric industry.

The last point is on the Solar Incentive Act. No legislation has yet been written. I am sure that it will have to go to another committee, probably the Ways and Means. Congressman McCormack will not have direct influence over this Act, but I think it will be written in his office and I will be working on it.

Let me just mention some of the things that come along and try to get a little reaction to them. Congressman Vannick from Cleveland has suggested a solar energy data bank that will probably become a part of an Incentive Act. Certainly some suggestion that NBS establish standards will be forthcoming. It is suggested that somehow your income tax mortgage interest deduction be tied to energy conservation. That is a negative incentive and it may not meet with approval. Allowing tax credit for repairs, improvements, or installation of solar equipment, and tax credits for manufacturers, builders, and home owners, something like twenty percent or twenty-five percent probably will come out. Accelerated depreciation, maintenance, training costs, and low interest loans are possibilities. There is a study now under way within the Patent Office to come up with a solar patent publication, and that may be made part of law. There are no ideas on how you can provide incentives to the utilities, the banking industry, or the architects. I would appreciate suggestions in any of those areas. One of the problems is how to legislate life cycle cost and, generally speaking, I think people would agree that the solar energy industry would move faster if we knew how to do that. The key problem is three-dimensional zoning. This country does not have it. England does and we need to at least start studying it. Some of these ideas for national centers sound excellent; possibly there could be a public corporation.

W. R. Cherry, Chairman
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I want to make one comment on the discussion we had about solar cells and arrays. I have the idea that you will not be able to differentiate between solar cells and arrays. I think they are going to be integral and that you should not make something and then try to glue it down. I think it's got to all come in one operation as a kind of integral continuous operation, or I don't see how we are going to make the costs that we are talking about.

I also think we need to do something toward a national renewable energy utilization objective in this country; that is, we've got to utilize energy from renewable sources. Solar energy is, of course, one of these sources. Anything that we can do to promote this, and hopefully Congress can do something, would certainly help us. One very interesting observation is that the public can get involved in solar energy development R and D. I don't think there are too many people working on nuclear reactors in their basement or working on coal gasification or things of this nature. So, if it is done right, if we can get the good public relations that are necessary in promoting a new thing like this, we can get the public interested and to back us up. The best way in the world to get the Congress behind you and to get money into the field is to get the public to demand it. The thing is to do it right and to make sure that we are moving in the right direction.

I think all of us should be aware that we are in the honeymoon stage of solar energy. Arthur Cantrell of AVCO, at the Solar Energy Society meeting in Cleveland a couple of weeks ago, tried to give us a sobering realization of bringing in a new field like this, when the funding seems temporarily insignificant. But when this funding becomes a significant part of the total energy R and D of the United States, we are going to find our enemies coming out from nowhere. We must prepare ourselves for the time when the sailing will not be so smooth.

I think another point I want to make here is that definitely the government has got to do some pump priming. The semiconductor industry got started in the same way. But if you would look at the cost of semiconductors, you could see that there wasn't much of a reduction over the years during the fifties. But as soon as the large amounts of government expenditures dropped off, the prices started coming down; the competition went up; and those who could make it for the price stayed in the field. The same thing is going to happen with us.

Industrial Panel Discussion

C: I want to comment on the size of the problem, and also on Arnold Lesk's discussion of how many ribbon pullers are necessary, etc. Alvin Wienberg, Director of the Oak Ridge National Laboratory, delivered a talk before the National Atomic Energy Conference in Geneva in 1971 in which he outlined the situation with respect to nuclear energy over the next hundred years. In this, he talked about the thirty years from inception of the liquid metal fast-breeder reactor program, and that we would be producing one 5000-megawatt reactor per day. At $500 for a kilowatt, it amounts to roughly 2-1/2 billion dollars per day. And then in sixty years from now there would be two reactors per day – 5 billion dollars per day. That's the size of the problem that the nuclear people are talking about. This is responsible crystal ball gazing from the nuclear field.

C: I think I should comment a little bit on the R and D effort. The photovoltaic specialists say, "Well, I think we are pretty sure that it works." Yes, but there's a liquid metal breeder reactor – we know how to operate it by steam. With silicon or cadmium sulphide, we want to be more careful in order not to lose credibility, so we do it on a shoestring basis. And I want to warn against doing this. We will do it forever if we do it on a shoestring basis. We should recognize that it can be done if we make the decision to do it right; otherwise we will not succeed in bringing the price down and making it reliable.

PANEL II.
USER REQUIREMENTS FOR PHOTOVOLTAIC SYSTEMS

PANEL II. USER REQUIREMENTS FOR PHOTOVOLTAIC SYSTEMS

L. R. Lomer

J. Werth

T. Schneider

H. J. Pfeiffer

O. D. Gildersleeve

F. H. Morse

L. R. Lomer
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I am going to address only one of the potential Coast Guard uses for photovoltaic devices, and all of them that I am working with now deal with navigation.

Our primary objectives are to provide an improved power supply on our Nation's present navigation system (Fig. 1). All of our buoys and a lot of our minor lights located ashore and aboard pile structures are now powered with the zinc-air primary batteries. We are looking for something that has a six-year life. Pollution-free life cycle is very important to us. Zinc-air batteries have mercury in them, and of course we can't dispose of the things anymore. The costs are getting worse. If the system can be lighter weight than the batteries we have now, we can go to smaller buoys, and therefore, we can go to smaller buoy tenders, with all of the associated gains in the cost of the system. The last two objectives, low cost per ampere hour and decreased maintenance cost, of course, follow the others.

The candidate solar powered aids to navigation are identical to the aids to navigation powered by these zinc-air systems (Fig. 2). Roughly speaking, we have 8,000 minor lights and 4,000 lighted buoys. The present bulbs aboard these devices now are flashing signal lights – 12-volt dc from three to thirty kilowatt hours per year. It is easy to anticipate that these loads can go up by a factor of three, because whenever you have a platform out there, people want to hang other things on it. Remote recording weather stations, radio beacons, devices that tell us where the buoys are, whether they are on position, and a whole host of other devices, including sound signals, can end up aboard these platforms. Our last year's cost for these primary batteries was 1.8 million a year, and the servicing cost is 7 to 8 million dollars a year, which points out why we are interested in solar cells. Just as a rough guess, let us say that 75 percent of the battery powered needs for navigation could be converted to solar power (Fig. 3). This means 70 to 100 thousand watts or 12,000 watts a year for the system we are talking about now. There are other applications such as remote stations in the 5- to 15-kilowatt mean power range, which are slated to go into automatic operation. Storage is the thing that we are worried about there. The other component in the system is the storage batteries and there's a potential there for 1.3 million ampere hours or a quarter million ampere hours per year. The things I am most worried about right now are the lifetime of these solar array packages, and our lack of knowledge about the lifetime of the secondary batteries.

Figure 4 is a rundown on the evaluation program that is currently underway. We now own 72 complete solar cell power supplies that include the batteries and the needed conditioning elements. There are four major programs underway. A laboratory test program will test the majority of the systems at the Coast Guard Research and Development Center in Connecticut. All of these systems are roof mounted right next to the water, along with automatic data collection instrumentation to measure all we can about the operation of the systems. In a compatible program, we also have a few tests under way in conjunction with the manufacturers.

Field tests include establishing a buoy farm in Long Island Sound off the Research and Development Center. We shall have solar cells mounted on buoys. We have other field tests operated by manufacturers, and we are going ahead and putting some of these systems that we own aboard operational aids to navigation right now.

Independent battery tests are a separate program to bring out what we need to know about the batteries. We are trying to get a program started here in design concepts and system integrations, to actually design and analyze every system that we could possibly use to see what kind of system we will need in Coast Guard stock, how that would be handled, and what the economics are. Present test sites that we have now for buoys are in Ketchekan, Alaska, St. Petersburg, Florida, and in the Boston Harbor. There is a test site in Texas, which is now operating lights on an oil drilling platform.

PROVIDE AN IMPROVED POWER SUPPLY FOR AIDS TO NAVIGATION HAVING:

(1) SIX-YEAR LIFE

(2) POLLUTION-FREE LIFE CYCLE

(3) LIGHT WEIGHT

(4) LOW COST PER AMPERE-HOUR

(5) DECREASED MAINTENANCE COSTS

Fig. 1. Objectives in Solar Power for Aids to Navigation

(1) 8,000 MINOR LIGHTS

(2) 4,000 LIGHTED BUOYS

(3) LOAD: FLASHING SIGNAL LIGHTS
12 VOLT DC
3-30 KILOWATT-HOURS/YEAR

(4) BATTERY COSTS: $1,800,000/YEAR

(5) SERVICING COSTS: $7-8,000,000/YEAR

Fig. 2. Candidate Aids for Solar Power (Presently Powered by Zinc-Air Batteries)

IF 75% OF BATTERY POWERED A/N ARE CONVERTED TO SOLAR POWER:

(1) 72,000 WATTS OR 12,000 WATTS/YEAR FOR 6-YEAR LIFE

(2) BATTERIES:
1,300,000 AMPERE-HOURS OR
220,000 AMPERE-HOURS/YEAR FOR
6-YEAR LIFE

CRITICAL UNKNOWNS:

(1) LIFETIME OF ARRAY PACKAGING MATERIALS

(2) SECONDARY BATTERY CHARACTERISTICS

(3) USABLE INSOLATION

Fig. 3. Estimated Potential Solar Power Requirements

SOLAR PROGRAMS:

(1)	LAB TESTS	USCG R&D CENTER MANUFACTURERS
(2)	FIELD TESTS	USCG R&D CENTER MANUFACTURERS OPERATING USCG UNITS
(3)	INDEPENDENT BATTERY TESTS	
(4)	DESIGN CONCEPTS AND SYSTEM INTEGRATION	

T&E PARAMETERS:

(1) CELLS FROM VARIOUS MANUFACTURERS
(2) PACKAGING TECHNIQUES AND MATERIALS
(3) VOLTAGE REGULATORS
(4) BATTERIES
(5) MARINE ENVIRONMENT
(6) DESIGN CONCEPTS

Fig. 4. U.S. Coast Guard Evaluation Program (72 Complete Solar Power Supply Systems on Hand)

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I would like to talk about the economic prospects for an electrochemical method of storing bulk quantities of energy, and about the efficiency. I am not prepared to discuss how much of this storage will be needed. That is what some of the other speakers have been looking at with much more expertise than I can provide. Let me tell you about what could be done if it were needed.

The economic target for batteries (large battery systems) is approximately 80 dollars per kilowatt of installed capacity. Depending on how long you have to use the battery at rated power, you would add approximately $2.00 for every hour of energy delivered or stored at rated power. For example, if you wanted a battery that would store ten hours worth of energy at rated power, this would be 80 plus 2×10, or $100 per kilowatt. If you wanted a week of storage, say, that would be about 170 hours, so 2×170 equals $340 plus the basic $80 for the power generation part of the battery, or approximately $400 for a week.

The lifetime target for utility load leveling is 10 to 20 years, which means, in that case, 3000 to 6000 deep cycles over the lifetime of the battery, with an efficiency target of about 80 to 90 percent. In order to achieve the long cycle life in years, we feel it is necessary to avoid the so called solid-solid transformations. For example, lead dioxide and lead sulphate, both of which are solids, go back and forth between these two states. This kind of change in crystalline structure would eventually result in failure of the material, because every time you go from one phase to the other, there will be some degradation. There is really no way of getting anywhere near 3000 to 6000 cycles with solid-solid transformations.

We have achieved efficiencies on small cells in excess of 90 percent, so that the overall turn-around efficiency (energy out over energy in) on large batteries can very reasonably be predicted to be at least 80 percent, possibly even as high as 90 percent if the rate at which a battery is charged and discharged is a long rate. By that I mean 15 hours or longer. The shorter the rate, the lower the efficiency, unless the battery is oversized.

I'd like to just say a word about an alternate electrochemical method of storage which has been discussed quite a bit recently – that of using hydrogen as the means of storing energy. Charging would involve decomposing water in an electrolyzer, and the discharge would be using the hydrogen which has been stored in the intervening time in a fuel cell. One of the problems of using hydrogen in your kind of application is the efficiency. The most optimistic efficiency that one can predict – turn-around efficiency for a hydrogen storage scheme – would be about 50 percent. A more probable turn-around efficiency would be between 35 and 40 percent. This means that, in your case, if you wanted to use hydrogen as a means of storage, for every watt that you have of photovoltaic generating capacity, you would only really get a half a watt, if it has to go through a storage of actual usable energy. Since it appears to be difficult enough to get a reasonable cost per watt, paying twice as much for a watt for the sake of getting continuous generation would present a problem.

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Today the utility industry is faced with the critical problem of supplying their customers' needs for adequate amounts of reliable, economic, and environmentally acceptable energy with due regard to the conservation of natural resources to insure that energy will be available tomorrow. To meet this responsibility, the electric and gas utilities are mounting significant research and development efforts, and examining all alternatives. Kilowatt hours and therms do not face discrimination on the basis of their source of origin. If an economic and environmentally acceptable method of utilizing solar energy can be incorporated into the utility system, it will be used. Basic questions must be answered prior to a large-scale utilization of terrestrial solar energy. Questions of direct interest to the utilities are many, and there are three that I would like to identify right now. They are very general questions, and the first is: How can photovoltaic systems be incorporated into utility operations? The second is: How will photovoltaic systems interface with utility operations if they cannot be directly incorporated into utility systems? And the third is: Will solar energy utilization adversely affect the profitability of economic utilities? These questions should be addressed and answered early in the development of terrestrial applications of photovoltaics. However, the most important single user requirement, from the point of view of the utilities, is low cost. Energy from solar cells must compete with very cost-effective alternatives and must prove to be their equal. Significant reductions in manufacturing costs must be made, and I am sure you are all aware of this, while maintaining adequate efficiency and life. This requires an adequately funded and properly administered research and development effort aime ' ~t developing low-cost solar cells for terrestrial application. Large systems studies are not appropriate at this time when the future cost of solar cells is so poorly defined. I am sure there will be lots of questions on that statement, but the point is that it is very difficult to make planning studies for installing solar cells in an operating system if you do not know what they cost. The projections that you are using are fuzzy, both in terms of the time period when a particular device would be available, as well as the cost and performance criteria. So, all of this means having an uncertainty principle in planning. User requirements that are of secondary importance include low cost, efficient energy storage, and power conditioning equipment. If the solar cells are cheap enough that they can be used directly for peaking, then you will not have a storage requirement. So storage is secondary, because even without storage, if solar cells are cheap enough they can be used. But the energy storage and power conditioning equipment is of significant value even without solar cells. It will be developed because of existing needs. Low-cost and efficient energy storage devices that can be used as alternatives to pumped hydro are under development today. You will hear some more about this later, and they should be available for commercial use in the 1980's. Relatively inexpensive power conditioning is available as communicated earlier today, and further price reductions are expected in this area. The crucial requirement remains low-cost solar cells with adequate life and efficiency. When this goal is achieved and these cells are developed, they will be used and will be incorporated in utility systems. There are also very specific areas that we can go into and talk about, such as how these things might be incorporated in utilities. But what I want to stress is that if you are talking about a device that is $20,000 a kilowatt or $5,000 a kilowatt—it just can't compete, even with all of the environmental control equipment you have to put on conventional units. At $5,000 a kilowatt, you are not going to get any missions that you are going to really worry about to compete with coal. You are not going to install solar cells to compete with coal on that basis. So you first have to define clearly, or as clearly as possible within a given time period, what your projected costs are before you go into large-scale systems studies as to how you might incorporate them into any given operating system.

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Introduction

The present restricted view of solar cell applications has occurred because

(1) Utilities have not considered intermittent solar energy a firm generating source.

(2) Solar cell specialists have been unfamiliar with utility concepts and their economics.

(3) Many attracted by solar energy have not viewed utilities as a means of implementation.

Discussion

To use solar generation, utilities would have to expand their definition of forced outages to cover energy source outages as well as equipment malfunctions. Solar outages would occur due to cloudiness or sunset.

To determine how intermittent solar power would affect utility service, the load and generation models utilities use in reliability studies could by analyzed by time frames. Hence there could be an annual load duration model for a 10 a.m. to 3 p.m. time frame and so forth. The corresponding generation model would include solar generation in day time frames but not night time frames. The allowable solar component of utility generating mix could be determined by relating the utility system reliability during the various time frames to the utility's reliability criterion. The criterion of many utilities is that generation should exceed customer loads except on one day in ten years.

Incorporating a solar incidence and availability model into utility production costing programs that use hourly loads would permit evaluation of a break even price for a utility solar generator.

In comparing residential and utility applications of solar voltaics, the utility market is clearly better for these reasons:

(1) A single residence has a poor load factor while a utility serves a diversified load that will facilitate amortization of solar generating equipment.

(2) Large residential electric demands seldom coincide with solar availability and thus require storage through which considerable losses may occur. On the other hand, electric utilities could use solar generation whenever the sun shines.

(3) Due to the availability of alternate generation, a utility may need no solar energy storage to cover adverse weather conditions. A residence needs a redundant system plus storage.

(4) Utilities may be installing electric storage battery plants for load leveling beginning in the early 1980's. This development is timely for solar voltaics because battery plants may have converter capacity during the day which, at no additional cost, may be used for DC to AC conversion of solar cell output. By comparison, residential solar power systems will have to bear the cost of inverters to supply certain appliances, and the specific cost of small inverters is likely to be high.

(5) A centralized utility solar plant may be installed and maintained more inexpensively than individual units on residential roofs. Centralizing solar cell plants does not invalidate the concept of residential solar/thermal systems for heating.

This comparison of residential and utility solar systems suggests a utility application that may be the first major terrestrial market for solar voltaics. Figure 1 shows a utility load leveling battery substation associated with which are solar panels deployed on the roofs of adjacent buildings. This concept could be called utility distributed solar generation.

It should be noted that solar/thermal electric plants may be more economically attractive, more significant and near term for utilities than solar cell plants. It all depends on their relative annual cost per kilowatt.

Conclusions

Terrestrial solar cells have been identified largely with electric systems for individual houses. But relative to these residential energy systems, solar cell power generation for electric utilities appears to justify a higher installed cost per kilowatt, to constitute a larger and faster growing market and to provide better utilization of electricity from the sun. Therefore, in terrestrial photovoltaics development of significance to national needs, the larger support should go to utility solar power applications.

Recommendation

When processes for the production of low-cost solar cells are available, utility applications should dominate development.

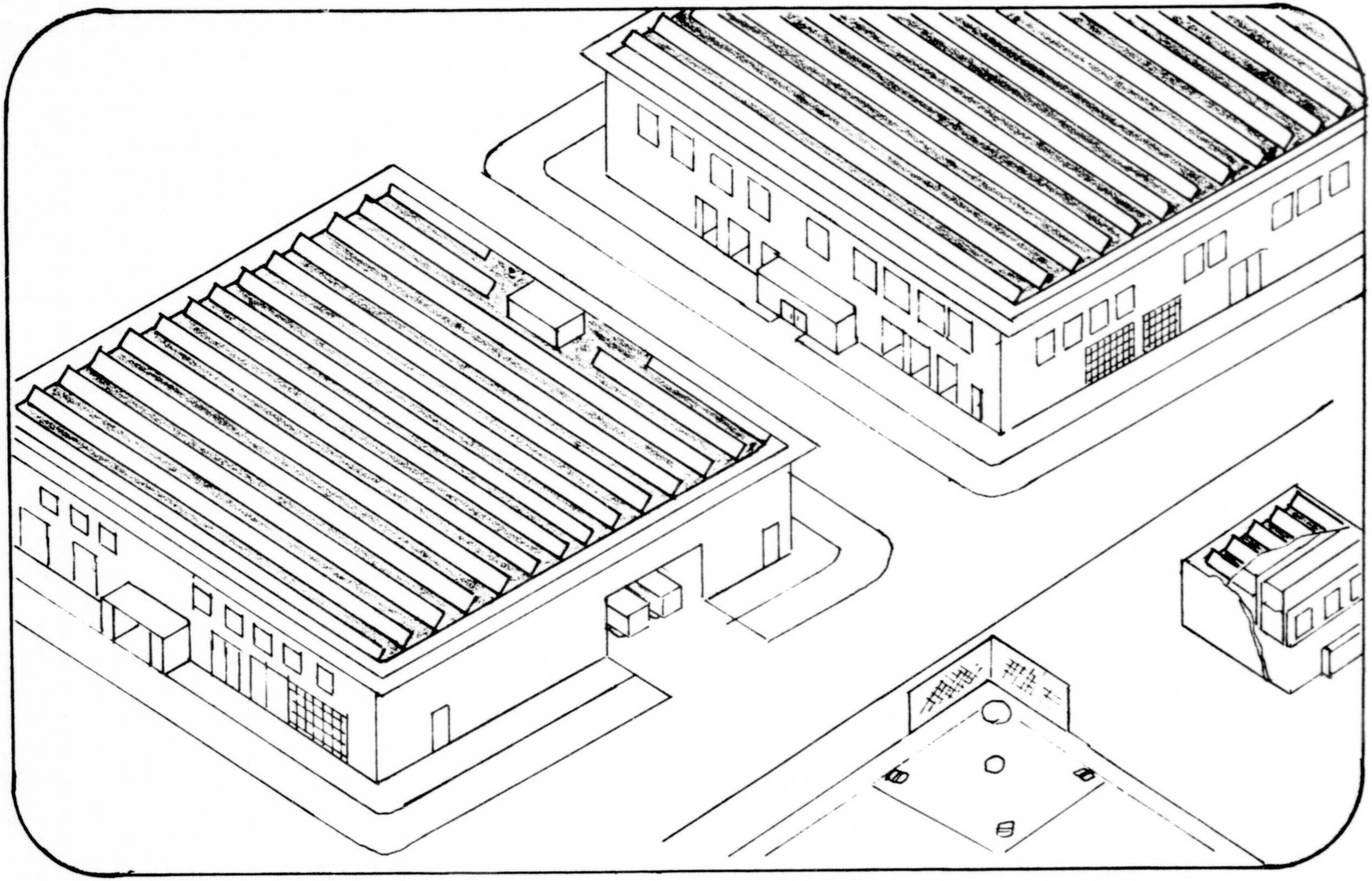

Fig. 1. A Utility Substation with Load Leveling Battery and Solar Cell Panels Deployed on Industrial Buildings

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With three of us on the panel from utilities, we got together to try to not repeat comments. I will go into the question of how does one work with the utility, because it is quite different and I'm going to go at it from two viewpoints. For over twenty years I managed a good share of the General Electric Company's projects with the utilities. I worked out of the General Electric Research Lab, and I have been with the utility now for over a year. But from that point of view, the utility is a little bit different to work with than most other firms you would work with. A friend of mine once said that a college professor's wristwatch runs in semesters. Well, the utilities' wristwatch runs in decades, generally, and right now we're planning plants we are going to be installing in 1985. So from a utility point of view, you don't have to exaggerate or worry about pushing the time scale too hard. Utilities appreciate the fact it takes time to do things. And in recent years events have come so that the utilities have become very research minded. So I think as you approach a utility with this project, always keep in mind you are approaching the utility at this point with a research project, not with an operational reality. If you were to present what we have heard the last few days as an operational reality, it would not produce a very good reaction. On the other hand, there has been enough progress; this is an interesting enough approach from a research point of view with a reasonable size installation. I think you would find the utilities, particularly in the areas of high insolation right at the moment, very receptive to engaging in a joint research project.

The other thing about the utilities is that in most of their rate structure now there is an allowance for research. The allowance for research may amount to about a half of one percent, typically, and so for their part of it, such as construction, things like that, you wouldn't have any trouble at all having them take over responsibility in that area. In other words, a joint project would be very much in the realm of possibility if it's reasonable and if it's approached as research, because the moment you get into operations, you get into the other side of the utility business, which is not only the cost but the extreme reliability with which they work. We still remember two blackouts: one on peak a.m., and one on the northeast system that occurred ten years ago or more. The reason we remember them is because they are so rare. By and large the utility customer has outages of well under figures like two hundredths of a percent, just to use a number. Utilities are very jealous of reliability, so on a research project it is fine. Operationally, every utility wants to be first with something that has been tested for thirty years. That is the general attitude you run into when you get operational, but as far as research goes, I think we are receptive. I think that monies would be available for a reasonable project; I think they would be glad to work with you. Utilities know a lot more about systems stability, problems of synchronization, and so forth, than anyone in this room, including ourselves, because we are not system engineers. So if you are going to work with the utilities, you've got to get in with the people who know about those things. And you are not going to reinvent them because there is a very long history behind them. So, utilities should be brought in early.

The other thing to keep in mind is the nature of the crisis we are facing. There are really two things: there's a crisis which is a short-term problem that peaks. We are in an energy crisis of that type right now. It has nothing to do with the world resources of energy. This is a relatively short-term thing which has to do with transportation and political factors, lack of refining capacity, and a few other things. Then, we've got a longer-term energy problem which does have to do with the resources, and utilities are very aware of this. If you plot world discovery of oil, you can estimate that by 1985, according to the curve, we will be discovering less oil than we are using on a worldwide basis. That I consider a problem. It is quite separate. The solar energy is not going to affect the present energy crisis in any way. But for the 1985 problem, which will start to really bite, solar energy could make a contribution. There is not going to be a single solution to that contribution from a utility point of view. In other words, I think we will liquify some coal, the nuclear plants will be used where they are the best, and solar energy will be another factor. None of these are going to be the sole factor. I think you can count on the utilities for assuming every one that looks reasonable in this area. Solar energy is a reasonable approach and there are several others. So I think you should put it in that context in your thinking.

Communications between utilities, or among utilities, are very good,and if you have a project among utilities, you can get a tremendous amount of exposure. It's not quite the same as when you are working with an industrial company that's worrying about proprietary rights. By and large a utility will not be worrying about proprietary rights, but if they put in an effort, I think a licensing right or something like that might be in the cards. But they are not in the normal antitrust situation that you're in if you work with electrical manufacturers, for example, or as other companies are, because they do have a fear of monopoly, and, therefore, they are very closely controlled. Antitrust, generally, is not a problem. This makes projects with utilities different from projects with other private companies. There are some important differences there.

Since Dr. Balzhiser isn't here, Howard Feibus and I are going to make a few comments about EPRI. The utility industry for a long time was divided into two groups: the Edison Electric Institute, which is an association of the investor-owned utilities, and the public power group having an equivalent organization. With the crises in energy coming along, and the great criticism that insufficient research was being done on electric power, for the first time, really, the privately owned and the public utilities got together to form the Electric Power Research Institute (EPRI). It now includes about 85 percent of all of the power generating groups in the United States, both public and private. They came to an agreement that each utility would be assessed on the basis of its generation about one fourth of one percent of the generating revenue—and it's a sliding scale—to support research in electrical industry. Eighty percent of this money would go to the central organization of the Electric Power Research Institute, and the utility would retain 20 percent for local and regional problems—research problems.

The 80 percent in dollars amounts to about 65 million dollars this year and will build up in a few years to about 200 million dollars a year. So after it was founded last December, Dr. Chauncy Starr, at the time with UCLA, was appointed Director. Dr. Richard Balzhiser, who was in the Office of Science and Technology, advising the President—he was Assistant Director I think—directs the Fossil Fuel and Advanced Technology Division. This division has two task forces: one is fossil fuel and the other is advanced technology. In each task force there are a series of subcommittees that will be involved in steering and guiding the actual projects. There are three other divisions: a Transmission and Distribution Division, a Nuclear Division, and a Systems Division. These too have substructures about which I will not go into detail. EPRI has set up offices in Palo Alto near the Stanford campus. They plan to hire about 300 people in-house. Of these, 100 will be on a rotating basis from universities, utilities, government, and the electrical industry, on a three-month to one-year basis; 100 will be permanent senior staff; and the other 100 will be supporting staff. That's a general picture and this operation will do systems planning and will look for research projects that fill in the gaps in our knowledge.

Up to now, it has largely been almost a grant-type agency, where people have come in with good ideas and they have been financed. Now Dr. Starr wants to decide what projects are needed, and then go looking for the capability to carry out those projects. Perhaps Howard Feibus of Con Ed can add to that?

Feibus: I don't really have anything to add, but I just want to emphasize one point, and that is EPRI is intended to be a mission-oriented organization, and in a classical sense of the word, research will be a small part of its emphasis. Really it's a developmental effort, and, as an example of that, a major program that has been underway in EPRI for the past several years is a battery development program for electrochemical energy storage programs for bulk energy storage. It's not really research aimed at any basic purpose, but we are doing this directly for utility application. Many areas such as additional storage, generation, coal gasification, transmission, and distribution and other major problems areas will be funded.

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Q: What does it take to get the utility companies involved in making progress and not standing on the side and measuring progress?

A: At this stage I think it is appropriate for the Federal Government to take the lead in this area, because you do not have to use very detailed cost estimates or worry about the cost of money, inflation, or any consideration like that. But given our present economy, you know what the cost of electricity is and you can associate that cost of electricity with the cost of generation, which is the major component, and you take an order of magnitude estimate of the cost of nuclear energy, which is about 500 dollars per kilowatt. So that tells you without any detail that you need very cheap silicon for very cheap solar cells before you can make a power plant. Now cheap is relative to the present cost. I have spent a couple of days here and I have heard a lot of interesting presentations on the goals for research, primarily with respect to cost, which I think is a very key crucial element at this point. The 1985 target of 50 cents per watt translates roughly to 500 dollars per kilowatt just for the cell, and that is not the cost of the plant. I would say that active utility involvement to the point where you would consider putting in a pilot plant and where you would get some very useful benefits from in-depth utility involvement may result in some financial and direct participation, more so than the utilities standing on the sidelines waiting to see what the situation is.

C: Yes. I think the point is that in the past, say three or four years ago, there was no reason to believe that photovoltaics would ever make it on an economical basis. Now you are starting to get to the point where you can say, 'we may be able to do it.' The point when the utilities will get actively involved is when you can say, 'we can do it,' and support that with a lot more evidence than has been presented today.

C: I would just like to make a philosophical comment on the economics of this whole thing. I think you can make some kind of an axis of economic thought by putting panic at one end and pleasure at the other. Now I work for a company, RCA, which has been at both of these extremes. The mainstream of our business is entertainment, and that is a discretionary thing, and its sale is governed entirely by what things cost, what people want to buy, and what the competition is doing. And that is the way we like to see things go and that's the way the utility people tend to look at the power business presently. If you go back a couple of decades, just to take another specific, RCA was prime contractor on the ballistic missile Early Warning System, which at the time was a very large project of a billion dollars. That did not have a profit and loss statement. It was considered a national necessity. There was competition among people who bid on the project, but there was not a question of whether we could build; it was just where could we get it done most economically. Now I think that some people in this discussion may put the energy business at the discretionary end of this thing. The utilities tend to look at it that way. But at least in the long term it seems to me that this problem has gone down the axis a little ways, maybe not half way, but it is toward the end that is typified by something you have to do, regardless of what it costs. Now that is the long-term problem where you run into a real energy shortage, and people vary in their estimates of when that is – like is it twenty years or 150 years – but it is coming sometime. And I would just like to inject the thought that you cannot stay at the discretion end of this axis and you probably do not want to let it slide to the panic end, but it is important to decide where it is.

Q: I wonder if the utilities would mind indicating what they think they will have to pay per installed kilowatt for power-generating stations like now, 1985, and 2000, using 1973 dollars. What do you envision as your costs as the years go by in the current century?

A: One of the things that one has to look at with regard to solar plants is, does it replace other installed capacity? Or does it just replace utility energy? The two are quite different. If the solar cell is used in such a way that I cannot possibly store the energy from it for ten days, so that I still will have to buy as many gas turbines to keep on hand for peaking when the sun is not shining, then the only thing the solar cell is doing is saving the oil which is burning in the gas turbines. That is the very rock bottom of the economics. If you can actually replace installed capacity, then the economics start to improve, depending on the nature of the installed capacity you put in. Now if you take nuclear -- say nuclear plants go 500 dollars a kilowatt, but the fuel cost is down to a couple of mils, then if you have any economic sense at all, that plant is on the line as much as you can possibly have it on the line, because your fuel is not costing you much. But this means you have to have other means of following a variable load. One of these means is to let the nuclear plant generate 24 hours per day and pump water or store at night. Another means of doing it is putting in plants of lower capital cost, even though they have higher fuel costs. One of these possibilities is to put in flexible fossil fuel plants. Now, say a fossil fuel plant would run 300 dollars a kilowatt, but the fuel for the fossil fuel plant is rising fairly rapidly, so that the fuel costs might start getting out to 0.6, 0.7, 0.8 or a cent per kilowatt. That is the plant I would prefer to shut down for purely economic reasons. Now if I go above that and just have occasional need for extra generating capacity, I would put in gas turbines that may run 140 dollars a kilowatt. But the fuel costs on the gas turbine may be up as high as two cents per kilowatt. So you see there is this hierarchy of plants. The solar plant has zero fuel costs, so it is not like a gas turbine, but it has 40 to 30 percent availability, so it is not like a nuclear plant. And this is the thing that we put in our system studies -- one of the things that needs more attention. The prices that I have quoted might apply in 1985.

C: I would like to make two comments. One is to congratulate the people from the power utilities who are active in looking at this, and I think that Dr. Gildersleeve has made some very constructive remarks to create something which is new and valuable for us to look into. I think that it is important for us to interact now and not to wait five or ten years from now, or fifty years from now, when we hopefully do have 50 cents a watt available, and then say, 'here it is; it is all there; now see what we can do.' But before we do it, let's see how the markets can develop and see what kind of things we have to look into, because it is not just the solar cells, but it is storage as well. There are endless possibilities. What are the different shapes of the markets? And so a close interaction is needed to see what kind of plant is developed and what kind of lifetimes are really needed. What kind of deployment do you really need to develop this together? I think this discussion has to be stimulated at as early a time as possible. And here very small inputs, for instance proof-of-concept ideas, may be of importance. You have switching stations; there are batteries sitting out there some place; and charging these batteries by means of solar energy is just one of many possibilities which one can think of to develop a little feeling of what solar cells can do. Which way would we like to go to create interim markets, and so on, needs to be discussed also.

A: I think there is a great fundamental difference between interacting with utilities, which the utilities are very anxious to do, and examining all of the options. We want to look ahead into the future and see what is around the corner. We know what is going to be there in 1990, because either you have a system that is going to compete with electric utilities, or you are going to have a system that is incorporated in them. Either way, utility wants to know. But there is a difference between interaction and large-scale involvement like funding for research projects. It is a fundamental difference.

C: I think you made a very good point in saying that these will either compete or not. We are very much interested in bringing solar energy out of the realm of the tinkerer who puts this thing up on his own house roof and wonders what to do when it breaks down. We need someone who will take the responsibility to service this thing and to help develop it. Of course, an energy company is one very good possibility to develop some market in the right direction. This is one thing that I think should be dwelt upon.

C: I think the comments the two gentlemen made a little earlier are typical of the kind of thing I have heard many times from the power companies. I think it is insulting to tell me about how you define the advent of the nuclear generator and by properly doing the R and D, get down to a cost of 500 dollars a kilowatt installed, which I do not believe. If you look at the records, you will really not find very many of them that got to this. I believe there are problems with the solar cell system and there are going to be a lot more of these problems showing up. If I were to tell you right now, with better than 90 percent certainty, that we can make the solar cell systems for less than 50 cents a watt in 1985, with somebody there who would order some quantity of them some reasonable time in advance of that, I would like to know what the power companies would be willing to do about it? I don't work at a university, I work for a company that has to make money, and would supply equipment like this, and I say that the certainty is very high. Yet I do not want to wait until 1985 to come and see you, because if we are not producing them for somebody, they certainly will not be produced for 50 cents a watt for a system. So there is the problem.

A: The impression I got today is that there was no certainty that you could achieve 50 cents a watt. I use the figure 50 cents a watt because for cell cost, that translates to roughly 500 dollars per kilowatt, for which, by the way, you can get nuclear generation. I do not see any systematic program that is realistic enough to lead one to believe that you could produce a 50 cent per watt cell by 1985, which still would not mean that you could build an economical solar energy plant; but at that point you could have a systematic program leading to a cheaper cell. At that point a power plant study would certainly be appropriate. But the probability of success at this stage is something that is debatable, and it is probably premature to spend money in other areas rather than to improve cell costs.

C: There is a place where a national program in this area has to do something, because on the one hand you are saying I do not know how surely you can get there, and on the other hand you are saying pretty surely we can get there.

C: He is not going to be convinced by government spending, though.

A: Oh, by all means. I think that is the only means by which we are going to be able to demonstrate it, and you have to be able to demonstrate the fact that you can build these cells at low cost.

A: Let me make another point on that. I think we decided yesterday that we wanted to get a 100-kilowatt plant, which certainly is not enough of what you need to do to build up a business. But a 100-kilowatt plant can be done within present boundaries, and on the basis of such a 100-kilowatt plant, with solar cells at the same time continuing to improve as projected, it would be much easier than to plan a project similar to, for example, the fuel cell project that the utilities are engaged in, which would produce a large enough volume. But our level of ignorance is quite high, and the projected cell development is not quite here yet, so I think this 100-kilowatt project is a very important one. It is not a size that is really interesting to a utility as a substation device, but it should start giving us some idea on the life of arrays, on a few of the problems connected with arrays. And if cell development comes along, then it can be incorporated and it can be put into forward planning. Right now, there is not enough information to put it into the hard forward planning for a real substation.

C: I think the point with the fuel cells is an important one. The utilities have gotten into the fuel cell business right now in a very large way. This information will be coming out in the near future as well as some publicity releases. But here you have a manufacturer and a group of utilities both of whom are willing to gamble hard money – not just speculation on paper. And when it gets to the point where the technologists are willing to back up their predictions with hard money and you can present the data to convince utilities that your technologists are correct, then you will see joint development programs.

Q: I think it was Dr. Pfeiffer who said that a long-term fossil fuel shortage may start around 1985?

A: I was talking particularly of petrochemicals.

Q: I guess the question is, is there any reason for us, the utilities, Senator Gravel, or anybody else to be worried about a nuclear reactor-based power economy, either from the standpoint of security or from the standpoint of technology or of public acceptance? Is that a real problem from a utility standpoint?

A: It would be very difficult for a utility to operate with its total generation in nuclear, at least with present generation nuclear plants, simply because of the way they operate. I mentioned the economics. There are certain reasons why you do not want to ratchet nuclear plants up and down in power level more heavily than you have to. Therefore, for load following, you need some other kind of power and right now the only thing we have is various fossil fuel plants. So I think we are going to see fossil fuel plants designed and put in specifically for load following. The other alternative or approach is energy storage. We are going to have to have energy storage systems increase to take advantage of the flat power output of the nuclear plants, and we are going to see new fossil plants go in also for load following — not peaking — but load following. So the total generation can not be in nuclear. They just do not follow a load well enough. So we are sort of stuck. We are still planning fossil plants. I think that is the reason we are worried. The other thing is, of course, aside from the power industry, that in the transportation industry we have no replacement for petrochemicals at this point. But we will have more fossil fuel plants being built for the rest of this century.

Q: Coal?

A: Right now I would have to say coal, yes.

Q: And that is not a problem right now, I guess?

A: There is not much coal available in the first place. There are not very many coal miners. The mines have been closed down and it takes about three years to develop a mine. So even though the coal industry is a depressed industry, if the orders increased ten percent, it could hardly meet them. There are about 36 million tons a year we export that we could divert for internal use by refusing to honor export commitments. But there isn't any surplus of coal that is mineable right now.

Q: Then perhaps the coal industry growth projected on page 6 of the Solar Energy Panel Report showing a six-fold increase in what coal would have to supply to produce fifty percent of the electric power generation will be as formidable as what the semiconductor material growth might have to be?

A: Well, it is typical of people to overestimate what can be accomplished in the short range, and to underestimate what's going to be changed in the long range. And it takes time to develop coal mines and to recruit miners. So in twenty years, I feel we can get the coal. We can't get the coal in one or two years, or even three years because you've got to build up the industry of developing the mines before you can get the coal.

Q: I'd like to ask Commander Lomer if any consideration has been given to putting these solar farms or solar conversion stations on the sea? Nuclear stations have been proposed for shallow coastal areas. And one point I would like to ask about in conjunction with that is that of systems coordination. In the initial stages of planning, might it not be beneficial to consider the possibility of integrating the so-called ocean thermal design for energy conversion with some type of radiation capture that is solar thermal or photovoltaic? The land cost for capitalization is one consideration and this combined approach to a coastal plant site might be worth while. One other point I'd like to throw out is that I would like to encourage consideration to long-term projections as we assess insolation. There are a lot of us that would recognize the constraints of our change in power utilization now and the consequent change in pollution, and that this might affect the incident radiation available. For instance, we are planning to have very drastic changes in Montana and the Dakotas during the next decade in terms of coal utilization power production. There is no doubt this is going to change the pattern of insolation in the Midwest. That should be considered too.

A: I'm willing to take a shot at the off-shore, but I don't know about the pollution aspects. You have to have concrete projects that power engineers can be convinced are viable. When you look at the cost of a supporting structure — just the cost of the plywood and shingles in your house — you are getting pretty close to the cost of the solar cells. You are talking fifty cents to a dollar a square foot for your roof; and if you try to make a floating power plant for less than a dollar per square foot, it is going to have to take into account all of the problems you have with ships running into the thing, waves overlapping the surface, and the bouncing nature of the surface of the ocean. Nobody really, that I know of, has ever made a calculation that would indicate there was some chance of doing it. But the off-shore nuclear station is a very compact device. And even at that, the protective equipment involved and the breakwater around it is in the hundreds of millions of dollars. This is just concrete block.

Q: I was going to invite the utilities to play a different role in the development of solar energy. I am thinking of the other two modes of energy production that are available, one of which is nuclear energy, which became available to the utilities through the war and the subsequent government supported research programs. And there was the one before that where the University professor in England, Michael Faraday, discovered that in rotating a conductor in a magnetic field, you've got a current flowing through it. In those days there were no kinds of utilities to support any research, and it wasn't useful in wars. So it took fifty years, or something like that, of slow development before Thomas Edison pulled switches in New York City and began electrification of that area. In the case of solar energy, there is the possibility of utilities entering into the stages of the research and development process earlier than in anything we have had described here so far. Is there some possibility of moving them to accept this invitation to join in this large project?

A: You are asking two questions. One question is, "Will we interact with you?" And I think the answer is "yes." If the utility that supplies your power is large enough to have such an R and D effort, the residential engineering department, I am sure, will be willing to talk to you. If you are talking about large commitments in man hours, you are talking about large costs. Utilities all over the country are in a very tight financial situation. And if you look at what happens with requests for rate increases, they are frequently turned down or they are delayed. Costs are escalating faster than rate increases are coming through. When you are talking about large-scale expenditures of money, it isn't there right now.

C: I think that assuming a situation where solar energy were allowed to develop as rapidly as the technology could, without a limit of money, we still would need the involvement more than the talking of the user, and I think that perhaps the users feel we are asking for money from you. We are not. We are asking for the teamwork so that when we get there, it is usable, and I think that that's the point that we wanted to make.

A: I think that all these issues are really ones that can be adjusted. I think that if a company is really interested in selling solar cells at a price that is competitive with gas-fired or nuclear plants, that's fine. And that's one thing that some people here represent that they are in a position to do. I'm not certain whether or not this is true. The other point is whether the utilities want to get involved in the type of R and D effort that NSF is sponsoring, and I think the answer is "yes". I think the most appropriate mechanism for that on a national level will probably be the Electrical Power Research Institute.

Q: Except that we heard that EPRI is mainly development; that it isn't research.

A: No. There are several different things. What Dr. Feibus really was talking about, I believe, was about large-scale R and D effort -- the program planning in R and D development effort at EPRI is at a somewhat different level, and it, in a sense, would act as an information bank in this area for you, and provide a central interaction point in terms of how utilities would interface with something like this.

A: At this stage of the state of the art where you are technology limited, the real problem is to build a device that is less in cost by several orders of magnitude. I don't see what particular advantage you get from utility money at this point, because the problems aren't yet addressed to the particular application.

A: It is quite true that the nuclear industry grew as a result of the war and as a result of government funding. But I think that in fairness to the utilities, the suppliers did not approach the utilities to talk about nuclear plants in those early days, certainly not after exploding the first nuclear bomb, and certainly not after the running of the first nuclear submarine. When we had enough nuclear work done -- most of it under government auspices -- and when a credible economic and technical stage had been achieved, then the manufacturers went to the utilities I think what we are saying is that it is a worthwhile goal for the Federal Government to support, and when the project has developed to the point that manufacturers such as we are, and utilities such as the people on the panel represent, can begin work, it will be a success.

Q: The manufacturers are different in one respect in that they are very dependent upon a mass production type of growing or learning curve, and we cannot build a prototype at fifty cents a watt or whatever the desired goal is. We never can get there unless you agree that the projection we are making is a realistic one, or something that you will have confidence in by the time you get up to the production rates that will make power plants. But nobody can give you that price until they have made it, so it has to be a projection that you believe in. Can the government support do that? By building a hundred kilowatt plant, we will not get the cost down to what a thousand megawatt plant will do. So you never can demonstrate the cost that will meet the cap rate, and I'm not quite sure how we will ever get over that particular situation. We will never get near the costs where you can use it. That's the difference, I think, between a nuclear plant where you can build a smaller scale and cost out the items and say it's going to cost so many dollars per kilowatt. You can't do that here.

A: What are the meanings of the cost projections that I saw here this morning? Aren't they valid?

C: They are all projections. You are relying on some other interests to generate the volume. They are somewhat like earlier nuclear projections, though.

A: I guess what you need is to build a basic development program like that which is funded at this time, primarily by NASA. You still have to demonstrate its value and you are still talking several orders of magnitude reduction in cost. That's a lot to predict.

C: I would like to make one possible suggestion. Maybe what we need is a Solar Energy Commision to do the same job for solar energy that was done in the AEC for nuclear. Now the only trouble with that is that the initials would be those of a commission that already exists – the Securities and Exchange Commission.

C: It has been rumored that those initials are NSF, the National Solar Foundation!

Q: I have a question that has to do with the crossover point that was mentioned: if one has to build plants at a remote site, say in Arizona, and ship the power to New York City, you indicated that hydrogen production perhaps was not a way to go for storage. Where is the crossover point in terms of transport of electricity, for which piping of gas becomes feasible?

A: I think I wrote a paper for Scientific American on this in 1963, but my figures might not be exactly right. At that time the crossover between transmission of electric power overhead and by moving coal by rail was around 200 to 300 miles.

Q: And you think it hasn't changed?

A: Since the moving of coal by pipeline and moving coal by unit train are in the same order of economics, I think it hasn't changed that much. Now, of course, when you have solar energy, you take an additional loss when you make the hydrogen. So the crossover point would probably be in the favor of longer transmission. There are various electrical problems as you go to longer transmission which I don't think I will go into.

NSF/RANN PLAN FOR NATIONAL PHOTOVOLTAIC CONVERSION PROGRAM

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NSF/RANN PLAN FOR NATIONAL PHOTOVOLTAIC CONVERSION PROGRAM

H. R. Blieden

A NATIONAL PLAN FOR PHOTOVOLTAIC CONVERSION OF SOLAR ENERGY

H. R. Blieden
National Science Foundation
Washington, D.C.

Introduction

I wish to take this opportunity to thank all of you for your contribution to the success of this workshop on photovoltaic conversion of solar energy. It has been very interesting to listen to the progress being made in research on photovoltaic devices and systems. The working group chairmen have provided us with good summaries of the prospects and major problems in their respective areas. The panel discussions have given us a better insight into the nature of the barriers remaining to effective implementation of photovoltaic conversion of solar radiation when low-cost photovoltaic arrays become available.

Where do we go from here? I wish to outline the current NSF/RANN Solar Energy Program in Photovoltaic Conversion and to describe a plan for a major national effort to develop photovoltaic conversion of solar energy. I shall keep my remarks brief so that there will be sufficient time for discussion of this plan.

NSF/RANN Solar Energy Program

The National Science Foundation (NSF) established a research and development program in terrestrial applications of solar energy in FY 1971 in the Research Applied to National Needs (RANN) program of the Research Applications Directorate (Fig. 1). The major responsibility for the solar energy activities in the RANN program resides in the Division of Advanced Energy Research and Technology and the Office of Public Technology Projects (Fig. 2).

As we indicated earlier, the general objectives of the NSF/RANN solar energy program are:

(1) To provide the research and technology base required for the economic terrestrial application of solar energy; and, to foster the implementation of practical systems to the state required for commercial utilization.

(2) To develop at the earliest feasible time the potential of solar energy applications as large-scale alternative energy sources.

(3) To provide a firm technical, environmental, social, and economic basis for evaluating the role of solar energy utilization in U.S. energy planning.

These objectives are based upon the recommendations of the Solar Energy Panel, organized and funded by NSF and NASA in January, 1972 under the auspices of the Energy R&D Goals Committee of the Federal Council for Science and Technology. This Panel's purpose was to assess solar energy technologies and to propose a research and development plan. In addition to NSF and NASA staff participation, about 35 solar energy experts from universities, industries, and other government agencies became working members of the Solar Energy Panel. The Panel's report*, issued in January, 1973, became the basis for a five-year U.S. solar energy research and development program organized into the following areas:

*This report can be obtained from the National Technical Information System (NTIS), Department of Commerce, Springfield, Va., 22151, Document PB-221659 (2.75).

(1) Heating and Cooling of Buildings
(2) Solar Thermal Energy Conversion
(3) Photovoltaic Conversion
(4) Bioconversion
(5) Wind Energy Conversion
(6) Ocean Thermal Energy Conversion

Five-year objectives and plans and five-year budget projections to implement these plans have been formulated for each of the solar energy program areas. The specific objectives of each of these areas are summarized in Table 1.

The NSF budgets for solar energy applications in past years and for the current year are shown in Table 2. The relatively large percentage increases in total funding are very apparent over the period from FY 1971 to FY 1974. The funding in FY 1971 to FY 1973 has represented a start in initiating a solar energy research program in several application areas. The estimated funding in FY 1974 is the first year funding requested by NSF in an integrated five-year plan to move ahead more rapidly in six identified application areas. The changes in emphasis between application areas in FY 1971 to FY 1973 arise because of relatively limited funds and as a result of continuing analysis of priorities and opportunities. The estimated FY 1974 Federal funding of solar energy research and technology performed outside of government laboratories is $15.6M. The number of Federal agencies considering some external funding for FY 1974 is five, including NSF, NASA, the Department of Housing and Urban Development (HUD), the Atomic Energy Commission (AEC), and the Department of Defense (DOD). In addition, there are a number of Federal inhouse research and technology projects that will add to the Federal total funding. Table 3 summarizes the number and total value of current projects being supported by the various Federal agencies at non-federal institutions. There are 52 projects listed with a total value of $8.5M. A large fraction of these projects have been initiated in FY 1974.

As of October, 1973, about $4M in federal funds have been expended on solar energy research projects initiated during the last three fiscal years. A total of about $10M has been obligated up to the present time. Total federal energy R&D funding by technology areas is shown in Table 4.

Phased Project Planning

The NSF planning for implementing solar energy applications emphasizes a phased project planning approach embodying integrated programs of multidisciplinary research, analysis, experiments, and system studies. The more important steps in phased project planning leading to a new application are shown in Table 5. This approach will be followed in organizing all of our projects. The research phase includes basic and applied research on new techniques for solar energy conversion; research and analysis on innovative ideas, materials, components, subsystems, or systems; basic data required for systems analysis; research to show scientific or technical feasibility, etc. Proof-of-concept experiments (POCE) are major milestones in the program plan. After a POCE, the plan continues with the demonstration system and commercial system phases.

The steps from Phase 0 to Phase 2 lead from the Research Phase to an operational system. This experimental system is used to measure system performance and to obtain initial data on system reliability, lifetime, and operational and maintenance problems. The measured performance is compared to the predicted performance to check the design calculations for the integrated system. In addition, the results and experience from the system POCE provide a better basis for projecting the economic viability of a larger-scale system, such as a demonstration system or a commercial system.

The result of the Phase 0 efforts is to identify the most economically viable systems for continuing into Phases 1 and 2; and, to identify critical areas of technology that require further research. Supporting research and technology is focused on these critical areas to improve the probabilities for success in the later phases. In parallel with the Phase 0 work, a continuing program of advanced research and technology will be planned to obtain improved performance of existing materials, components, subsystems, and systems and to provide new options for these same elements. The advanced work can lead to an improved first generation system POCE but is directed primarily to a second generation system.

The result of Phase 1 is a completed preliminary design of the system experiment that can be projected to a successful conclusion of Phase 2.

A successful Phase 2 provides performance data and operational and maintenance experience to verify the analysis and design for a reliable, predictable system. In addition, the projected economics for a larger system must show that industrially produced and installed systems can be economically viable on a system lifetime basis compared to other energy alternatives.

Each phase in the phased project planning employs major milestones to permit technical, environmental, and socioeconomic evaluations and assessments to be made prior to entering a commitment to proceed.

Proof-of-Concept Experiments

A system proof-of-concept experiment is undertaken to prove that the full technology base is available to enable the user community to move toward the design and development of economically viable systems.

Other forms of proof-of-concept experiments are subsystem proof-of-concept and engineering system proof-of-concept. Subsystem experiments are programmed as soon as possible in the research and technology programs to verify the performance, lifetime, and operational and environmental responses associated with subsystems making up parts of a full system. Engineering system proof-of-concept is an early system experiment to obtain performance data and operational experience. Engineering system experiments are designed to be flexible to accommodate system changes or improved subsystems. The performance and other data from these proof-of-concept experiments are checked with predictions based upon previous analysis and experiments.

In the federal solar energy program, the NSF and other agencies can be involved in the phased project planning steps through Phase 2. After the completion of Phase 2 in the system proof-of-concept experiment, the NSF intends to pass the direct project procurement and management on to mission-oriented agencies and departments to implement Phases 3 and 4. This technology transfer function is important to allow the primary efforts within NSF to be directed to the research and technology phases leading to system proof-of-concept experiments in other applications. At the same time the user agencies can apply their larger resources to the management problems involved in large-scale implementation of relatively proven, economically viable systems that can be regarded as alternative energy and power sources for the nation's needs.

Table 6 shows in summary form the progress that is planned over the next few years in undertaking the estimated budget for FY 1974. The plan shows that by the end of FY 1978 system proof-of-concept experiments will be completed in three program areas: heating and cooling of buildings, wind energy conversion, and bioconversion to produce methane gas from organic wastes. In the other areas, the plan calls for the research to progress into Phase 1 of the proof-of-concept experiment.

Research Utilization

A research utilization plan is required for each project. This plan is aimed as maximum utilization of the results of research funded under the project. The utilization plan is directed toward the industrial and commercial enterprises as producers; the public sector of federal, state, and local government as regulators and controllers; as well as the general public users.

A major emphasis in the solar energy program is the utilization of solar energy for the production of electricity. In FY 1974 component and subsystem proof-of-concept experiments will be initiated to evaluate the quality and costs of photovoltaic arrays and systems. Also alternate approaches for fabrication of solar cells and for new solar cell materials will be undertaken. An analysis will be initiated for photovoltaic systems for a variety of applications, e.g., residential power, remote power stations, and special commercial power needs. Integral to these studies is the development of a utilization plan that will ensure the rapid implementation of solar systems by industry, mission agencies, and other interested parties when the proof-of-concept experiments are completed.

Other utilization activities under way in solar energy include workshops such as this one to exchange research information, discuss solar energy program objectives, and obtain feedback from researchers and users; the preparation of a handbook on the application of solar energy for heating and cooling of buildings; and, the publication of project and workshop reports. Seven workshops have already been conducted in solar thermal energy conversion, heating and cooling of buildings, wind energy conversion, ocean thermal energy, and three aspects of bioconversion. We expect a vigorous program to continue in this area in the future.

NSF/RANN Photovoltaic Conversion Program

The general objective of the Photovoltaic Energy Conversion Program is to develop low-cost, long-lived, reliable photovoltaic systems resulting in the commercial availability of these systems for a variety of terrestrial applications, capable of producing a significant amount of energy. An initial five-year program has been developed to achieve the following specific objectives:

(1) To undertake proof-of-concept experiments showing a factor of ten reduction in production solar array costs (presently about $50 per peak watt);

(2) To conduct a broad research and development program on photovoltaic devices that show a potential for a factor of one hundred or greater reduction in production costs;

(3) To conduct systems and applications studies to identify suitable proof-of-concept experiments of photovoltaic energy conversion systems and to conduct marketing and business planning studies for these applications;

(4) To conduct proof-of-concept experiments showing a factor of one hundred reduction in solar array production costs; and

(5) To conduct proof-of-concept experiments of photovoltaic systems to provide electrical energy for buildings and central power stations.

The first objective will be achieved by focusing efforts on the present state-of-the-art silicon cells since these cells have received significant research and development support in the past and show strong promise for cost reduction. The tasks in support of the second objective include development of single-crystal ribbon and polycrystalline thin-film silicon solar cells and thin-film devices from a variety of promising semiconductor materials, such as CdS and other II-VI compounds, GaAs, certain metal-oxide compounds, and organic materials. The fourth objective will be achieved by selecting the most promising of those photovoltaic devices developed under objective two and moving them into the proof-of-concept phase. The fifth objective will be achieved by implementing the system proof-of-concept experiments identified under objective three.

The federal funding for terrestrial photovoltaic conversion is presented in Table 7. A summary of current grants in the NSF/RANN Photovoltaic Conversion Program is given in the Appendix of the Proceedings.

National Plan for Photovoltaic Conversion of Solar Energy

Research funding for terrestrial applications of solar energy has come during the past few years from a number of federal agencies. Most of the federal funds obligated for support of external research projects have come from the NSF/RANN solar energy program. In April of this year the lead federal agency role in terrestrial solar energy applications was assigned to the NSF by the President's Office.

The Interagency Panel for Terrestrial Applications of Solar Energy (IPTASE) was organized by NSF to coordinate Federal activities in solar energy research and technology. The panel held its first meeting in June, 1973. This panel, which now includes representatives of about ten departments and agencies, has had monthly meetings during FY 1974. Staff from these agencies are coordinating research and development activities, funding resources, laboratories, and staffs to provide domestic energy supply alternatives based upon U.S. solar energy resources. Agency staff participating in program development have come from the National Aeronautics and Space Administration (NASA), the Atomic Energy Commission (AEC), the Department of Defense (DOD), the Department of Housing and Urban Development (HUD), the National Bureau of Standards (NBS), the General Services Administration (GSA), the U.S. Department of Agriculture (USDA), the Environmental Protection Agency (EPA), the Department of Interior (DOI), and the Department of Transportation (DOT).

In response to the President's initiative of June 29, 1973, to formulate a five-year, ten-billion-dollar program for energy research and development, the IPTASE members were invited to participate under NSF leadership in the formulation of a coordinated national program for research on solar energy applications. The response was rapid and wholehearted. The resulting national program has now been submitted for evaluation in competition with programs involving other types of conventional and less conventional energy resources. The selection of priorities and programs through this evaluation procedure may determine in a large measure the course of solar energy research, and other energy research, for the immediate future.

While the details cannot be discussed at this time, I would like to summarize the main points of the plan for photovoltaic conversion. The direct participants in this exercise who are with me today to discuss this plan are listed below..

H. Bennett	NBS
D. Bernatowicz	NASA – Lewis R.C.
W. Cherry	NASA – Goddard S.F.C.
J. Goldsmith	NASA – JPL
F. Morse	NSF
W. Siekhaus	AEC – LBL
N. Yannoni	DOD – AFCRL

In order to achieve the five major objectives of the Photovoltaic Conversion Program outlined above, a plan has been proposed that consists of two major areas each consisting of three subprograms:

Area 1: Research and Development of Photovoltaic Arrays for Terrestrial Applications

- Subprogram 1: Silicon Solar Cell Arrays
- Subprogram 2: Cadmium Sulfide Solar Cell Arrays
- Subprogram 3: Other Materials and Devices

Area 2: Application of Photovoltaic Energy Conversion Systems to the Power Needs of the Nation

- Subprogram 4: Photovoltaic Conversion Systems for On-Site Power
- Subprogram 5: Photovoltaic Systems for Central Power
- Subprogram 6: Test and Evaluation Laboratories

A detailed description of the plans for these two areas follows.

Research and Development of Photovoltaic Arrays for Terrestrial Applications

Introduction

Single-crystal silicon conversion devices are presently employed in all practical solar photovoltaic power sources. As we have heard in this meeting, it is the cost of these devices which primarily determines the cost of the array at this time. In order to become economically feasible, the converter cost must be reduced as well as low-cost techniques be developed to efficiently integrate these devices into completed modules ready for field installation. This total cost should be less than $0.50/watt (peak). Single-crystal silicon solar cells can become economically viable if processes can be developed to produce high quality, single-crystal silicon through a low-cost growth technique, suitable for incorporation into a continuous array fabrication process. Single-crystal silicon is not the only material from which terrestrial solar arrays can be fabricated. There are other techniques and materials in various stages of development which could prove to have equivalent or better terrestrial application cost advantages than single-crystal-silicon. Potentially better results may be realized by producing converters from films of such materials as: polycrystalline silicon; cadmium sulfide; gallium arsenide, and other III-V materials; metal oxides and organic compounds. The effort to develop CdS solar cells has provided interesting results. Devices have been fabricated which have shown conversion efficiencies up to 6%. Although these cells will be less efficient than silicon they possibly could be manufactured at considerably less cost per square foot, and hence, ultimately demonstrate a comparable or lower dollar per watt system cost. The major technical problems limiting its terrestrial application are associated with its instability.

There are advantages and technical problems associated with other photovoltaic converters. A conscientious effort to develop these alternate solar photovoltaic converter technologies could lead ultimately to devices/arrays which would convert solar energy to electrical power at considerably less than $0.30/watt (peak).

Program Plan

The development of practical solar cell arrays technology for terrestrial applications will require the organization and continuation of a significant technical effort that must treat all steps from basic device research to the production of a solar cell array, ready for field installation. Major aspects of the efforts will include the production of acceptable quality silicon, the production of continuous single-crystal silicon photovoltaic devices, the development of stable CdS cells, the development of other potential film-type devices, and the continuous automated processes to integrate these devices into arrays. It will require a significant contribution of the nation's silicon semiconductor industry as well as the production know-how of those specialists in mass production techniques.

The first objective, to reduce solar photovoltaic cost to $5/watt (peak), will be accomplished by automation of the present batch process for making cells from silicon single-crystal wafer material. These reduced cost cells will make practical many small-scale applications and thereby promote an expansion of the solar cell and array industry. These cells will also be used in early projects to develop and demonstrate solar cell systems for large-scale users, notable solar cell-powered residences.

Further cost reduction to less than $0.50/watt/(peak) will very likely require development of new techniques. Presently small research and technology activities on various new methods will be greatly expanded. One approach includes ways of growing single-crystal silicon ribbons directly from molten silicon and then developing low-cost, continuous methods of fabricating finished cells and arrays.

Another technique which will be pursued is to develop less than $0.50/watt (peak) technology through the production of stable cadmium sulfide cell arrays. At the present time CdS solar cells are made on a hand batch basis in sizes of 3 × 3 in., starting with evaporation of the CdS on a thin conductive substrate followed by a dip in copper sulphide solution and then baking. A conductive grid is next placed on the active surface and finally the whole assembly is sealed in a transparent encapsulant. Providing solution of the materials/device problems, the process steps can be greatly automated and made cost effective.

The longer range goal of further reduction of cost to less than $0.30/watt (peak) is considerably more speculative and will depend upon the development of materials and film device technology which is in the very early stages of development. The risk associated with depending upon the availability of this advanced film array technology is great; however, the potential payoff in ultimate cost savings to the users merits the continuation of a significant research effort in this area of advanced thin film arrays. Materials and devices of present major interest in this area are polycrystalline silicon, GaAs and other III-V compounds, metal oxides, organic materials, new Schottky devices and photovoltaic materials with strong sensitivity in the infrared.

Necessarily, an aggressive effort of the nature described is based upon capitalizing on the best existing technology while anticipating good progress in all areas of development. Good program management will always seek to minimize research investment in areas which do not have the potential of returning more in improvements than was spent in development of the technology. It is a major concern that money not be wasted, that projects be carried far enough so that their real potentials can be evaluated, that technology be demonstrated at a level that can be evaluated by industry, and that the economics of the systems can be realistically ascertained. The Photovoltaic Conversion program is organized to employ this philosophy. Considering the increasing energy demand facing the United States, the availability of two or more economically competitive solar converters (e.g., silicon and CdS) will probably prove to be very advantageous.

Milestones

Four major milestones associated with the development of photovoltaic arrays for large-scale terrestrial application have been identified:

(1) 1977: attainment of $5/watt (peak) technology

(2) 1979: attainment of $0.50/watt (peak) technology feasibility

(3) 1981: completion of a pilot line to manufacture $0.50/watt (peak) solar arrays

(4) 1986: completion of a pilot line to manufacture $0.30/watt (peak) solar arrays

The major initial direction of this program will be to develop low-cost single-crystal silicon ribbon solar technology in parallel with film CdS cell development. This direction is selected as the initial step because of the high potential of success either of these techniques offers providing the solutions can be found for the continuous silicon ribbon process problems and the CdS instability problem. In parallel with this effort a serious, significant effort will be applied to the development of other film technologies. It will be a long-range program goal to develop a film technology capable of demonstrating an economy of less than $0.30/watt (peak) of electrical power by 1986.

Application of Photovoltaic Systems to National Needs

Objectives and Program Plan

It is anticipated that large-scale application of solar photovoltaic technology will become economically viable by approximately 1980. This will be made possible by the reduction of solar array cost to less than $0.50/watt (peak). Providing this technology to potential users will not be enough due to the unconventional nature of this power source. Effort must be invested in evaluating solar system and user problems. Such factors as providing meaningful system design data to efficiently size the power source; techniques to evaluate the suitability of various devices for particular applications and the problem of power transfer, control and overall economy and efficiency need to be evaluated. It is the objective of this subprogram to provide the basic information required. The specific objectives of this effort include:

(1) Develop the technology, personnel and facilities to provide the basic insolation data, and the calibration, standardization, testing and control capabilities needed for large-scale solar photovoltaic application.

(2) Design, develop, integrate and test a photovoltaic solar array system as it would be required for individual residential user or equivalent power level applications in the United States.

(3) Design, develop, integrate and test a large central power station system as it would be applied to satisfying industrial, commercial and residential energy needs.

Milestones

The following milestones have been identified:

1975:	Insolation data collection network established
	Materials characterization and analysis laboratory established
1976:	Standards and calibration laboratory established and operated
	Terrestrial environmental test facility operated
	Maximum allowable costs of photovoltaic systems for on-site and central station applications in several U.S. locations (taking into account meteorological data and the effect of such systems on communities, environment and society) determined.
1977:	On-site system design completed
	Testing of cells and arrays
1979:	On-site system installed and testing initiated
	Central Station System design completed
1982:	Photovoltaic systems in the range of 0.01-1.0 MW into new and existing buildings (homes, schools, shopping centers, etc.) integrated.
1985:	Photovoltaic systems of about 10-MW capacity into communities and large industrial plants integrated.
1990:	Photovoltaic systems of greater than 100-MW capacity into towns and power networks integrated

Cost Projections

Projected costs of three representative systems with the high efficiency low-cost arrays that are the goal of this subprogram are listed in Table 8. The annual cost of capital (interest, taxes, depreciation, maintenance, insurance) was assumed to be 15.5% of initial investment over a twenty-year period. The projected rate of implementation is given in Table 9. Solar array systems will be capital intensive, but have low operating and no fuel costs. Investment costs will eventually be below $1000/kw installed average generating capacity and operating costs may be similar to those experienced in hydroelectric installations. The possible impacts of large-scale implementation of photovoltaic conversion technology are summarized in Table 10. Sand, the source of silicon, is so abundant as to present no resource limitation. However, the silicon reduction and refinement industry will have to be expanded by two or three orders of magnitude to provide for photovoltaics as well as the greatly expanding electronic device industry.

If CdS cells are used predominantly, then about 150,000 tons of cadmium would be needed to generate 1% of the year 2000 U.S. electric power needs exceeding the known U.S. reserves of 130,000 tons available at 1971 prices. The plating industry would be major competitors for cadmium. Plastics are likely to be the encapsulants and perhaps structural elements of arrays. The amount of hydrocarbons needed to manufacture the plastic has not been estimated. Aluminum and steel are the likely other structural materials and will not be a significant portion of reserves.

A photovoltaic power plant can come on line in segments during its construction while other systems must be completely built. This means it can start earning sooner. The solar energy industry will have a modest impact on the labor market amounting to no more than a few percent. By the turn of the century photovoltaic processes could produce a percent or two of the nation's electrical needs saving some $10 billion in fossil fuels per year, much of which would represent imports. This would help in reducing balance of payments deficit. Also the U.S. might export multi-million dollar solar cell systems.

Foreign markets should be extensive especially in regions of abundant sunshine. By the 1990's, as fossile fuels become supply critical, world-wide billion dollar markets may develop.

Conclusions

The general objective of the NSF/RANN Photovoltaic Energy Conversion Program is the development of low-cost, long-lived, reliable photovoltaic systems, resulting in the commercial availability of these systems for a variety of terrestrial applications, capable of producing significant amounts of energy. The specific program objectives and associated milestone dates are:

(1) To conduct proof-of-concept experiments showing a factor of ten reduction in solar array costs to $5.0/watt (peak) by 1977.

(2) To conduct a broad research and development program on photovoltaic devices to demonstrate the attainment of a $0.50/watt (peak) technology by 1979.

(3) To conduct systems and applications studies to identify suitable proof-of-concept experiments of photovoltaic systems by 1977 and 1979, respectively.

(4) To conduct proof-of-concept experiments of photovoltaic systems to provide electrical energy for on-site and central stations - by 1979 and 1985, respectively.

The major initial direction of this program will be to develop low-cost silicon solar array technology in parallel with thin-film cadmium sulfide array development.

The $5.0/watt (peak) objective will be achieved by automation of the present batch process for making single-crystal silicon wafer cells. Further cost reduction to $0.50/watt (peak) will require the development of new techniques, presently under study, such as continuously growing single-crystal silicon ribbon arrays and the continuous production of cadmium sulfide arrays. The longer range goal of further reduction of cost to $0.30 to 0.10/watt (peak) will require the development of an advanced thin-film technology using, for example, polycrystalline silicon, gallium arsenide, other semiconductors, or metal oxides, or organic materials.

The estimated electric power cost for a 1-kw average residential photovoltaic system using $0.50/watt (peak) arrays is 7¢/kwh, based on a 20-year lifetime, 14% overall system conversion efficiency, and a 15.5% cost of capital over a 20-year period. The electric power costs drop to 1.6¢/kwh with the use of $0.1/watt (peak) arrays. These $0.1/watt (peak) arrays are projected to produce power at 1.8¢/kwh for a 10-MW central station.

Discussion

Q: Are you going to make some of these charts available in a hand-out form?

A: This will be a part of the proceedings. Part of this talk will also appear at the IEEE Photovoltaic Specialists Conference and will be in the Proceedings for that conference as well.

Q: With regard to this 10 billion dollar energy R and D program, do you know if each agency has submitted a large program so that the total is way over 10 billion, and what is the probability of Congress passing this?

A: This is a recommendation, or I should say, a set of proposals that are being evaluated for the Chairman of the Atomic Energy Commission by a series of panels organized under the AEC. The report on the recommendation of these panels will go from the Chairman's office to the President as currently planned on December 1, 1973, and we would normally assume it then will go into the Legislative stream.

Each agency has its own plan. The National Science Foundation is in the first year of a five-year program, and I'm sure that NASA also has plans.

Q: I noticed in your ratio of support that the silicon program has three and a half times as much as the cadmium sulphide, and that other materials have even less. I wonder how you feel about those ratios in view of the proceedings of the workshop?

A: I have not put the numbers down from this morning's recommendations, but I think they may be fairly close to what actually came out of these recommendations. Does somebody have that ratio?

C: For five years we had something like 530 million dollars. Of that, single-crystal silicon is 250 million, polycrystalline silicon is fifty million, cadmium sulfide is 185 million, materials and devices is 10.5 million, insolation and evaluation is 20 million, and systems is 15 million. If you add up the two silicons it comes to approximately 57 percent of the total and 35 percent for cadmium sulfide.

C: I should make some remarks and make a public recantation of my slides on the "Other Materials and Devices." When I spoke with some members of the group who saw for the first time what was presented here this morning, they have been quite unhappy on the grounds that this was much lower than what they thought was appropriate. We hadn't discussed money at that meeting last night, as I said, and I have had numbers like a factor of two more suggested as being more appropriate.

Q: With respect to the direct additions of the Systems Group and the analysis of our panel, would you review the applications sequence that you have suggested?

A: We certainly will, and we found the discussion today quite interesting. It will have a substantial impact on our thinking. A question might be, "How does this workshop information now affect NSF plans? You might say, "You have already submitted a written document," but anyone who has dealt with any of these exercises knows that the next step is a further justification of what has been submitted with a slightly modified format, perhaps. So there will indeed be sufficient opportunity to try to modify the plan and to reconsider the plan before it is final.

Q: The plan includes the fiscal 1975 budget?

A: Yes.

Q: Are you saying that the fiscal 1975 budget is not established yet?

A: That is correct. There is an on-going FY '75 submission on the part of all agencies at this time, but this is being considered separately, and presumably would eventually get folded.

Q: You mentioned the establishment of a laboratory – a materials characterization laboratory. Do you have any more information on this particular laboratory? What will its functions be?

A: The relationship between a materials characterization laboratory and the cognizant authority for gathering of insolation data has a problem in that, if you are going to gather data, you will need agreement about the methods of obtaining the data. The problem is, in the process of taking such data, are you going to be using material with the agreed properties? Will you have two separate centers or will it just be one primary laboratory? I don't think that has been worked out in nearly enough detail to answer your question right now.

And I would also like to mention that the Materials Research Center, under the Material Research Division in NSF, is interested in participating in this program.

C: As you may or may not know, NSF had twelve interdisciplinary labs prior to FY 1973, and we added two more in FY 1973. It is not clear what role these laboratories will play with respect to some of the basic research needs in this photovoltaic conversion program. That remains to be defined. Certainly there is much basic research on the way right now in this area in many existing labs. Of the half million dollars in my program called Engineering Materials Supports, roughly four out of every five dollars is allocated to individual research project support. There was another question about the budgetary process. FY 1975 becomes public information when it gets submitted or transmitted to Congress, which is around January 29 or 30, 1974.

Table 1. NSF/RANN Solar Energy Program

Program Element	Objective
Heating and cooling of buildings	To establish the widespread utilization and availability of systems using solar energy for heating, cooling, and supplying the hot water needs for buildings in the United States to the degree that the systems applications are economically viable, technically feasible and socially acceptable.
Solar thermal conversion	To prove the technical and economic feasibility of solar thermal conversion systems providing electrical or combined electrical and thermal service.
Photovoltaic conversion	To develop low-cost reliable photovoltaic conversion systems, capable of producing a significant amount of energy, resulting in widespread availability for terrestrial applications.
Bioconversion	To prove the economic feasibility for large-scale conversion of waste, cultivated organic materials, and water to gaseous, liquid, and solid fuels using bio-organisms.
Wind conversion	To develop reliable, cost-competitive wind energy conversion systems capable of rapid commercial expansion on a significant scale.
Ocean thermal conversion	To establish system reliability and economic viability of large-scale power plants converting ocean thermal energy into electrical energy.

Table 2. NSF/RANN Solar Energy Budget (in Millions of Dollars)

	FY 1971 (Actual)	FY 1972 (Actual)	FY 1973 (Actual)	FY 1974 (Estimate)
Solar energy for buildings	$0.54	$0.10	$0.40	$ 5.60
Solar thermal conversion	0.06	0.55	1.43	2.20
Photovoltaic conversion		0.33	0.79	2.60
Bioconversion for fuels	0.60	0.35	0.65	1.10
Wind conversion			0.20	0.80
Ocean thermal difference conversion		0.14	0.23	0.60
Workshops and program assistance		0.19	0.26	0.30
Totals	$1.20	$1.66	$3.96	$13.20

Table 3. U.S. Solar Energy Research Program Summary

	Projects			Total Projects	Total Funds Obligated
	NSF	NASA	HUD		
Heating and cooling of buildings	8	1	1	10	$1.293M
Solar thermal conversion	5	1		6	1.634
Photovoltaic conversion	9	5		14	1.578
Bioconversion	9	1		10	1.641
Wind energy conversion	3			3	0.201
Ocean thermal conversion	3			3	0.313
Other projects	1			1	0.152
Technology assessments	1			1	0.247
Phase 0 – heating and cooling of buildings	3 (est)			3 (est)	1.500 (est)
	42	8	1	51	$8.559M

Table 4. Federal Energy R&D Funding by Technology Areas (in Millions of Dollars)

	FY 1970	FY 1971	FY 1972	FY 1973	FY 1974
Coal	30.4	49.0	73.5	94.5	119.9
Oil and gas	8.8	11.5	12.9	12.8	9.1
Nuclear fission	283.4	295.2	358.0	412.0	475.4
Nuclear fusion	37.5	42.2	52.8	65.5	88.5
Geothermal energy	0.2	0.2	1.4	3.4	4.1
Solar energy		1.2	1.7	4.2	12.2
Control technology	22.1	19.8	28.6	38.1	47.5
Other		1.3	8.5	11.8	15.1
Totals	382.4	420.4	537.4	642.3	771.8
Supplement					100.0
					871.8

Table 5. Steps in Phased Project Planning to Develop a New Application

Research phase		Analysis and test of new procedures
		Interdisciplinary research and systems analysis
* * * * * * * * *		Research on materials, components, and subsystems
Phase 0		Conceptual design and requirements specification
	P	Economic analysis and impact assessment
	O	Research on critical materials, components, and subsystems
Phase 1	C	Preliminary system design
	E	Critical subsystem research, design, and test
Phase 2		Detailed system design
* * * * * * * * *		System construction, test, and evaluation
Phase 3		Demonstration system design, construction, and operation
Phase 4		Commercial system design, construction, and operation

Table 6. Terrestrial Solar Energy Program

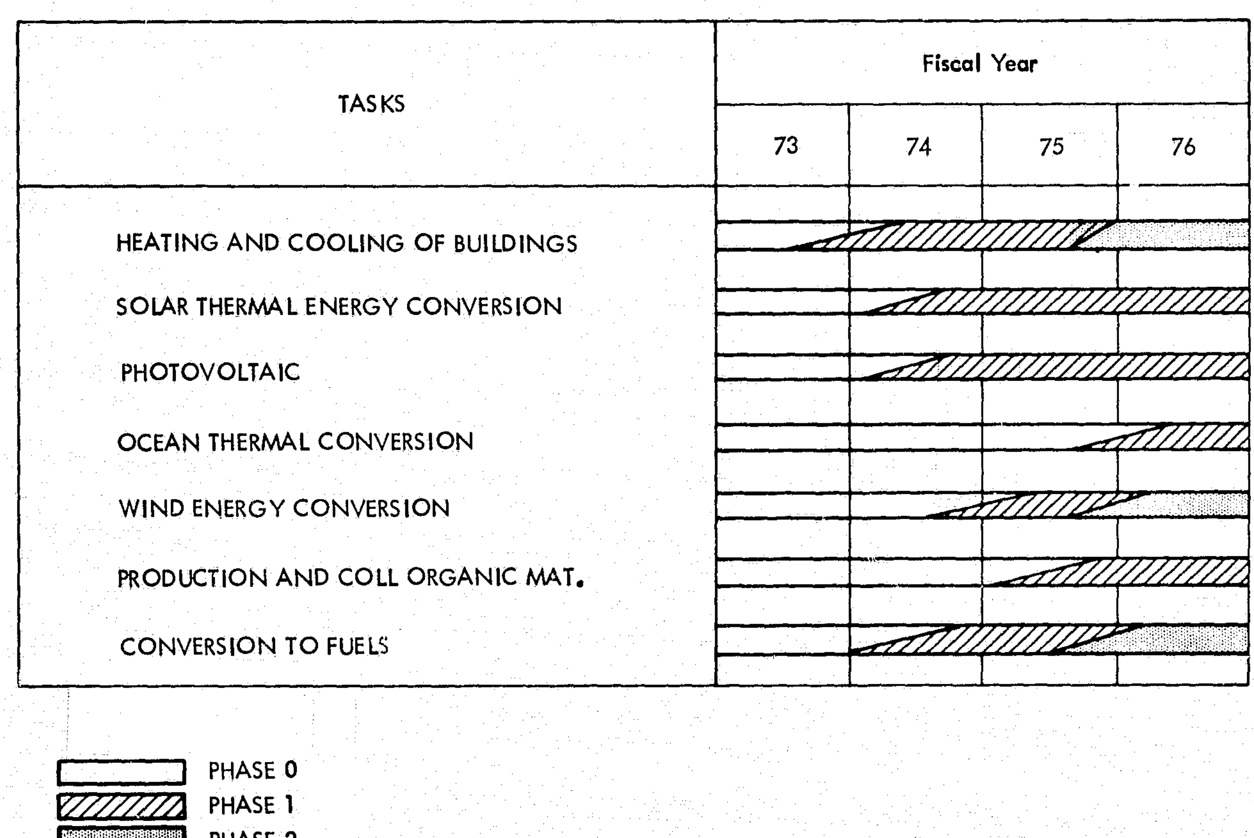

Table 7. Photovoltaic Conversion Program

Federal Funding		FY 1974	FY 1973	FY 1972	FY 1971
(External)	NSF	$2600K (est)	$794K	$359K	
	NASA (Lewis, JPL) (Terrestrial)	179K	130K	50K	$30K

Other Participating Federal Organizations: NASA and DOD

Table 8. Economics of Implementation – Photovoltaic Conversion

Type/Time	Average* power, KW	Area, ft^2	Array, $/watt (peak)	System, $\$X10^3$	Operating, $\$X10^3/yr$	Life, yr	Power cost, ¢/kwh	Life, yr	Power cost, ¢/kwh
Residence/1985	1	420	$0.50	3	0	20	7	30	5
Central station/1990	10,000	4.2×10^6	0.10	7000	100	20	1.8	30	1.2
Residence/1990	1	420	0.10	1	0	20	1.6	30	1.0

* Average output power = Integrated peak insolation X (duty factor) X (system** efficiency)
= (constant over 6 hours) X 1/5 X (14%)

** System efficiency = (Basic cell conversion eff) X (packing factor) X (power condition eff) X (overall loss eff)
= (21%) (85%) (90%) (90%)

Table 9. Rate of Implementation – Photovoltaic Conversion

Year	Peak Power Output Capability of Arrays Produced in One Year (MW)	Cumulative Output (MW)
1981	1	1
1983	10	13
1985	1000	1100
1990	5000	10,000
1995	10,000	40,000
2000	20,000	100,000*

*AUI Projected Electrical Generating Capacity (U.S.) Required in the year 2000 is 1636 mkw(e). This would then be (at peak output) 6% of U.S. requirements.

Table 10. Impacts of Implementation – Photovoltaic Conversion

Natural Resources Required

Energy and Capital Inputs Required

Compatibility with Existing Energy Systems

Environmental Impacts

Occupational Health and Safety

Other factors (e.g., future demographic and land use patterns, social costs and benefits, long term impact)

C 18

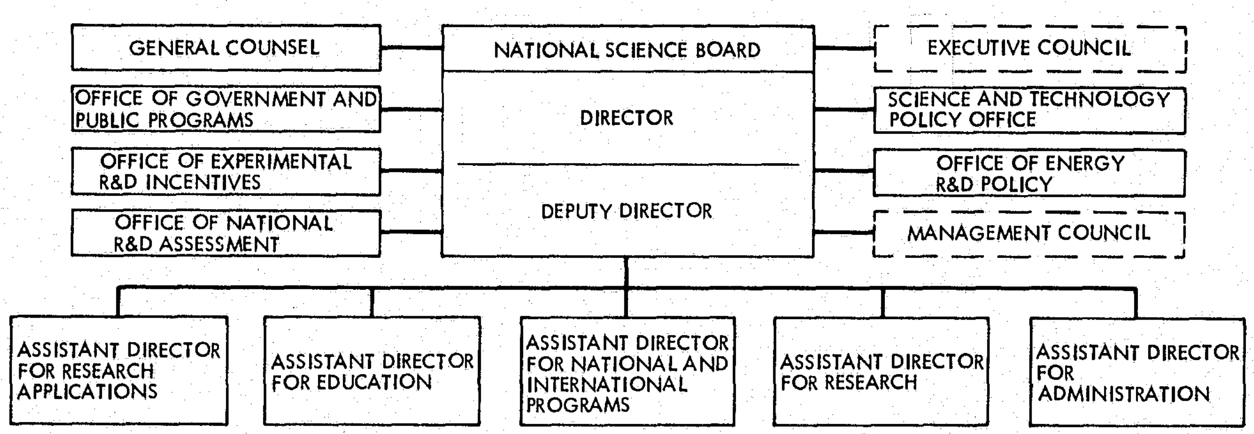

Fig. 1. National Science Foundation Organization

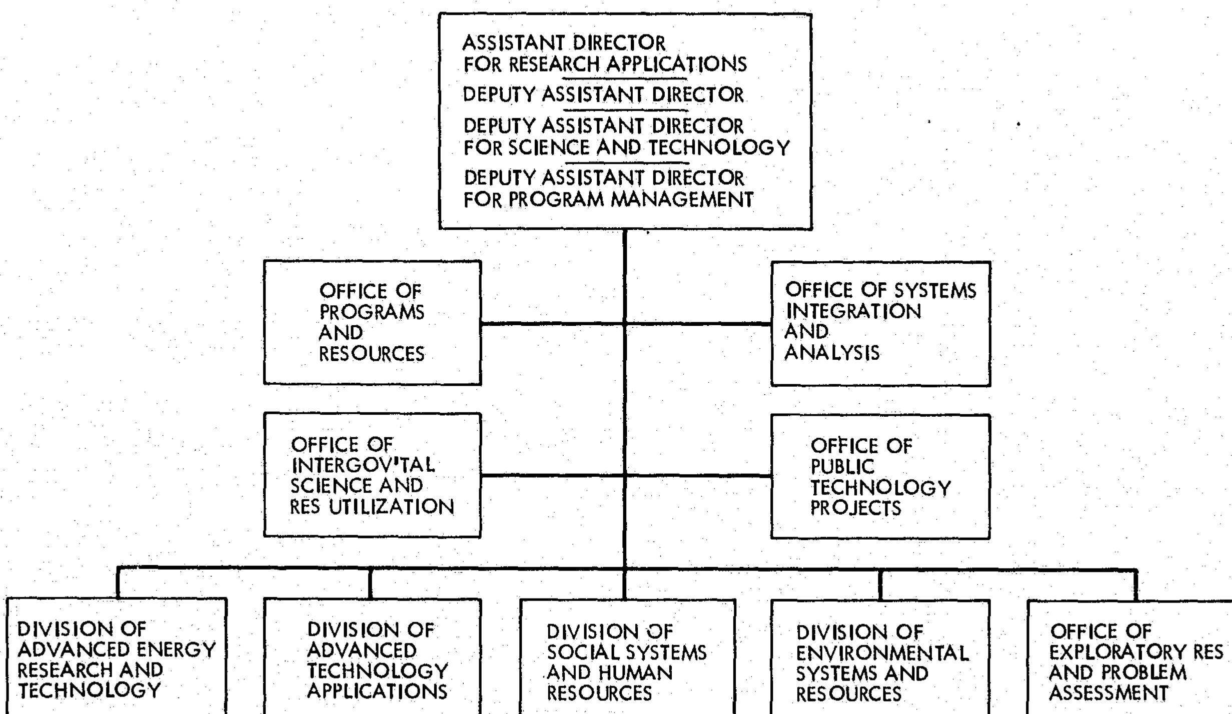

Fig. 2. Research Applications Directorate

ACKNOWLEDGMENT

The Photovoltaic Energy Conversion Program presented in this paper is the result of the efforts of many people from universities, industry, and government laboratories. Starting with the program developed as part of the NSF/NASA Solar Energy Panel, many meetings, workshops, and planning exercises were held in order to further define the program. Several individuals contributed significantly to this effort: Fred Morse, Lloyd Herwig, Daniel Bernatowicz, William Cherry, John Goldsmith, Wigbert Siekhaus, and Nicholas Yannoni. Their contributions and those of the others are gratefully acknowledged.

APPENDIX

CURRENT NSF PHOTOVOLTAICS PROJECTS

The NSF/RANN solar energy program is presently supporting eleven photovoltaics projects involving university, industry and university/industry joint efforts. It is anticipated that as the program develops, the number of projects will grow significantly, showing a shift towards greater industrial participation and support. The objectives of the current NSF photovoltaics projects are shown below:

(1) "Low-Cost Continuous Fabrication of Silicon Solar Cells;" – Harvard University/Tyco Laboratories; Bruce Chalmers/A. I. Mlavsky

The goal of this project is the development of techniques for low-cost continuous production of silicon crystal ribbon for continuous manufacture into low-cost silicon solar cells. A technique of crystal growth has been developed by the university and industrial collaborators in this proposal and has been previously applied to the production of continuous sapphire single crystal shapes including large single crystal ribbons. Their technique of Edge-defined, Film-fed, Growth (EFG) of single crystals is a process by which single crystals may be grown having a shape controlled by the outside dimensions of a die with the crystal growth taking place from a very thin film of liquid fed by capillary action from a crucible below. This project proposes research to develop the basic understanding and the engineering processes necessary for the application of the EFG process to the growth of silicon single crystal ribbons that can be used in continuous production of silicon solar cells.

(2) "Low-Cost Silicon Photovoltaic Cells for Large Solar Power Systems" – Boston College; P. H. Fang

This project is concerned with exploratory research to examine advanced methods for fabricating thin films of polycrystalline silicon photovoltaic cells. The longer term goal is to develop automated continuous processes for producing these cells and to reduce cell costs per watt substantially below those of present production methods. The advanced methods of fabrication to be studied will include: (1) vacuum evaporation of silicon onto flexible substrates at high temperatures, to try to obtain satisfactory thin (perhaps 10 microns) polycrystalline films suitable for large-area, photovoltaic cells; (2) sputtering of silicon to form thin polycrystalline films; (3) electron-beam ion plating of silicon to form suitable films; and, (4) chemical vapor deposition of silicon containing gases onto heated substrates to obtain suitable polycrystalline films.

(3) "Development of Low-Cost Thin Film Polycrystalline Silicon Solar Cells for Terrestrial Applications" – Southern Methodist University/Texas Instruments; Ting L. Chu

The objective of this project is the development of low-cost thin film polycrystalline silicon solar cells suitable for large-scale terrestrial utilization. Specific goals include: (1) the deposition and characterization of polycrystalline silicon films of adequate quality, (2) the preparation and characterization of suitable junction, of the p-n type and/or the Schottky barrier type, and (3) the fabrication and evaluation of thin film solar cells having efficiencies and cost projections warranting further research and development support.

(4) "Direct Solar Energy Conversion for Large-Scale Terrestrial Use" – University of Delaware; Karl W. Böer

This research project is directed to further understanding and development of CdS/CU_xS solar cells to obtain longer life, higher performance, more economical cells for applications in large and small-scale solar energy conversion systems. The principal objectives are: (1) improved understanding of the basic properties and conversion mechanisms of CdS/CU_xS cells, (2) improved cell lifetimes and methods for accelerated lifetests (goal in excess of 20-year lifetimes), (3) improved performance and conversion efficiency at elevated temperatures, and (4) improved production techniques to increase reliability of cells and decrease production costs.

(5) "Investigation of Thin Film Solar Cells Based on Cu_2S and Ternary Compounds" – Brown University; Joseph J. Loferski

The objective of this project is the investigation of thin film solar cells based on Cu_2S and ternary compounds of the type $CuInS_2$ for large-scale, hence low-cost, terrestrial solar energy utilization. Specific goals include the fabrication and testing of: (1) metal-semiconductor photovoltaic cells consisting of Cu_2S or Cu, (2) homojunction cells involving $CuInS_2$ on Cu, (3) heterojunction cells involving $CuInS_2$ on Cu, (4) heterojunction cells of $CuInS_2$, (5) heterojunction cells consisting of P-type $CuAlS_2$, (6) heterojunction cells of Cu_2S on single crystal Si, and, (7) homojunction cells involving $CuInSe_2$ and $CuInSe_xS1$-x.

(6) "Applied Research on II-VI Compound Materials for Heterojunction Solar Cells" – Stanford University; Richard H. Bube

The objective of this project is the investigation of heterojunction solar cells based on several II–VI systems suitable for large-scale terrestrial utilization. Specific goals include the preparation and characterization of the CdTe-CdS, ZnTe-ZnSe, CdTe-ZnSe and ZnTe-CdS systems. The Bi_xS_y-CdS system will also be studied. The difficulties encountered with the Cu_2S-CdS cell, while presently under active investigation, suggest that new photovoltaic materials, keeping the positive advantages of the Cu_2S-CdS system, while avoiding the stability and degradation problems, are needed. This project is directed towards the problem of preparing and testing several II-VI compound photovoltaic materials having the promise of low-cost, long-lived solar arrays for terrestrial applications.

(7) "Studies of Surface Structure and Electronic Properties of Polycrystalline Photovoltaic Materials and Devices" – The University of California; Gabor A. Somorjai

The objective of this project is to establish the relationship between the electronic properties and the surface structure and in-depth composition of thin film polycrystalline photovoltaic devices. The motivation is to develop devices having high conversion efficiencies and low-cost potential. Specific goals of this project include: (1) to study the morphology of thin polycrystalline films of various materials and to correlate this with electronic properties, (2) to study the correlation between surface structure, junction region, composition changes with thickness and electronic properties of currently available single-crystal silicon solar cells, of polycrystalline silicon solar cells, and of polycrystalline CdS/Cu_2S solar cells, and (3) to evaluate how changes in the film deposition parameters influences device performance through the effect on the surface structure.

(8) "An Improved Schottky Barrier Photovoltaic Diode for Solar Energy Conversion" – Rutgers University; Wayne A. Anderson

The objective of this project is to develop a more efficient and cheaper photovoltaic device using Schottky Barrier Diode (SBD) principles. This project includes calculations to determine the optical properties and to select thicknesses of various metal coatings on semiconductor substrates for proposed designs of SBD solar cells; the testing of metal films (e.g., Au-Cr) for optical and electrical properties; the fabrication of solar cells using evaporation and sputtering techniques; and testing to evaluate the efficiency of the resulting SBD solar cells. Preliminary calculations and experiments indicate that SBD principles can improve the efficiency of a solar cell by increasing the fraction of photons that optically reach the active volume and by increasing the usable photon energy range for generating free carriers in the metal or semiconductor films.

(9) "Assessment of Photovoltaic Conversion of Solar Energy for Terrestrial Applications" – Jet Propulsion Laboratory; Ralph Lutwack

The objective of this project is to provide a detailed technical assessment of the photovoltaic conversion of solar energy for terrestrial applications. Recommendations will be made concerning research and development programs necessary to develop the full potential of this solar energy conversion technology. These recommendations will contain task objectives, milestones, program phasing, implementation approach and required levels of support. A workshop on photovoltaic energy conversion will be organized to provide a sound basis for this assessment. A report on the conclusions of this project will be prepared for widespread dissemination.

(10) "Photochemical Conversion of Solar Energy" – Boston University; Norman W. Lichtin

This grant is for the identification and characterization of inorganic photoredox systems which can be used in solar-powered photogalvanic cells or for the photo formation of fuels. Fundamental research concerned with photochemical reactions of coordinating complexes of transition metals will be performed in the chemistry department of Boston University. Applied research concerned with the investigation of devices which employ the photochemical processes studied at Boston University will be performed at Corporation Research Energy Conversion Unit of Exxon Research and Engineering Co. The overall goal is for the construction and demonstration of a photogalvanic cell which has 5% engineering efficiency, i.e., converts at least 5% of the energy of the solar flux at ground level into electrical power. There is anticipated an achievement of 25% quantum efficiency of photogeneration of useful fuel by photo-redox reactions of homogeneous inorganic aqueous solutions.

(11) "Research on Cadmium Stannate Selective Optical Films for Solar Energy Applications" – American Cyanamid Company; G. Haacke

The objective of this research project is to develop a transparent, electrically conductive material, cadmium stannate (Cd_2-SnO_4), for incorporation into CdS solar cells and solar heat collectors. The research will seek to develop technology for the preparation of crystalline Cd_2SnO_4 films and optimize the electrical and optical properties of these films for energy conversion applications. Optical data on Cd_2SnO_4 films will be evaluated for use as coatings for flat plate collector covers. When the desired optical properties are achieved, flat plate collectors will be assembled and tested to determine heat collection efficiency. Cd_2SnO_4 films on transparent substrates will be used for the fabrication of thin film CdS solar cells and the photovoltaic properties of these cells will be evaluated. A feasibility study of low-cost methods for the production of large area Cd_2SnO_4 coatings will be conducted.

AGENDA

NATIONAL SCIENCE FOUNDATION/JET PROPULSION LABORATORY PHOTOVOLTAIC CONVERSION OF SOLAR ENERGY FOR TERRESTRIAL APPLICATIONS

Cherry Hill Lodge
Cherry Hill, N. J.
October 23-25, 1973

Tuesday Morning, October 23

8:30-9:00 Introductory Remarks

9:00-11:35 Session I—Single Crystal Silicon
Session Chairman—J. V. Goldsmith, Jet Propulsion Laboratory

9:00- 9:10	E. Ralph, Heliotek
9:15- 9:25	J. Lindmayer, Solarex Corp.
9:30- 9:40	C. Currin, Dow Corning Corp.
9:45- 9:55	T. Surek, Harvard University (Grant)
10:00-10:10	A. Mlavsky, Tyco Laboratories (Grant)
10:15-10:30	Coffee
10:30-10:40	G. Schwuttke, IBM
10:45-10:50	R. Fiandt, Centralab
10:55-11:00	E. Rittner, COMSAT
11:05-11:15	R. Riel, Westinghouse Research Laboratories
11:20-11:30	D. Bernatowicz, NASA Lewis Research Center
11:35-11:45	A. Blum, Washington University

11:50-12:35 Session II—Polycrystalline Silicon
Session Chairman—F. H. Morse, University of Maryland/NSF

11:50-12:00	P. Fang, Boston College (Grant)
12:05-12:10	M. Nowak, Northeastern University
12:15-12:25	T. Chu, SMU-Texas Instruments (Grant)
12:30-12:35	N. Laegreid, Battelle-Northwest

12:40-2:00 pm Lunch

Tuesday Afternoon, October 23

Session II (continued)

2:00- 2:05	W. Berry, University of Notre Dame
2:10- 2:15	A. Terrill, Auburn University
2:20- 2:30	P. Iles, Centralab

	2:35- 2:45	L. Crossman, Dow Corning Corp.
	2:50- 3:00	B. Seraphin, University of Arizona

3:05-6:15 Session III—Systems and Diagnostics
Session Chairman—H. R. Blieden, NSF—RANN

	3:05- 3:15	M. Nicolet, California Institute of Technology
	3:20- 3:30	W. Siekhaus, University of California—Berkeley (Grant)
	3:35- 3:50	Coffee
	3:50- 4:00	J. Morabito, Bell Telephone Laboratories
	4:05- 4:10	I. Greenfield, University of Delaware
	4:15- 4:25	C. Backus, Arizona State University (Grant)
	4:30- 4:40	C. Bishop, Boeing Co.
	4:45- 4:55	F. Eldridge, Mitre Corp.
	5:00- 5:10	M. Watson, Aerospace Corp.
	5:15- 5:25	M. Wolf, University of Pennsylvania
	5:30- 5:40	H. Siegel, Grumman Aerospace Corp.
	5:45- 5:55	A. Forestieri, NASA Lewis Research Center
	6:00- 6:10	P. Goldsmith, TRW Systems

6:30-8:00 Social Hour

Wednesday Morning, October 24

8:30-9:45 Session IV—CdS/Cu_2S Thin Film Cells
Session Chairman—R. J. Stirn, Jet Propulsion Laboratory

	8:30- 8:50	K. Böer, University of Delaware (Grant)
	8:55- 9:05	P. Brody, Westinghouse
	9:10- 9:20	H. Brandhorst, NASA Lewis Research Center
	9:25- 9:30	L. Kazmerski, University of Maine
	9:35- 9:40	J. Jordan, D. H. Baldwin Co.

9:45-3:40 Session V—Other Materials and Devices
Session Chairman—R. Lutwack, Jet Propulsion Laboratory

	9:45- 9:55	A. Fahrenbruch, Stanford University (Grant)
	10:00-10:10	J. Loferski, Brown University (Grant)
	10:15-10:30	Coffee
	10:30-10:40	F. Wald, Tyco Laboratories
	10:45-10:55	W. Anderson, Rutgers University (Grant)
	11:00-11:05	S. Li, University of Florida
	11:10-11:20	B. Mattes, Stanford University
	11:25-11:35	J. Berkowitz, A. D. Little, Inc.
	11:40-11:50	H. Hovel, IBM Watson Research Center
	11:55-12:00	P. Reucroft, University of Kentucky
	12:05-12:15	J. Eckert, ESSO Research and Engineering Co.
	12:20-12:25	A. Adler, New England Institute

12:30-2:00 Lunch

Wednesday Afternoon October 24

Session V (continued)

2:00- 2:05	W. Anderson, Ohio State University
2:10- 2:20	S. Chiang, University of Pittsburg
2:25- 2:30	W. Granneman, University of New Mexico
2:35- 2:40	F. Chernow, University of Colorado
2:45- 2:50	I. Melngailis, Lincoln Laboratory
2:55- 3:00	D. Tchernev, University of Texas
3:05- 3:15	R. Bailey, University of Florida
3:20- 3:25	P. Rahilly, Air Force APL
3:30- 3:35	G. Haacke, American Cyanimid Co. (Grant)
3:40- 4:00	Coffee
4:00- 5:30	Working Group Sessions

5:30-7:00 Dinner

7:00- ? Working Groups continuing

Thursday Morning October 25

8:30-11:10 Working Group Resumes and Discussions
Session Chairman—J. V. Goldsmith, Jet Propulsion Laboratory

8:30- 8:55	(1)	Single Crystal Silicon, P. Rappaport
8:55- 9:20	(2)	Polycrystalline Silicon, T. L. Chu
9:20- 9:45	(3)	CdS/Cu_2S Thin Film Cells, K. Böer
9:45-10:05	(4)	Other Materials and Devices, J. Loferski
10:05-10:20		Coffee
10:20-10:45	(5)	Insolation, Standards, and Diagnostics, H. Brandhorst
10:45-11:00	(6)	Systems, C. Backus

11:15-12:45 Panel I—Industrial Aspects of Large Scale Photovoltaic Utilization
Panel Chairman—W. R. Cherry, NASA/GSFC

	(1)	R. Fiandt, Centralab
	(2)	J. W. Yerkes, Heliotek
	(3)	A. Lesk, Motorola Inc.
	(4)	P. Rappaport, RCA Laboratories
	(5)	W. Reed, Monsanto Corp.
	(6)	C. Currin, Dow Corning Corp.
	(7)	J. Jordan, D. H. Baldwin Co.
	(8)	G. Wiener, Westinghouse Research Laboratories
	(9)	I. Seddon, Optical Coatings Laboratories, Inc.
12:00-12:45		Discussion

12:45-2:00 Lunch

Thursday Afternoon October 25

2:00-3:30		Panel II—User Requirements for Photovoltaic Systems Panel Chairman—F. H. Morse, University of Maryland/NSF
	(1)	O. Gildersleeve, Philadelphia Electric Co.
	(2)	M. Lotker, N. E. Utilities
	(3)	T. Schneider, Public Service Electric and Gas
	(4)	J. Werth, Electric Storage Battery Technology
	(5)	H. Pfeiffer, Pennsylvania Power and Light Co.
	(6)	L. Lomer, U. S. Coast Guard
2:40- 3:30		Discussion
3:30- 3:45		Coffee
3:45- 4:45		NSF/RANN Plan for National Photovoltaic Conversion Program
4:45- 5:00		Workshop Adjourned

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PHOTOVOLTAIC CONVERSION OF SOLAR ENERGY FOR TERRESTRIAL APPLICATIONS

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October 23-25, 1973

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