

## The PSR Story

by Donald Rapp  
Jet Propulsion Laboratory  
Pasadena, CA 91009

Composite sandwich structures are used throughout the aerospace industry. Such a structure has two relatively thin parallel "facesheets", separated by a lightweight honeycomb core. The facesheets are made from a "layup" of multiple layers of fiber-reinforced resin, cured as a unit to form a hard multi-layer composite sheet. The fibers are oriented in appropriate directions in different plies so that the in-plane properties of a facesheet are almost isotropic. Such a sandwich structure can be either flat, or curved, depending on the mold or "tool" against which the flexible fiber-reinforced "prepregs" are pressed during curing.

While such composite sandwich structures are usually used in situations requiring high strength-to-weight or high stiffness-to-weight, these types of structures may also be applied in situations requiring high dimensional stability. Dornier and MAN in West Germany pioneered in the development of these structures for use as ultra-lightweight mirrors in telescopes starting in the early 1980's.

Over the same time period in the early to mid-1980's, a proposed mission was under study at JPL and other NASA Centers to develop and deploy a large (~10 to 20 m dia) sub-mm wavelength telescope in space, which was called the "Large Deployable Reflector" (LDR)<sup>1</sup>. Meetings were held every two years at Asilomar, California to update progress made on structures, materials, detectors, cryogenics, and any other elements necessary for such a telescope. Two leading JPL participants in LDR planning had an inspiration that composite sandwich technology might provide the means of building the LDR primary mirror system within acceptable weight and cost. This idea of developing composite sandwich technology for the LDR required very significant advances in the technology, both in manufactured surface precision, and in thermal stability.

At this point in the mid-1980's, the Hubble Space Telescope was not yet the "Hobbled Space Telescope", and the "Great Observatories" were a major part of NASA's plans. The LDR was battling it out with the Space Infra Red Telescope facility (SIRTF) for the right to be the fourth Great Observatory<sup>2</sup> after the Hubble Space Telescope (HST), the Gamma Ray Observatory (GRO) and the Advanced X-Ray Astronomical Facility (AXAF).<sup>3</sup> In the 1980 National Ten Year

---

<sup>1</sup> At one time in the mid-1980's, the LDR was one of the highest priority proposed missions in NASA plans. As the years went by, and the priority of LDR decreased sharply, it became known as the "Large Deplorable Reflector". In fact, it even appeared as such in an AIAA publication, but only as a result of a typo.

<sup>2</sup> It is noteworthy that the LDR was referred to jokingly as the "fifth of the four great observatories".

<sup>3</sup> After around 1984, the LDR was transferred to JPL as a JPL-led mission, while SIRTF was led by Ames Research Center (ARC). In the time frame from 1984 to 1987, JPL and Caltech personnel wrote learned treatises to show that LDR could compete effectively with SIRTF, to the consternation of the ARC-NASA Headquarters bloc that was pushing SIRTF. A few years later, when SIRTF was also transferred to JPL as a JPL-led mission, and support for LDR was on the wane, JPL and Caltech personnel wrote learned treatises on the worth and value of SIRTF and mostly ignored LDR. LDR was replaced on the NASA mission

Plan for Astronomy<sup>4</sup> the LDR was one of the highest priority planned missions. The main impediment to the LDR appeared to be the weight and expense of the primary reflector. Nevertheless, in the 1985-7 time frame, LDR was still considered to be viable, and the development of a lightweight reflector for the LDR was considered to be an important technology development. These were the early days when the Applications Codes of NASA were just beginning to talk to Code "R", the advanced technology arm of NASA, and focussed technology development tasks were beginning to come into vogue. Code "Z" (astrophysics) gave Code "R" the advocacy to develop a lightweight composite sandwich reflector system for LDR and other related missions. NASA, under pressure from many sides to increase its support for advanced technology, originated the Civilian Space Technology Initiative (CSTI) which placed particular emphasis on "focussed technology" leading to mission enabling results. Code "R", hearing of the recent progress made by JPL and its contractor in composite sandwich technology, decided to fund a major technology development task in CSTI which was to be called the Precision Segmented Reflectors (PSR) Task. This task would be funded at \$20M, spread as four equal installments of \$5M per year over 4 years. Approximately 1/3 of the funding was mandated to go to LaRC and 2/3 to JPL.<sup>5</sup> JPL would manage the task. The task began at the start of FY 1988 in October, 1987. It appeared to take NASA less time and effort to come to the decision to fund this \$ 20 M Task, than it usually takes to decide to fund a \$ 100 K task. I am not aware that there was a serious appraisal made of the chances of success, the veracity of cost predictions, and even the innate probability that the job could be done at any cost. In a situation like this, where the sellers of the program were not experts in the technology they were selling, and the buyers didn't have a clue as to whether the Task was feasible, the decisions were based more on the personalities involved, and NASA's need to put something into the CSTI even though Code R was understaffed and overextended.

The Mechanical Systems Division (MSD) at JPL would play a major role in this Task. The MSD includes JPL's capabilities in structural design and analysis, materials technology, and thermal analysis and control. However, the MSD was not a happy family. The Materials Technology Section and the Structures Section had a longstanding contest for the upper hand in such activities.<sup>6</sup> It was finally decided that the PSR Task would address both the support truss on

---

plan books by a small (2 to 3 m dia) precursor called SMILS, SME, SMIM and other things at various times, mostly containing words such as "intermediate", "moderate" or other such unassuming adjectives. JPL enthusiastically endorsed these unassuming replacements for LDR, even though the Asilomar meetings repeated emphasized the need for a large sub-mm telescope.

<sup>4</sup> Astronomy and Astrophysics for the 1980's, National Academy Press, 1983.

<sup>5</sup> This required division of funds was purely political and had nothing whatever to do with the work breakdown structure. Indeed, the work breakdown structure was made to fit this political subdivision.

<sup>6</sup> The Structures Section asserted that a composite mirror was basically a structure, even though it was made of materials, and therefore the materials part of the work was subsidiary to the structures work. The Materials Section, on the other hand, insisted that it was the materials issues that determined whether the mirrors would meet the requirements, and the structural issues, while important, were subordinate. (In this regard, I am reminded of an oft-seen beer commercial on TV in which alternate groups argue whether the principal virtue of the beer is that it tastes better, or whether it is less filling). In the end, the official position of the PSR Task was that a mirror is a structure, and that materials issues were just one part of the structural design. This viewpoint was sold based on a famous (or infamous, depending on one's point of view) viewgraph, which has been used in

which the composite sandwich mirrors would be mounted, as well as the mirrors themselves. LaRC was primarily responsible for the support structure, while JPL was responsible for mirror development. JPL was also responsible for integration of the system, and testing of concepts in various test beds that were planned. JPL was expressly forbidden from working on "advanced materials", which was relegated to LaRC<sup>7</sup>, but JPL was allowed to work on upgrades to existing materials under the euphemism: "alternate materials".

Ambitious plans were laid out for two major testbeds, with a rapid development of spherical panels, and a transition to parabolic mirror panels in about 3 years. The person chosen as manager of the PSR task, while a senior person with good general experience, knew essentially nothing about composites and composite structures. The person chosen to lead development of composite sandwich panels was a senior structural engineer with a fine record, but he was not a composites expert. This is a hallmark of JPL technology tasks; "good managers" are put in charge of advanced technology tasks, when they are not only lacking in expertise in the technology, but are actually non-conversant in the technology. This seems to work out all right when everyone is in agreement, but when disagreements and controversies arise, as they are wont to do in advanced technology, such a Task Manager is at a disadvantage in making determinations. When the Task Manager does not understand the technology he is managing, his decision on who to back in a controversy must depend on the force of the personalities involved, and not on the technical merit of the arguments. It is therefore not surprising that Task Managers try to avoid controversies by setting up authoritarian, hierarchical task management structures, as well as by avoiding or excommunicating troublesome people such as Division Technologists.<sup>8</sup> The ambitious plans in the early stages of the PSR Task were based on several factors. One was the fact that no one involved had the technical strength to really appraise the chances of accomplishing any given set of objectives within the budgets allocated. Another was the fact that in order to "sell" the Task, very ambitious goals had to be set. In the atmosphere of the first year of the CSTI, there was an expansionary, visionary atmosphere in technology which had not been seen in the NASA community for many years. Finally, no one ever accused JPL of pessimism about their chances on a technology task.

It should also be noted that despite the ambitious program initially defined by the PSR Task, one potentially critical area was neglected. There is a major juncture in designing space telescopes at the point where the diameter of the primary reflector plus thermal shielding is equal to the diameter of the launch vehicle shroud. Smaller telescopes can be completely assembled on the ground, whereas larger telescopes must either be assembled or deployed in space from smaller or folded-up components. Any plan to develop large telescopes must seriously address the problem of assembling or deploying in space.

Large telescopes are needed because the angular resolution of a telescope at any wavelength  $\lambda$ , is proportional to the ratio  $\lambda/D$ , where  $D$  is the diameter of the primary. Telescopes operating in the infrared or the sub-mm require successively larger diameters to achieve angular resolutions comparable to those achieved in the visible. At a wavelength of 100 microns, one needs a diameter 200 times larger in diameter than a telescope in the visible, to achieve the same angular resolution. Some day, large, cold telescopes in space will achieve angular resolutions

---

at least 100 presentations, showing an interactive structural, materials, thermal and optical approach to the development of composite mirrors.

<sup>7</sup> I have searched my records in detail, and I can find no indication of any value added whatsoever under "advanced materials" by LaRC, despite the expenditure of about \$ 1.5M during the 4 year PSR Task.

<sup>8</sup> Technology task managers may not always understand the technologies they manage, but they are not stupid people.

near 1 arc-sec in the IR and sub-mm, with very high sensitivity. However, the PSR Task has not added much to our understanding of assembling or deploying precision structures in space.

Because of the project mentality which is so pervasive at JPL, the PSR Task was set up like a project with requirements, milestones and deliverables. However, when one carries out a research and development program, it is never clear how far one can push the technology into uncharted territory. At no time did the PSR Task simply say they would push the technology as far as it can go. Instead, very specific deliverables were defined even though these deliverables involved unprecedented advances in composite development. Part of the job of selling to NASA is to be overly optimistic and utterly convincing. The price paid for this was a set of initial requirements and deliverables that had to be continually descoped during the four year PSR Task.

As we have seen, the very act of setting requirements on an R & D task raises questions because one cannot know in advance how far the technology can be developed. However, the situation in PSR was even more confused because the project(s) that might use PSR technology were not clearly defined, and therefore it was difficult to set firm requirements. While it was generally agreed that the initial efforts under PSR would be addressed to the needs of sub-mm observatories such as LDR, there was a desire to keep the technology as generic as possible, "in case LDR goes away". In fact, the phrase "in case LDR goes away" was used numerous times during 1987-9 by the PSR Task management. However, in a series of memos, I pointed out that in the sub-mm area, LDR was the only significant game in town, although several small precursors were also proposed. It didn't make sense to talk about LDR going away, because then significant sub-mm astronomy would go away, and one couldn't hope to apply PSR technology to shorter wavelengths until one can first satisfy the less demanding requirements for sub-mm wavelengths. The logical conundrum is that one had to address PSR to LDR because if PSR couldn't satisfy the needs of LDR (or at least its precursors) then it wasn't good for anything! Yet the Program Manager for PSR at NASA HQ (ref. 9) is quoted as saying in early 1988<sup>9</sup> that PSR should not be too closely tied to LDR "because LDR is only one example (and not necessarily a very representative one) of the observatories which would use precision panels". In my memo (ref. 9), I pointed out that I was not aware of the other mysterious observatories that could use PSR panels. This same NASA Manager went on to say: "... we should push forward to materials with high radiation stability, high thermal stability and higher precision surfaces (which are of little concern to LDR)". As I said in my memo (ref. 9) high radiation stability and high thermal stability are of major concern to LDR, and the PSR Task will be hard put to achieve the precision surfaces required for sub-mm missions, let alone for shorter wavelengths. It is clear that in the early days of PSR, unbridled optimism prevailed, and the Task was loaded with great expectations for sub-micron mirrors deriving from the PSR Task. Aside from the problem of the relation between PSR and LDR, there were problems defining exactly what LDR was. A very important aspect of LDR was the orbit in which it would be deployed. In the early planning for LDR dating to around 1980, it was assumed that the LDR would be placed in earth orbit at a moderate height of perhaps 900 km. NASA had boldly proclaimed that they would develop an infrastructure around the Space Station which would include the capability for periodic refill of the liquid He cryostat on-orbit, and replacement of instruments when needed. By 1987, it had long since become clear to me that this was just another NASA fantasy, and that the growth in costs would make such a scenario much too expensive to afford. In ref. 1 I said:

---

<sup>9</sup> When the hubris of PSR was flying at its peak, before grim reality had set in.

"The tendency has been to assume that we can rely on periodic refilling of stored liquid helium cryogen .... Such operations are likely to be costly, unwieldy and downright risky and dangerous. I cannot abide by the statement that we should "treat stored cryogen as a viable option" [in low earth orbit].

In ref. 8, I reiterated this position, and also pointed out that at the Asilomar III meeting, when confronted with a short cryostat lifetime, the members of the sensors panel simply drastically reduced their "official" estimate of instrument heat loads, based on "gut-feel". I also pointed out that the NASA system of placing cryogenics technology in the organization as a subordinate part of sensors technology, only assured that NASA would continue working on sensors and pretty much ignore cryogenics. By early 1989, the SIRTf pre-project had been moved from Ames Research Center to JPL, and a study showed that by placing SIRTf in high earth orbit (~ 100,000 km) the lifetime of the cryostat could be extended, and the viewing efficiency greatly increased, so that a viable mission was possible without periodic refill of cryogen, which was appearing less and less feasible with each passing month. In March, 1989, (refs. 10,11) I suggested that the only viable way to plan LDR was to use an orbit such as the one chosen for SIRTf, with its reduced heat load from the earth, and higher viewing efficiency. Nevertheless, in June, 1989, the official LDR mission plan was still for low orbit. In June, 1989, I said (ref. 16):

"It seems clear that operating altitude would be either  $\approx 900$  km or  $\approx 100,000$  km. In low Earth orbit, it seems unlikely to be much lower than 900 km, in order to reduce drag effects, the effects of space debris, and to improve viewing and reduce thermal problems. Nevertheless, the 100,000 km orbit is much more desirable because there is an insoluble cryogenic problem at 900 km (unless you believe that tankers could transport 15,000 liters of liquid helium to the telescope every few years), and for lots of other reasons (viewing, thermal stability, communications, etc.). The main problem with the high orbit is the limitation on mass that can be transported to this altitude. Whichever orbit proves to be selected, if the telescope is erected at 250 km, it must be dragged to higher orbit as a fully constructed unit. Prospects for an orbital transfer vehicle to be developed are very uncertain. Furthermore, it is not clear whether the assembled telescope could stand the loads, and whether it would be contaminated by its stay in low orbit.

There have been a number of fantasies that have been repeated so often that many people in the NASA community have taken them for truth. These fantasies are variants of a brave new world in which there are all sorts of manned activities in low Earth orbit which revolve around a Space Station. Astronauts would float around and assemble structures in space, and orbital maneuvering vehicles would transport telescopes to their operating altitudes. Tankers would deliver huge cargos of cryogenic fluids to telescope facilities.

While this millenium may some day occur, it does not seem to be in the cards for the period 1998-2005 in which we are interested. In the meanwhile, we must face certain constraints in defining an approach for the substructures of large segmented telescopes. First of all, there is a limitation due to available launch vehicles. We are not only limited in mass and lifting energy, but also by the upper limit of about 4 meters diameter in launch vehicles, which limits the size of structural elements which can be carried into space."

This message was repeated in refs. 17 and 19,

"Although the official party line on LDR has been (and apparently still is) that it will be erected by astronauts in low Earth orbit, and replenished periodically (every 2-3 years) with cryogen by cryotanker, I find this hard to believe because (1) I think the capabilities of astronauts on orbit have been overestimated, and (2) the cost of each cryogenic refill (if it even becomes technically feasible) is likely to run many hundreds of millions of dollars, resulting in a project lifetime cost of billions. In my opinion, Code EZ will never buy off on this.

Therefore, I think that LDR must be carried out in high Earth orbit. This has major implications for the structure, and has significant implications for the goals of the PSR Program. For example, the panels will operate at a significantly lower temperature than 200 K, and they will be subject to less thermal variation once on orbit. They will not be exposed to atomic oxygen for long periods of time. We must also conjecture a procedure for building the LDR and taking it to orbit, or taking it to orbit and deploying it. At present, there is no credible scenario for the LDR that I can discern."

As recently as 1991, LDR planners were still clinging to low earth orbit, but weakening. Today, planning for LDR (which is fairly dead) and its precursor is apparently based on an elongated elliptical orbit, which provides the benefits of reduced earth heat load and high viewing efficiency, but which requires less propulsion to put a given mass in orbit. The importance of the orbit to the PSR Task is severalfold. In low orbit, the expected reflector temperature is considerably warmer than in high orbit, there is considerably less radiation, and there is a potential O-atom erosion problem. Some materials for reflectors will work well in one orbit, but not the other. In the early days of the PSR Task, the indication from LDR was for low orbit, so the PSR Task was aimed at materials for temperatures of 200 K and a weak radiation environment. A couple of years later, the sub-mm missions finally succumbed to a high orbit, and now the old materials were not appropriate because of the colder temperatures (~ 100 K) and high radiation environment.

I am convinced that the reason why planners of sub-mm missions such as LDR continued to officially plan on a low earth orbit is because of the way NASA's bureaucracy works. It was clear to the LDR planners as early as 1986 that low earth orbit was not a viable scenario for LDR, but they refused to acknowledge this obvious fact because of the NASA's Soviet Union-type autocracy. As long as NASA clung to the fiction and fantasy of the Space Station Infrastructure with tanker spacecraft flying around and refilling cryostats, with periodic change out of instruments in astronomical facilities, JPL had to "go along with the gag" and pretend along with NASA, for if JPL assumed otherwise, it would imply doubt of NASA's official position. In addition, there were real doubts about the ability of launch vehicles to lift enough mass to high orbit, and LDR could have been "caught between a rock and a hard place" in which low orbit did not lead to a viable mission, and there wasn't enough propulsive power to put the LDR in high orbit. There is very intense competition for precedence in the process of selecting future missions, and it is necessary to "keep a happy face" and resolutely avoid any doubts, risks, or uncertainties in a planned mission. As Division Technologist, I was interested in abstract truth; however the advocates for a new mission are mainly motivated to present a good case for their mission.

Somehow, with LDR's wavering baseline, PSR's determination not to tie too closely to LDR, and the need to set firm requirements in a project-like atmosphere, the PSR task began in October 1987. The requirements and objectives of the Task remained fairly constant until the late summer of 1989, when some massive changes were made in both the direction of the PSR Task, as well as in its management. The most recent rendition of the original objectives and

requirements was a presentation to the LDR Science Coordination Group in June, 1989 (ref. 15). In this presentation, the basic outlines of the PSR Task were given. The 4-year, \$ 20 M Task was divided into four pieces:

- Composite panels (\$ 8 M)
- Precision support structures (\$ 4.5 M)
- Figure controls (\$4 M)
- Technology demonstration test bed (\$ 3.5 M)

No mention was made of funding for the materials tasks, but as I recall, these budgets came out of the composite panel subtask. Although the plan called for the combination of 0.9 m parabolic mirror panels, a second generation 4 m support truss, and a 1 micron figure control system into a 5 micron parabolic test bed, this test bed would not be assembled until the end of the fourth year of the Task, and therefore no work would be done on it during the normal tenure of the PSR Task. The plan for panel development had the milestones (ref. 15):

- Sept. 1989: 0.9 m, 3 micron RMS spherical panels
- Sept. 1990: 0.9 m, 3 micron RMS parabolic panels
- Sept. 1991: 0.9 m, 1 micron RMS panel (shape not specified)

The required thermal stability of the panels was stated as a maximum change in RMS figure error (relative to the room temperature figure) of 3 microns for a 100 °C reduction in temperature from room temperature. This was an overly conservative requirement, because what really matters is the RMS deviation of the mirror panel surfaces from the best-fit telescope surface at the working temperature, not the RMS deviation from the best-fit telescope surface at room temperature. When a mirror panel is cooled from room temperature to a cryogenic temperature, the change in figure can be represented as a change in focal length plus changes in other aberration terms (such as astigmatism and coma). While there is not much one can do about the astigmatism and coma, one can compensate for the change in focal length by merely moving the position of the secondary mirror relative to the primary mirror. Thus, the change in focal length from room temperature to operating temperature is not in itself a problem, unless the various panels change focal length by different amounts. Since the largest effect of cryogenic cooling on mirrors made in the early part of the PSR Task was a change in focus, the requirements should have been made in terms of the consistency of focal lengths of panels at cryogenic temperature, not the change from room temperature.<sup>10</sup>

It is clear that two years into the PSR Task, the PSR managers apparently still believed that they could meet the panel requirements listed above (ref. 15). This included the manufacture of 3 micron parabolic panels by the end of FY 1990. Looking backward from the vantage of the present, it is clear that the PSR managers were vastly overoptimistic. For example, just setting up a mirror test facility for parabolic panels would have cost well over \$ 1M. As it turns out, after 5 years of PSR, and the expenditure of over \$ 23M, no parabolic panels have yet been made.

---

<sup>10</sup> The requirements continued to be given in terms of the required maximum change in RMS for panels cooled from room temperature until late 1991 when they were finally changed to a requirement in terms of RMS deviations from the best-fit surface at operating temperature. This pointed out the need for the ability to duplicate panels with the same focal length, a topic that had been avoided for the first 3 1/2 years of the PSR Task.

In the period 1988-1989, the PSR Task began to slowly confront the reality that its goals were too ambitious for its budget. At the same time, its organizational structure was too loose and uncoordinated. One area which seemed to be very poorly integrated was materials development. From the start, a fundamental stumbling block was the political need to pay homage to the fact that Langley Research Center (LaRC) is the officially designated Lead Center for NASA work on structural materials.<sup>11</sup> Therefore, a separate sub-task of PSR was created called "advanced materials" and this was assigned to LaRC. In order to assuage JPL, a much smaller task, "alternate materials" was given to JPL. Supposedly, JPL was only mandated to work on modifications and improvements of existing materials, whereas, LaRC was supposed to look at more far-reaching exotic materials. Although I searched assiduously, I had difficulty finding any value added to the Task by the LaRC "advanced materials" subtask. In ref. 2 (June, 1988), I said:

"The work on advanced materials at Langley seemed vague and diffuse. There was no evidence of any progress since PSR started, and there was a lack of clear direction. The LaRC group did not seem to be familiar with the up-to-date requirements, and did not seem to be in communication with JPL materials people or the PSR Task Manager."

In ref. 9 (February, 1989), I said:

"Finally, I want to say that --- is right on the target in his last paragraph (¶6). JPL needs to find a way to get LaRC to be more productive and successful in the advanced materials arena, because if they are attacked by OAST, we will bear some blame and feel repercussions. We'd be much better off trying to make them more successful at what they are supposed to do. ---'s plan to open more communication with LaRC is a very good first step."

In ref. 16, (June 7, 1989) I said:

"The materials work is apparently divided into two separate efforts, one at LaRC and the other at JPL. I am not sure how much funding is going to LaRC for materials research, but I suspect it is of the order of \$ 2M. I have yet to see anything useful come from this effort. As I understand it, they spent the whole first year pursuing a composite matrix material advertised by Shell Inc as having mysteriously good structural rigidity, which turned out to be a very average material. I know that there are delicate political

---

<sup>11</sup> The Lead Center concept was created to try to avoid duplication of capabilities in advanced technology between NASA Centers. The idea is that by setting up one Center with the best facilities, and an exclusive budget, this Center would have the capability to solve all the problems and supply all the technology needs of all the Centers for their future projects. In actual fact, it is widely practiced, though rarely acknowledged or admitted, that the designated Lead Centers tend to follow their own directions, ignoring the needs of other Centers, and therefore, each of the non-lead Centers has set up moderate-size capabilities which do duplicate those of the Lead Center, in order to get someone to work on problems of importance to the non-lead Centers. These small, often subterranean efforts at non-lead Centers are extremely important for many planned projects. In 1991-2 NASA has indicated that they intend to tighten up the Lead Center concept and transfer technology development going on at non-lead Centers back to the Lead Centers. This will be a disastrous mistake, and is based on a preconceived view of Lead Center effectiveness, ignoring the actual facts.

problems in dealing with another Center, but I still think we must insist on performance in the work we manage."

I also raised the question as to whether any of the advanced materials would ever be used in a mirror panel, provision for which did not seem to be made in the PSR Task. These issues were brought up again in refs. 17 and 19, but there was never any response or any indication that anything was being done to alleviate the problems. For example I said:

"A major problem for PSR is that much of the new technology created during the course of the program, such as composite cores for panels, and advanced materials, will not be incorporated into the demonstration structures. Indeed, many of the new materials innovations will not even be incorporated into panels. As a result, the demos will demonstrate where we were after about a year of PSR, while subsequent development will not be incorporated into our systems."

Even today, in 1992, I fail to recall any value added by the advanced materials subtask carried out by LaRC.

Another aspect of the work done in the 1988-1989 era is the almost total lack of documentation and the lack of revelation of technical details. It was not clear what materials the mirror panels were made from or what chemicals were used to separate the panels from the tool, and the manufacturing process was not described anywhere. In ref. 5, I pointed out that there were many reports of activity, but very few reports of content or results. In ref. 13, I said:

"Finally, I want to close with a statement on archiving the recipes for panel preparation. Admittedly, the situation is complicated by the participation of --- Corp. which has put a great deal of its own resources into panel development. We do not want to infringe on their proprietary interests. But if we go through the PSR task and there is no archived corporate knowledge of how to produce panels, except in the heads of one or two people, I think we are in trouble. People are finite and frangible. Corporations are forever. Building and testing panels will do NASA no good if we don't catalog the procedure for manufacturing and testing them. If it takes funding to do this, PSR should provide the funds, even if it results in cuts to other tasks.

In ref. 16, I said:

"One [question] is how we plan to go about archiving the information on how to produce panels. We seem to have a great deal of emphasis on delivering a panel or panels with a certain performance, but do we intend to catalog the prescription for how to produce such a panel? Do we have a clear statement of specifically how each panel was produced? Present plans for archiving appear to me to be grossly inadequate."

In ref. 19 I said:

"Finally, there is limited value in producing and demonstrating panels if there is no archive which preserves in detail how the panels were produced. If the know-how on making panels resides in a couple of heads, it is rather volatile and can easily be lost. One of the things that concerns me is that the milestones of the panels are described in terms of surface accuracy (10 microns, 3 microns, etc.) but the differences in technology of these panels remains mysterious."

In actual fact, the task remained almost completely undocumented until late 1991, when I was asked to write a four-year report summarizing the work done.

Perhaps the most worrisome feature of the PSR task in the 1988-1989 era was the gap between what was intended and the available budget. Although JPL had originally requested \$ 46 M for the PSR task, NASA only provided \$ 20 M. Instead of scaling back to lesser goals, PSR tended to bull ahead, hoping for "over-guideline" supplements. In ref. 19 I said:

"The PSR Task keeps saying that it only has a mere \$ 20 M, and therefore it can't do realistic prototypes on such a mere pittance. If it had the full \$ 46 M that it originally asked for, then it could do parabolic panels and deployable structures, etc. But I say that all proposals get scaled back, and one must then select limited goals which allow you to make significant progress in a few areas rather than vague and ephemeral forays into too many areas. There never was any hope of getting \$ 46 M. That is a fantasy. To me, \$ 20 M for PSR is a once-in-a-decade opportunity, and we should make the most of it."

In ref. 13, I said:

"When PSR first started, there was great uncertainty about the technical and financial requirements to do the various tasks. This was not the result of any lack of preparation, but merely a reflection of the fact that we were entering uncharted territory from a low level baseline. As is usual in NASA programs, we fashioned a very ambitious program, which at best, would severely strain the budgets that were available. As we got into the program, we found inevitably that technical difficulties and costs were greater than we had originally hoped, and it became increasingly difficult to stay on the original track of planned accomplishments. This is, after all, an R & D program, and it is not possible to specify in advance what will be encountered.

There are two orthogonal philosophies which could be taken in this context:

1. Continue to work on all tasks as planned, and accept a certain amount of shortfall in each area compared to the original plan.
2. Cut back significantly in some areas to provide additional resources to critical areas so that these critical areas will meet irreducible minimum levels of accomplishment.

And, of course, there is a continuum of possibilities in between these extrema.

I have gotten the impression that the PSR Program is being run on the first philosophy, and using the shortfalls to argue to Code RM for extension and expansion of the funding. This could work if we are successful in getting the additions. However, the fact that we may not do anything very definitive in four years for \$ 20 M could turn NASA off and might leave us with a large number of unfinished accomplishments.

I personally believe that it would be better to define a hierarchy of prioritized goals which would guide us in carrying through a limited subset of activities to a reasonable state of completion, even if it means cutting way back on some, or even stopping work on a few. This would, hopefully, guarantee that when all was said and done, we would have at least one substantially accomplished goal at the end of four years. Equally important is the need to show sufficient progress in one or a few areas at this stage of the game (FY

1989) that NASA will be encouraged to follow through with funding for the planned four years. Far be it from expanding PSR, we could lose it if we don't produce in the interim years. It may be important to demonstrate advances in panel capability at the end of FY 1989 in order to keep PSR going.

In my opinion, the highest priority goal should be the capability of producing panels."

Although I did receive one letter of support for this viewpoint which said: (ref. 14)

1 June 1989

To: D. Rapp

From: S. Synnott

Subject: Your PSR Memo of May 17

Although I wasn't on the distribution list I did get it indirectly and I wanted to respond to it even though it is none of my business.

I think your discussion that the program has to pick some things to finish and stick to them is ABSOLUTELY DEAD BANG ON the right approach....

RAPP FOR PSR PRESIDENT

as you will see if you read further, not only wasn't I made "PSR PRESIDENT", but about 7 months later I couldn't get elected PSR DOG CATCHER.

It was becoming generally apparent in the summer of 1989 that the PSR Task was fraught with difficulties, particularly trying to accomplish too much on too many fronts, and not being successful enough in any of them. At the same time, changes in programmatic priorities had occurred, leaving LDR in limbo, and only a small precursor to LDR seemed feasible, technically and financially. Such a small precursor required mirrors with a shorter focal length, a lower operating temperature, and resistance to a strong radiation environment. It was finally decided to replace the management of the PSR Task and to adopt a new set of requirements.

The degree of optimism in the PSR goals of the 1988-1989 era can be seen by comparing the viewgraphs used by the PSR Deputy Manager in a presentation made June 9, 1989 (ref. 14), with the viewgraphs made by the replacement manager of PSR on February 21, 1990 (ref. 25). The goals of the task had diminished considerably over that 8 month period, although a number of fantasies remained in the February 1990 presentation (i.e. it called for an LDR Phase A start in 1995 and a Phase C start in 1999, it claimed that 1 m 2 micron rms mirrors were already an accomplished fact, and it set up deliverables for 1990, 1991 and 1992 that were never met). Nevertheless, it did have a ring of sanity about it because it said: "the task was more challenging than originally anticipated" and that "PSR will not complete all the technology development for LDR and SMMM".

After about 2 1/2 years of the 4-year PSR Task, the PSR management began to make plans for a follow-on task. Two of the important sub-task managers (for composite mirrors and for "alternate materials") approached me for help in defining a rationale for such a follow-on activity. My response was given in a memo to them dated January 12, 1990 (ref. 23). This led

to some interesting consequences. In order to avoid any misunderstanding, I am reproducing this memo in its entirety below:

Jet Propulsion Laboratory

Interoffice Memo  
January 12, 1990

To: RF/DC

From: D. Rapp

Subject: Rationale for Follow-On to PSR

You have both indicated to me that the PSR Program is trying to define its destiny in terms of missions that it can enable, to thereby create a rationale for a follow-on program to the initial 4 year program which was planned to extend from 1988 to 1991. Although I am not connected with the PSR Program in any direct way, you have asked my opinion, and I do have some thoughts on the matter which I would like to share with you.

From the beginning, I think that there have been some misconceptions in Code RM regarding the number of potential missions, and the level of effort needed in PSR to enable them. Furthermore, there was a deep rooted antipathy by Code RM toward developing technology which enables essentially only one future project, and thereby providing the appearance of being a pre-project phase of that mission. These viewpoints were imposed upon the JPL PSR Program, and led to certain conflicts between what the PSR Program claimed it was doing and what it could actually accomplish. I think that the PSR Program Manager was caught with one foot on the dock of reality while the other foot was on the PSR ship of fantasy which was steadily pulling away from the wharf. While many flights of fantasy took place in 1988-89, I can perhaps summarize these in two points:

- Imagining the existence of planned or proposed missions that do not exist.
- Assuming that a \$ 5M/yr program for four years with about 25% allocated to reflector panels, could decisively demonstrate the capability to build large precision telescopes in space.

Now, in 1990, with the prospect of justifying why the PSR Program should be extended beyond 1991, these ghosts that have haunted us in the past, still remain with us. Whether it will be possible to create a logically consistent rationale without getting Code RM to face reality, remains to be seen.

At present, we seem to be limited in the size of precision reflectors to the diameter of launch vehicles - something in the range 3 to 4 meters, although even here there are questions of mass and operating temperature. In order to go to larger sizes, a credible scenario needs to be created for (1) fabricating the elements of such a structure, (2) deploying or erecting the full structure in space, and (3) transporting the structure to its operating orbit. There has been considerably fantasy in this arena about the capabilities of astronauts to perform operations in space, and about the capability to revisit such facilities and replenish cryogen and service instruments.

The PSR Program is mainly concerned with the first of these three parts of the overall scenario, namely the creation of elements of the structure, although it is also somewhat concerned with assembly of the structure in space. Nevertheless, the prime concern is to be able to provide assurance that one can fabricate the elements out of which such a structure would be created in space. Clearly, the development of technology to build large segmented reflectors in space will increase sharply in difficulty as one pushes to higher surface precision, wider excursions of operating temperature, longer lifetimes, and larger sizes. There is a natural hierarchy in the PSR Program in which one manifestly ought to start at longer wavelengths and gradually work down to shorter wavelengths - or to put it more accurately, to gradually improve surface figure precision. Fortunately, there are two very important application areas at longer wavelengths which are critically dependent on PSR technology:

- Sub-mm astronomy
- Optical communications

Sub-mm astronomy requires a large reflector (preferably 20 m dia) with a surface figure precision of a micron or two. Specific program plans have been embodied in terms of the proposed LDR and its small precursors SMME and SMILS. Perhaps we have placed too much emphasis on the specific plans for an LDR. Code RM has worried about "what will happen if LDR goes away"? But I think that this is a funny attitude. Even if the specifically planned LDR does "go away", sub-mm astronomy isn't going to go away. The sub-mm region of the spectrum is a critically important region in which the peak radiation from cool matter (3 to 30 K) in the universe (about 90%) radiates. More importantly, there is a plethora of molecular lines emitted in this region which provide a means to identify molecular species in molecular clouds and star forming regions. These sources are relatively weak and require large collecting areas. Furthermore, because the angular resolution of a telescope is inversely proportional to its wavelength, a submm telescope must be scaled at tens of meters to have a reasonable angular precision. A sub-mm telescope of this dimension, whether it be the LDR, or anything else "if LDR goes away", will perforce be an expensive proposition (clearly at least \$1 B). Is it not unreasonable to spend ~1/10 of this amount to develop and demonstrate the critical technology to enable this mission?

The need for optical communications has not been emphasized much in JPL's optical communications programs, because at this stage of development, JPL is more concerned (as it properly should) with the telescope transmitter system on the spacecraft than it is with the large receiver back at Earth. Nevertheless, the plans laid out by the JPL optical communications group call for eventual deployment of a receiver in Earth orbit which includes a ~ 10 m dia reflector with a surface accuracy of 1-2 microns - very similar to the needs for LDR. It is also worth mentioning that many people believe that optical communications is an enabling technology for Mars Rover Sample Return and other Mars missions as part of the Human Exploration Initiative.

Having these two moderate term (~10 years) critical mission needs for its technology gives the PSR Program a strong underpinning and rationale that is rarely matched in the annals of Code R technology development programs. The development of the capability to create 10 to 20 m dia reflectors in space with 1-2 micron surface accuracy, which can function for many years in their operating environment, would be an important achievement for NASA. In my opinion, this, in itself, is a reasonable justification for the

extension of the PSR Program. However, in extending the PSR Program, it is necessary to stick with our present goals and carry them out to completion, and not get carried away by flights of fancy to dabble in minor forays into very precise panels. We should continue to concentrate on reflectors with 1-2 micron surface figure precision and carry the technology convincingly to the point where the needs of sub-mm astronomy and optical communications are satisfied. The scale of funding that would be appropriate would be some fraction of the cost of a sub-mm telescope and an optical communications receiver system in Earth orbit. I would estimate the total cost of the two applications as over \$ 2B.

Eventually, the follow-on to PSR will push to shorter wavelengths (and, consequently, higher surface precision). An obvious application area for the second stage of PSR is extrasolar planet detection. It seems pretty clear that the optimal wavelength for direct detection of extra-solar planets is in the range 10-15 microns which would imply a surface accuracy of  $\sim 0.1$  micron according to the usual rule of thumb ( $l/100$ ). Furthermore, as I have shown in my report on Extra Solar Planet Detection (JPL D-6835), control of scattering due to figure errors over 1 to 3 meters is of considerable importance in detecting extrasolar planets because the stellar flux is so much greater than the planet flux. Therefore, one may have to either use mirrors with surface precision of ( $l/1000$ ), or use the method advocated by Diner in his IBIS Program of a very capable deformable secondary to even out figure errors in the primary. There are two approaches under study at JPL for extra-solar planet detection in this wavelength range. In one, D. Diner's IBIS Program, Diner and his team propose a large segmented filled aperture of 10 to 20 m diameter, with a deformable secondary, and a sophisticated post processing procedure for the focussed beam to discriminate against the star and thereby reduce the stellar background in the region of the planet image. Shao, on the other hand, is thinking about an interferometer in this wavelength range. I am not sure, but I suspect that he is thinking in terms of a baseline of perhaps 20 m, and individual siderostats of perhaps 2 m dia. Both the Shao and Diner systems would be passively cooled in space. Shao would clearly use small diameter reflectors but would require high surface accuracy. Diner would use a larger reflector but would handle some of the figure error with a deformable secondary. Both of these concepts are still in early conceptual stages and require considerably more study before requirements can be firmed up.

In moments of exhilaration, some people have talked about extending PSR technology down into the IR and eventually, the visible. If we first think about the IR, say from perhaps 4 to 100 microns (i.e. roughly the range of SIRTf), we need to grapple with some other issues. The SIRTf was designed as a small (0.85 m dia), cold (optics at  $\sim 7$  K, detectors at 1.4 K) telescope. Such a telescope reduces the telescope background to such small values that even with the comparatively small reflector, reasonable signal-to-noise is achieved for many targets in moderate integration times. More importantly, it allows SIRTf to observe regions with very low surface brightness. However, the angular resolution of SIRTf is not very high, being some 2.5 arc-sec at 10 microns. Thus the SIRTf is best used for extended sources rather than point sources. The comparatively small diameter of the SIRTf becomes a limitation for measurements at very high spectral resolution where the spectral selection in the observing instrument discriminates against the telescope background, and thereby makes high resolution spectroscopy independent of telescope temperature. Cooling the SIRTf has the most effect for low spectral resolution measurements. Furthermore, the background in the SIRTf is limited by the zodiacal light and therefore would not profit from further cooling. In fact, the only way to improve the sensitivity of the SIRTf (above the zodiacal background) is to increase its

diameter. Such a larger diameter SIRTf would have better angular resolution and sensitivity. Since the entire SIRTf is enclosed within a superfluid He cryostat, it will be difficult (and expensive) to increase the diameter of such a telescope.

The question now comes up as to whether a passively cooled segmented reflector telescope could compete with SIRTf technology in the 4-100 micron range. Helou and Beichman have carried out some studies of the performance of a passively cooled telescope compared to a cryogenically cooled telescope (JPL Report D-4580, 1987). They modelled three conceptual LDR-like configurations which are passively cooled to 200 K. Their LDR-A had a surface accuracy of 2 microns, and LDR-B had a surface accuracy of 0.5 microns. (They also modelled an LDR-C which employed chopping. This introduced extra noise in the background of LDR-C). Their models indicate that a 200 K LDR-B is much better than a 200 K LDR-A in this wavelength range, and is actually competitive with SIRTf for point sources. I suspect, without doing any calculations, that an LDR-like telescope, passively cooled not to 200 K, but closer to 100 K, and with a surface precision not of 0.5 microns but say 0.1 to 0.2 microns, might have significant advantages over the SIRTf for certain applications (high spectral resolution, high angular resolution). There are astronomical targets for such a telescope, such as star forming regions, and new galaxies. I think that someone should go back to the Helou-Beichmann model and estimate the performance of an LDR-like telescope with diameters from 8 to 20 m, surface precision from 0.1 to 1 micron, and temperatures of 50, 80, 100, 125 and 150 K. It would not be a large calculational effort. If I were you, I'd suggest to DL that he fund this calculation. This would help us decide whether it makes sense to extend the PSR technology into the IR to compete with SIRTf.

As far as the visible region of the spectrum is concerned, I think that fundamentally different approaches are called for. I doubt that the PSR Program should get into this muddy water at this time.

In this memo, I pointed out what everyone really knew anyway, that the PSR Task had been oversold in what it could accomplish, and that attempts to push PSR technology to shorter wavelengths were premature and based on fantasy. I did provide a number of potential future applications which could justify future PSR-type work, including optical communications and long wavelength astronomy, and I advocated that we concentrate on doing a good job on initial goals before adopting new, more ambitious goals. In short, a follow-on task would be needed to accomplish the initial goals set by the PSR Task, which were excessively ambitious for the funding available.<sup>12</sup> Apparently, this memo rubbed the PSR Task Manager the wrong way. Shortly after this memo was sent out, the PSR Management Oversight Committee (consisting exclusively of people who did not know much about composites technology) met, and according to the description given to me, the PSR Task Manager at that meeting stood up, pounded his fist down on the table as hard as he could half a dozen times, while his face turned red and purple, and he shouted at the top of his lungs while waving my memo in the air that he "wouldn't take it any more" and that I had to be stopped from writing these things. This position was avidly supported by the manager of the Observational Systems Division. As a result of this, the manager and the deputy manager of my Division at JPL led me to believe that my

---

<sup>12</sup> Looking back on this memo from the vantage point of September, 1992, I see no reason to change one word of the memo, and I think it was accurate, correct, and appropriate. I think the words of SS (ref. 14), "I think your discussion that the program has to pick some things to finish and stick to them is ABSOLUTELY DEAD BANG RIGHT ON the right approach" are very appropriate here.

job was in jeopardy. I was pulled off an important planning team for the Space IR Telescope facility (SIRTF) which had recently been transferred from Ames to JPL, and I was made *persona non grata* in the Division office. I asked my Deputy Division Manager whether he believed that there was anything in my memo that was factually incorrect.<sup>13</sup> He responded by telling me that this question was irrelevant, and that the only thing that mattered was what the PSR Task Manager thought. My job was to seek forgiveness from the PSR Task manager. Fearing for my job, I finally succumbed, and on January 23, 1990, I wrote the memo shown below (ref. 22) to the PSR Task Manager:

"I believe that you might have seriously misinterpreted the meaning of my memo of January 12 on "Rationale for Follow-On to PSR". What I said in the opening part of the memo was that the original expectations of NASA for this program were excessively high, causing the original Manager of PSR, in his efforts to comply with HQ attitudes, to develop a gap between what is realizable and what was planned. I suggested that one of our present challenges is to get HQ to understand more realistically what can actually be achieved in a technology development program such as a followon to PSR. I then went on to suggest some constructive technical approaches for providing the rationale for a PSR follow-on.

This memo was not about you. It does not directly refer to you. It is not critical of you. On the contrary, I have the highest regard for the job you have done. I expressed this in my note to you of December 5, a copy of which is attached. I know that you are a very busy person with a demanding job, and I have no desire to cause you any grief. If I had known how you would react, I never would have included that material in my memo. I am really sorry that you have reacted this way to my memo, and I want to assure you that I had no insidious intentions; I was really trying to make some constructive suggestions."

The PSR Task Manager never acknowledged receipt of this memo, nor did we ever communicate again. It should be pointed out that this memo admitted nothing wrong in the original memo, but only pointed out that he may have misinterpreted the memo, thinking that certain remarks were directed at him (which they were not). But the PSR Task Manager was a military man, educated at Annapolis, and he was used to a system where people stood up straight and saluted, never questioning orders or intentions. However, my viewpoint is closer to the quote:

"Chronic use of the military metaphor leads people repeatedly to overlook a different kind of organization, one that values improvisation rather than forecasting, dwells on opportunities rather than defends past actions, values arguments more highly than serenity and encourages doubt and contradiction rather than belief."<sup>14</sup>

Similarly, the Augustine Committee, after studying NASA and its needs, concluded:

---

<sup>13</sup> Of course, neither my Division Manager, nor his deputy, nor the PSR Task Manager had the proper technical background to decide whether the contents of my memo were factually correct. They were concerned with other issues, such as appearances, keeping up a good face for the PSR Task, and supporting the PSR Manager.

<sup>14</sup> Someone once sent me this quotation, but I cannot remember who wrote it.

"NASA needs inquisitive, penetrating and challenging people - people who ... Are continually questioning and ferreting out anomalies to be placed in full view of all involved"

In fairness to Division management<sup>15</sup>, these people are overcommitted and stretched by a very wide span of management concerns. They don't have time and energy to pursue issues arising out of technology tasks such as the PSR Task, and depend on Division personnel, including the Division Technologist to make things work. The PSR Task Manager referred to above had recently replaced his predecessor, whose management had not been regarded as effective, and Division management was under considerable pressure to back this new PSR Task Manager and give him support in whatever he did. The problem, of course, is that you can replace the PSR Task manager as many times as you want with people who are not experts in the technologies they manage, and you will still have the same old problems.

There were several outgrowths of this event. One was that the single person in the Division who was a generalist and most technically competent in the diverse system issues of telescopes, astronomy, materials, structures, and thermal engineering was essentially removed from the two most important development activities in the Division requiring these skills: SIRTf and PSR. I essentially dropped out of sight in regard to PSR for most of 1990, and I was excluded from many important activities involving SIRTf planning. Another was that I now knew that I could not expect backing from my own Division Office in regard to any controversies that might arise in the business of the Division, and therefore I stopped trying to actively represent Division personnel outside the Division.

From January, 1990 onward, I was in PSR Siberia. The PSR Task, heavily endowed in precision, thermally stable composite materials, representing one of the most important technologies in my Division, was not accessible to the Division Technologist. PSR activities went on, many reviews were held, and glowing reports of progress were given to NASA Headquarters. I contented myself with other studies, leading to a set of reports:

1. "Active Structures for use in Precision Control of Large Optical Systems", with J. L. Fanson and E. H. Anderson, *Optical Engineering*, 29(11), 1320-1327 (1990).

---

<sup>15</sup> The Division Manager position at JPL is a very difficult one. He heads an organization of over 600 people who work on an incredibly broad range of topics. The Division Manager is continually given special assignments such as heading up review boards, and must also deal with a thousand non-technical issues such as affirmative action, United Way subscriptions, car pooling, and safety and back problems. The real seat of power in the Division is the Sections (subelements of the Division). Section Managers hire and fire, and bring business in. The Division Manager is like a King trying to consolidate and unify a bunch of unruly Barons, Dukes, Earls and other sub-royalty. The Division Manager is also like a fireman, always putting out fires. He spends almost all his time on big fires in large projects and doesn't have time to deal with small eruptions in technology tasks. In the technology area, the Division Manager only wants to hear good news and that all is well. At JPL, there is a system called "quiet hours" where staff members meet with their managers once a month and discuss anything of concern to the staff member. I used to go to these quiet hours and recite litanies of what was wrong in technology, particularly PSR. Looking backward, I think my Division Manager dreaded those quiet hours and popped aspirin to get through them. After the great interdict on my PSR activities, I learned my lesson, and for a whole year I reported only good news at quiet hours.

2. Space Interferometry and Large Optics Program Prospectus, JPL Report D-6854, September, 1989.
3. "Direct Detection of Extra-Solar Planets", JPL Report D-6835, October, 1989.
4. "Direct and Indirect Detection of Extrasolar Planets and Brown Dwarfs", JPL Report, April, 1990.
5. "Binary Stars", JPL Report D-8028, January, 1991.
6. "Effect of Telescope Temperature and Surface Figure Accuracy on Performance of Conceptual IR and sub-mm Telescopes", JPL Report D-8175, January, 1991.
7. "Infrared Astronomy in the Post-SIRTF Era", JPL Report D-8482, May, 1991.
8. "The Dimensional Stability of Materials", JPL Report D-7667, July, 1990
9. ""Laminate Theory for Orthotropic Materials", JPL Report D-7747, August, 1990
10. "Laminates Used in the Hubble Space Telescope", JPL Report D-7781, September, 1990.
11. "Potential for Active Structures Technology to Enable Lightweight Passively Cooled IR Telescopes", JPL Report D-9449, March, 1992.

I tried to avoid confronting the PSR Mafia, and sought productive activities in other directions. However, I could not fully avoid PSR, in my position as Division Technologist. An internal review of Division 35 PSR activities was scheduled for August 1, 1990. Two weeks before that review, I was asked what I thought should be covered in the review<sup>16</sup>. We were now 33 months into the Task, and almost no details had ever been released on the panel development work. Dozens of glossy viewgraph presentations had been given during that period, but almost no technical detail. Documentation of the panel work simply did not exist.<sup>17</sup> I made the recommendations given in the following memo (ref. 27):

In response to your request, I think the review should do the following:

- (1) Specify which fibers and resins have been used in the panels, and why they were chosen.

---

<sup>16</sup> I had been trying to find out for years what unique approaches the PSR Team had brought to precision composites and gotten no answers. I had tentatively come to the conclusion that the only thing unique about the JPL PSR effort was that we had the audacity to try it.

<sup>17</sup> In fairness to those doing the panel development, the endless series of reviews, the huge pile of correspondence required to answer findings of reviews, the changing requirements, and the general atmosphere of continual oversight by august committees made the participants paranoid of presenting any real data because that would only provide ammunition for further attack. Furthermore, most of the reviewers didn't know anything about composites anyway!

(2) Specify which fiber volume percentages were used in the panels and why they were chosen.

(3) Specify which laminate layups (angles and number of plies) were used in the panels and why they were chosen.

In both (1), (2) and (3), a clear statement should be made as to how our selections relate to selections made by other institutions in their work reported in the literature, and what aspects of our selections are unique.

(4) Specify which tests have been conducted to measure the CTE, thermal conductivity and other properties of materials used in the panels. Report the data.

(5) Present the specific details of the "HAVOC" modelling procedure, including the assumptions made, the databases used, and the predictive powers of the program.

(6) Explain the effects of using an AI core with composite panels, and the effect of using the same radius of curvature (ROC) of front and back panels.

(7) Explain why it is acceptable to have large changes in ROC of panels on cool-down. Explain how this effect will affect non-spherical panel performance.

In actual fact almost none of this information was divulged at the review. Although I was invited to attend the review, I was not invited to be a review board member. I did, however, generate a memo on August 20, 1990 in response to some of the findings of the review, which violated what I thought were basic technical principles of composites (ref. 30). I pointed out that although one of the reviewers recommended against developing resins with a lower cure temperature than 350 °F, there were great potential benefits from using resins with a lower cure temperature.<sup>18</sup> I also raised the issue that the PSR Task was using materials with large coefficients of thermal expansion (CTE) when materials with low CTE were readily available. This led to a memo to me in November, 1990 from four participants in the PSR task defending their choice of materials. My answer to them, written November 28, 1990, concluded (ref. 31):

"... your consistent and unbending defense of this clearly inappropriate material reduces your own technical credibility and makes you appear less concerned with truth than with turf."

This was followed by yet one more memo from a member of the PSR Team to me in January, 1991, which I responded to on January 22, 1991 (ref. 32).<sup>19</sup> My memo began with the paragraph:

"I would like to begin by saying in all sincerity that I appreciated receiving your detailed and considered response which provides a thorough, intelligent viewpoint, even though there still remain rather large honest differences of viewpoint and opinion between us. In

---

<sup>18</sup> A year and a half later, under new management, the PSR Task embarked on a major effort to acquire cyanate resins with a 250°F cure temperature to replace resins with 350°F cure, in order to reduce aberrations and residual stress. These resins were used in panels made near the end of FY 1992.

<sup>19</sup> The Subject of my memo was "My reponse to your response to my response to Division 35's response to their response to PSR's request for a Division review."

past cases where I have raised issues regarding PSR technology, I have often met with quasi-astrophysical responses ranging from supernova (emotional outbursts) to black hole syndromes (excommunication). I think that your memo presents a reasoned rationale for why our panel program uses the particular materials that have been chosen."

My memo also contained the paragraph:

"I am astounded and shocked by the statements 'It was the contractor that would determine the material and not the panel team', and 'Thus the choice of baseline material was based mostly on the contractor's choice and not on P75/934 vs. C6000/F155 technical issues'. I have always (perhaps naively) believed that when JPL contracts out technology development work, it should be because an outside institution has better facilities, workforce and experience to produce the desired result, but JPL should always be smarter, more knowledgeable and more understanding of the technical issues than the contractor, so that we can effectively judge the worth of the contractor's approach, and evaluate his performance. While we might leave it to a contractor to actually fabricate panels, it seems to me that we would be derelict in our duty if we let him choose the materials without a thorough in-house review and approval of the choice of materials. Such an in-house review should have included 'P75/934 vs. C6000/F155 technical issues'. I get the impression that JPL was acting as a broker to place money with a contractor who knew more than JPL, instead of JPL being a technically informed organization that found a contractor to fabricate panels according to our models and concepts."

This series of memos was carefully contrived by all parties to be in response to the Division review, so that the PSR Task Manager (at that time), a man who divides the world into "team players" and "loose cannons", did not receive copies. If he had, I have little doubt that he would have tried to get me fired. Based on past experience, I have every reason to fear that my Division management would have supported him (rather than me) in such an issue. I managed to keep a low PSR profile for the next six months, and thus stayed out of trouble.

Just as in the post-World War II Soviet Union, where certain individuals rose to prominence and power and then faded, seemingly for no reason, various missions and mission directions ebb and flow in the NASA bureaucracy. Roughly every ten years, the astronomy community of the United States writes a ten year plan for astronomy in the U. S., including both space and terrestrial facilities. In 1980, the "Field report" listed the LDR as one of the higher priorities for the 1980's. In late 1987, when the PSR Task began, the LDR was still considered to be an important potential project, and indeed, the LDR was the principal programmatic "demand pull" for PSR technology. However, the LDR concept was built on a house of cards. The need to put a very large telescope into a high earth orbit required methods for assembly on orbit, and with the demise of the fantasied Space Station infrastructure of astronauts assembling antennas and reflectors on-orbit, and tanker spacecraft delivering thousands of liters of liquid helium to cryostats, made the LDR appear much more remote in the late 1980's than in the early 1980's. As the early years of the PSR Task struggled with developing structural elements to meet mission requirements, the technological viability of LDR, always uncertain, appeared even more risky. In the late 1980's, the SIRTf pre-project was transferred to JPL, and since SIRTf appeared more technologically ready, and had higher political priority, support for LDR waned. A watered down sub-mm mission to replace LDR began to gain favor, with a small reflector (2 to 3 m instead of 20 m) and a two year life (instead of 10 years). Sub-mm astronomers were getting older, and began to formulate a strategy of getting something into space, no matter how

reduced in scope from the LDR, before they retired. With the revelation of the Hubble Space Telescope woes, the prospects for a multi-billion dollar blockbuster telescope appeared to be squashed, and LDR was jettisoned. A series of small sub-mm missions were advocated, all with the words "moderate", "intermediate" or something like that to show that these missions fit the new NASA philosophy. In the 1989-1990 time frame, LDR had fallen so far in priority, that the requirements of the PSR task were totally rewritten to fit the shorter focal length, lower temperature, and higher radiation environment of the small missions in high orbit. By 1991, when the Bahcall Committee wrote the ten-year report for astronomy in the 1990's, the LDR was not even mentioned, and does not appear in the index!

In August, 1992 I wrote the memo (ref. 38):

There are many important astronomical targets in the IR and sub-mm parts of the spectrum, such as star formation, distant galaxies, and extra-solar planet imaging. The need to avoid atmospheric absorption and scattering, and the desire to cryogenically cool the optics are motivations for deploying such telescopes in space.

I have a vision of a future long-term NASA Program in which successively larger and more capable sub-mm and IR telescopes are developed and deployed in space. I would like to see NASA develop the technology base for such a program in the 1990's, and use this base to launch a series of telescopes with increasing capability in the first 15 years of the 21st century.

In my vision, we would adopt a single basic configuration approach for a series of telescopes, involving mirror segments mounted on a zero-CTE composite truss support frame, with passive shielding of the telescope from the sun. For IR telescopes that require low temperatures (~20 K) a relatively sophisticated thermal shielding system will be required. For sub-mm telescopes which can operate above 100 K, a simpler thermal shielding system will be adequate. The mirror segments for sub-mm applications can probably be made from composites, but may require some active optics to maintain the required figure in the space environment. For IR applications, it is not clear what materials will be feasible to achieve the required higher figure precision, and composites may not be adequate. A fully active mirror system will probably be required. But regardless of which materials or degrees of active optics is used, we would try to design these telescope in a modular fashion, to use as much inheritance from one to the next as possible.

A break point occurs when the telescope (plus shielding) diameter exceeds the diameter of the launch vehicle shroud. Smaller telescopes can be fully assembled on the ground. Larger telescopes must be assembled or deployed in space. The ability to set up large precision structures in space must be developed within the context of a realistic scenario for astronaut and robotic capability in low earth orbit, and not according to excessively optimistic scenarios such as were common a few years ago. I think everyone will agree that early missions should involve telescopes small enough to be fully assembled on the ground. But I would argue that these should be modular to enhance the transfer of this technology to larger designs which will require construction or deployment in space. We should not lose sight of the need for large telescopes, even in times such as these when all missions seem to be "moderate" or "intermediate". The angular resolution of a 0.7 m telescope at a wavelength of 100 microns is 36 arc-sec, which is very poor by almost any standard.

In order to develop such a family of space telescopes, a significant technology development program will be required. This program will be expensive. The development of large reflector systems which can function at cryogenic temperatures will be particularly expensive. Sensor and receiver development will also be expensive. Other cryogenic technology will also be required. Ground testing of prototype telescopes and components at cryogenic temperatures is a complex, expensive process, requiring expensive facilities. It is my observation that in the past, we have grossly underestimated these costs, and that we have ended up working piecemeal on small parts of the whole problem, with no clear picture of the entire system, and what it requires. For example, at each stage of the PSR Task, we have had to scale down our expectations, and scale up our costs. Meanwhile, we continue to operate under a system where an occasional task is funded in detectors or reflectors, but there is not much in the way of a system program for getting from where we are today to a full-up technology capability to mount a mission by some date. I think that there is an undercurrent of belief that if we really told NASA what the full required technology program will cost, even our presently funded bits and pieces might disappear. I believe that if NASA really wants to have the capability to deploy 3-4 m passively cooled telescopes by the year 2000, it needs to spend about \$ 100 M between now and then in a concerted, unified, non-fragmented focussed technology program. In order to further develop the capability to deploy 8-15 m telescopes by say, 2008, another \$ 100 M will probably be required. These are, of course, just guesses, and I might be on the low side.

As JPL tasks on passively cooled IR and sub-mm telescope technology gradually run out and fade away, the position of SIRTf becomes more important. It appears that if there is any practical hope of doing significant IR astronomy from space in the foreseeable future, it lies in SIRTf. Never mind that SIRTf is still a small telescope with poor angular resolution. Never mind that it will be difficult to scale SIRTf up to larger sizes without excessive cost increases. SIRTf is still a very good IR telescope for some applications, and would add considerably to our knowledge in the field of IR astronomy. I don't blame anyone for endorsing SIRTf. In order to have a career, one needs results to analyze before one retires. But I am concerned that IR and sub-mm astronomy need larger telescopes, and that SIRTf technology will be difficult to scale up. If we are lucky, and we get the go-ahead to build SIRTf, where will that leave us in the quest for large telescopes?

People back at NASA HQ need to understand that one of the great potential exploitations of space is the ability to do IR and sub-mm astronomy above the atmosphere with large, cold telescopes. The impact of results from such a program on our knowledge of the universe will be significant. But we can't get there from here with a bunch of small fragmented technology tasks, which don't add up to development of full system capability. While I strongly endorse and subscribe to the activities at JPL to bring down the cost and size of planetary missions, I also think it is important to realize that in astronomy, there is a conspiracy of nature which makes it necessary to build large systems (which inevitably are expensive). The challenge to lower costs then becomes one of designing systems in a modular fashion, with maximum inheritance as one scales up in size and down in wavelength.

However, JPL is very protective of its stake in SIRTf. There has been a movement to develop a concept for a passively cooled IR telescope called "EDISON", seeking European and NASA support, which would be larger than SIRTf, but not as cold. The group involved in EDISON

approached me for technical support and participation, but I was warned<sup>20</sup> by a prominent IR astronomer in the Caltech/JPL community that open support of EDISON by me would meet with very severe political repercussions.

In the summer of 1991, the manager of the PSR Task moved on to bigger and better things. A new Task Manager was appointed. The PSR Task had been in existence for 3 1/2 years, and the panel development activity was in shambles. There was essentially no documentation of what had been done, why it had been done, what had been concluded, or where we were headed. There were no error budgets or sources of error in mirror panels, and technical approaches based on modelling to reduce these errors. There was only a series of Edisonian tries to make better panels based on intuitive or gut feel motivations. In fact, there wasn't even a complete list of all the panels that had been made with their materials and processing parameters. The only records that were available were published by Division 38, who carried out the optical tests on panels and reported these rather carefully and extensively. Not only had the PSR Task failed to make acceptable panels, but it didn't really have a clue as to why panels behaved the way they did, or how to make better panels. Years of misrepresentation to NASA<sup>21</sup> had confused the issues to the point where no one could really define what had been accomplished. There were many greivous misconceptions and errors in the Task. One was that there was no concept of scaling laws. Surface figure errors measured for 1/2 meter panels that were fabricated, were assumed to be indicative of what could be achieved for 1 meter panels (that were yet to be fabricated). In fact, errors like astigmatism appear to scale as the square of panel size, so the errors in a 1 meter panel would be 4 times as large as the errors in a 1/2 meter panel. Thus, the results reported in the 1990-1 time frame for 1/2 meter panels were not nearly as optimistic as the PSR Task claimed they were. A second example was the neglect of the need to not only make panels with good surface precision, but also with closely matched focal lengths. Calculations have shown that the inability to very closely match focal lengths of mirror panels would be a major show stopper for PSR applications. Yet, no pair of identical panels was ever made in the Task to examine this question. A third example is that even though the PSR Panel Subtask Manager continually showed viewgraphs depicting an interactive development program with materials, structural, optical and thermal experts working together in a coordinated effort, the fact is that these experts never met one another, never worked together, and only made separate inputs to the Panel Subtask Manager, who was the only person to be exposed to the big picture.<sup>22</sup> Many basic technical issues did not seem to be realized, or at least they were not documented. These include such things as the importance of balancing adhesive on the front and back facesheets, the tendency of adhesive to drip off the back facesheet into the core, the importance of ply orientation errors, and the effect of thicker cores on astigmatism.

One of the first things that the new PSR Task Manager did was replace the Panel Subtask Manager. In the summer of 1991, the PSR Task Manager also called me up and said that he had read most of my memos of the past few years and he agreed with most of their content! He knew that he had a major problem in getting the Panel Subtask on a sound footing and asked me if I would try to help. The deep freeze was over. I was released from the salt mines of

---

<sup>20</sup> July, 1992, private communication from ----.

<sup>21</sup> For example, by making one panel with decent as-manufactured precision but poor thermal stability, and another with decent thermal stability but poor as-manufactured precision, and reporting that panels with "good as-manufactured precision and thermal stability had been manufactured".

<sup>22</sup> This was a sort of "divide and conquer" style of management. A leading materials technologist told me that he never met the person who did modelling of mirror panels in the first 3 1/2 years of the Task.

Siberia, and welcomed back into society. The new Panel Subtask Manager asked me to join his team. He started regular team meetings, and put a stop order on making any new mirror panels until we had a clear idea of why we wanted to make the next panel, and what we hoped to learn. I could have worked forever for that man, whose viewpoint and philosophy was after my own heart. The first thing he asked me to do was to document the work of the past 4 years in a detailed report.

Many good things were done in the PSR Task in FY 1992. These include:

- The acquisition of competitors to the original contractor, both inside JPL and outside, for fabricating mirror panels, and thus developing more leverage to get the contractor to do what JPL wanted.<sup>23</sup>
- Formation of a working team of structures, materials and optics people that met once a week to discuss progress and plans.
- A strong increase in funds allocated to finite element modelling, and much stronger integration of modelling of trends in the decision tree for how to make the next mirror panel.
- An attempt to document all work done on the task (which was made difficult by the previous 4 years of culture established whereby nothing was documented).
- Greater emphasis on composite cores.
- Established a logical framework for developing a manufacturing sequence of mirror panels.

However, there still remain important things to do. For FY 1993, I recommended a program for filling in all the basic knowledge that was needed but had never been acquired (ref. 37).

As it turns out, funding for continuation of the PSR Task beyond FY 1992 was cut out of the NASA budget, although no explanation was given by NASA. I asked for reasons why this task was being terminated, but got no reply: (ref. 39)

"For the past 5 months, I have been making repeated requests for clarification of NASA's position on why they plan to discontinue funding on the PSR/TT Task. I have received essentially no feedback. I would therefore like to repeat my request.

I think that JPL is entitled to a reasonable explanation of why this program is being terminated at a point where it is still far short of being ready for application, so that it will fall into ruins and provide little net value added to NASA's programs, like so many other programs funded by Code "R".

In the spirit of TQM, surely some kind of answer is due? Or is it? Perhaps I don't understand TQM?

---

<sup>23</sup> A perennial bone of contention was that JPL's contractor wanted to preserve proprietary interests in the products they manufactured, and was therefore reluctant to reveal details that JPL needed in order to stay in charge of development. One example of this was the composite core developed by the contractor, which was referred to as "Core X" at JPL because its content and properties were not given.

As you know, NASA has spent roughly \$ 22 M on the PSR Task and its TT follow-on, with roughly 3/4 of the funds spent through JPL. Now NASA plans to discontinue any follow-on to this work in 1993. Although NASA is entirely within its rights to make such a decision, I think that we at JPL are entitled to some explanation as to why this work is being stopped.

It seems to me that there are three major possibilities. Either:

(a) NASA believes that we did such a good job that the technology is proven and they can declare the program a success. Therefore they don't see any need to continue funding.

(b) NASA believes that we did such a lousy job that there is no sense in pouring more money down this hole with the expectation of a continued poor return on investment.

(c) NASA is generally confused about what we accomplished, and about the importance of completing this technology development for programmatic reasons, and it therefore slipped through the cracks.

In our phone conversation of this morning, you indicated to me that you thought that NASA is stopping this program because of reason (a). I would like to urge you to pursue this matter with NASA HQ and make sure that we understand their position clearly.

If it is actually the case that NASA believes that we did such a good job that the technology is proven and they can declare the program a success, I would like to tell you in the most emphatic way that I believe that NASA is badly mistaken, and it is our responsibility to enlighten them on their misinformation. It is possible that the NASA view of this Task may be colored by overly optimistic reports from JPL in the 1989-1991 time frame. In this respect, we may be victims of our own hyperbole. I have very carefully studied the mirror technology being developed in the PSR/TT Task, and I believe that I can speak authoritatively as to its accomplishments and prospects. My opinion is that the Task has excellent prospects, based on new concepts and approaches developed in the past 6 to 9 months, but that these approaches are mainly unproven and yet to be demonstrated by experiment. I believe that the Task is in the very precarious position where if we drop the program now, the \$ 22 M will have mostly been wasted, and NASA's return on investment will be small. I also believe that for another ~ \$ 10 M, we have a good possibility of demonstrating the technology for precision cryogenic composite mirrors for long wavelength astronomy.

I think that you ought to forcefully bring these points to the attention of NASA and try to get them to commit to the reasons why they are discontinuing the Task. If it turns out that reason is actually (c), then we need to clarify the issues for NASA. If the reason turns out to be (b), I'll have a great deal more to say in another memo."

So here we stand today in the late summer of 1992, and PSR is zeroed out for FY 1993, LDR is so dead that it is unmentionable, and the prospects for a small sub-mm space telescope in the next decade are uncertain. JPL has pinned its IR hopes on SIRTf, which has now shrunk to 0.7 m diameter. The exploitation of space for high resolution IR and sub-mm astronomy remains well beyond the horizon, and the present momentum is downward. In many ways, the PSR Task probably was a factor in causing the premature death of the LDR, with the realization of how difficult and expensive it would be to develop the LDR structure. On the other hand, the

watering down of the Space Station infrastructure and the shock of the Hubble Space Telescope problem also contributed to a negative atmosphere for large, expensive telescope projects. One could also say that these factors reduced support for the LDR, which in turn, removed the main programmatic "demand pull" for PSR technology.

Despite the present situation, I still believe that one of the important exploitations of space will be the use of large, cold IR and sub-mm telescopes for astronomy. I hope I live long enough to see this happen.