Stephen M. Andrich, Advanced Spacecraft Technology Division, MSC, February 21, 1967.

Hacker: Perhaps you could start by telling me what part you played in the paraglider program.

Andrich: I came with NASA in November '62 and was assigned to the paraglider about a month later. And I was assigned to the tow test vehicle, which was to be the manned vehicle later in the program and would be used to evaluate the landing system from an already deployed concept. Then we had the other vehicle, which was the FSTV - full-scale test vehicle - where we would actually demonstrate the deployment. And this would be an unmanned type concept. Several months after I was on the program it was decided that the program would be reevaluated. About this same time, Mr. Mathews came aboard to take over the Gemini program, which was, in general, having scheduling problems.

Hacker: Was it Mr. Mathews who was largely involved in redirecting the paraglider program?

Andrich: Yes, primarily. Essentially what happened is that when Mathews came aboard and took over Gemini, it looked like the Gemini program, as a whole, was slipping about a year from the schedules that had been put out. Paraglider looked like it was at least in that bad a shape or worse. After getting his feet on the ground, I would say three or four or five months after he came aboard, it was decided to reevaluate the paraglider and essentially we combined two phases that were in progress. I think it was contracts 167 and 539, if I recall correctly. And a new contract was let, called a 1484 - NASA 1484 contract. With the instigation of this new phase, it was decided that, to give the paraglider more emphasis, a special projects office would be set up, reporting directly to Mathews and Mr. Bill Smith was put in charge of the special projects office - I think his initials were W. F. The paraglider group, itself, remained under Ken Hecht for, I guess you'd say, administrative duties. He had the parachute system and ejection seats and anything else of that kind. I continued to have the TTV and Fred Burns had the FSTV. Ken, still, because of his background with the paraglider up to this point, not only helped us adminstratively but technically also, because of his background. We also, at this time, even though it was decided not - at the time we were evolving the 1484 contract, we looked beyond the development stage and were attempting - this was prior to the contract, prior to letting the contract ---

Hacker: This was in early spring of '63 that you're talking about now? The contract was - the letter contract was dated May 5.

Andrich: Okay, this is probably right. We probably didn't start this until about January or February of '62. I mean of '63.

Hacker: As far as I can find out, the real decision to do something about paraglider came after March 11 when the test vehicle was lost - I believe it was a full-scale test vehicle that crashed and was destroyed.

Andrich: No, this was a half-scale.

Hacker: That's right. It was the second half-scale that had been lost and from what I have been able to tell this is what really started the - I shouldn't say that - it looks as if this was what started the ---

Andrich: This is what gave us a reevaluation of our - of the, I'd gess you'd say - the North American flight testing techniques, and led to a very strict quality control and configuration management concept within the paraglider program, which, to me, killed the paraglider program, personally, because to be in a research and development stage and be attempting to act like you're getting ready to build the next piece of flight hardware - of course, from the same standpoint, we had had a system here that lacked redundancy for protection; it was poorly managed - that may be a poor choice of words here - from the standpoint of - Not enough thought had gone into how you would evaluate your systems to get ready for a flight test. The checkout was fair, but they acted more like this was a toy, rather than a good test vehicle. The half-scale, again, was a toy, if you want to look at it from that standpoint. The systems were attempting to use as much off-the-shelf type of hardware as possible. We did very little development here, except from the standpoint of taking this off-the-shelf hardware and coming up with an electro-mechanical system to deploy the wing and to get rid of - it was more a systems development, rather than an individual component development type deal. This was - ended up being done more on a garage-type operation, rather than a good systematic operation. At least, this was the impression we got coming in when we did. During this phase, they were trying to get basic information and trying to do it as cheap as possible. It didn't pay off. This led to quite a bit of rework in the checkout procedures. Getting back to Bill Smith, right after he was given the position, he had already been

negotiating to leave NASA and he left almost as soon as the announcement came out. I guess he was here about a month or so after they made the announcement, or a little longer. I took over - I was then assigned to replace him.

Hacker: When was this - about?

Andrich: About the time we moved down to this Center. I'll have to look to find out when that was - I can't recall. As I said, the office was set up to more intensify the importance of paraglider to the contractor and to give it top management attention, in that I reported directly to Mathews. I still drew people out of Ken Hecht's group. This didn't change, it continued to work this way, but it gave the emphasis which we thought was required to get top management attention. It was done for another reason, too. Shortly before Mr. Chamberlin left, he had directed North American to upgrade their management structure within the paraglider program, and he felt that, during this period of time, because of the Apollo program being in the North American shop that we were playing second fiddle, so to speak. He felt that we were not getting the priority or the type of people necessary to develop the system. So, with this, Mr. George Jeffs, who is now chief engineer on Apollo, was assigned as program manager and he was also made a vice-president of North American. He pulled together - essentially the basic staff stayed, that was there. The man that had been more or less program manager became deputy manager, Mr. Witte, and the man that had been acting as chief engineer became assistant chief engineer and they brought in a man from North American/Columbus to be chief engineer on the program. It was beefed up this way, from a management standpoint, with people that had top management attention out there. Mr. Jeffs had been on Mr. Storms' staff out there at North American - I guess he's vice president of the North American - president of S&ID - which is the division that handles Apollo. So with this, the program got new emphasis. This was still before the 1484 contract, quite some time before in fact, about the time that I came aboard, this had been done. You've got quite a bit of this background already, I guess. In fact, you have a better feel for it than I do.

Hacker: I know the dates. What you're saying adds a great deal of significance to these moves that's hard to get out of the documents.

Andrich: About this time, we had - about November or December of '62 - was when they had their first DEI on the vehicles. And at

this time, it was decided that quite a bit of additional redesign was necessary on the vehicle, from a reliability standpoint. We, shortly after that, saw some of the results of not having this redundancy and reliability in the half-scale vehicle. So it was anticipated, although because of the flight-scheduling that had already been done on the half-scales - we did nothing really to the half-scales - because they were already at Edwards at that time undergoing the test program. You pretty well have the information, I think, on the negotiation of the 1484 contract. As I was saying here, prior to the finalization of the 1484 contract, our concept was to redirect the development effort, but also to begin the procurement of flight hardware. And it was to be done two ways. We needed flight hardware out of - a considerable amount of the paraglider-type flight hardware was to be built by McDonnell and we needed to develop various pieces of hardware from scratch, which North American would be doing, on the actual payout equipment, stuff like this, and the flight controls, themselves. Most of this - the design and development of this was done by Vickers. We requested that we be able to go into the final development phase, or at least procurement of some of this hardware and, I should say, the setting of the specifications necessary for this hardware during this period. However, at the time that the contract was let it was decided by Mr. Mathews, one, that there was not enough money available in that fiscal year to instigate this, plus he felt that we were not far enough along with the actual development to - of the concept itself - warrant spending this money toward this hardware. In a sense, part of this was done, in that we started the development of the flight control system with Vickers. This was the only part that was really done. We also received some equipment from McDonnell that would meet the final Gemini hardware design, or at least prototypes of this design. And these were redesigned into the tow test vehicle. This much was done at that time. The new direction tended to try to make the manned vehicle as close to the Gemini vehicle as possible, in order to - right off the bat - attempt to, one, get the performance characteristics of the vehicle and also so there would be a minimum transition to a training phase, if, in fact, we did develop it far enough to start training astronauts in these two test vehicles. That's what we had in mind. Am I diverging too much off the subject?

Hacker: No, not at all. One thing I would like to ask - what was the precise nature of the redirection. This is something that I haven't been able to really put my finger on. Initially paraglider was expected to be flown on all the Gemini flights. Along about here ---

Andrich: All the manned Gemini flights. It was pretty evident, early, prior to my getting here, that it looked almost impossible to make the first flight. They didn't want to make remote control with the paraglider system. This is what it would have taken in order to bring it down to a landing. It was pretty unfeasible at that time, if you want - not feasible at all from our standpoint, to develop a remote control landing system. And it was thought that the first vehicle, in order to look at the structure, primarily, on the first vehicle, and temperature gradients on the first Gemini vehicle, it was easier to go ahead and put paraglider - I mean a parachute - aboard and recover the vehicle with a water landing, using a parachute. From vehicle 2 on, though, we had planned to have a paraglider aboard. Later, it was decided that it would be vehicle 3 before you had a manned flight. Part of this, I think, was politics, in order to hold the Gemini schedule which - here again I'm talking a little bit out of line, I guess - but in order to hold the Gemini schedule the way it was, it was decided to make a second flight, unmanned, and again look at more systems, because the first vehicle, again, was a political deal. We really had planned to have more systems aboard the first flight. They were not ready at that time and that was why the second flight was also unmanned, in order to get more basic data on the vehicle. With this, with our scheduling even at the time that Mathews came aboard, it was pretty evident that probably the earliest vehicle we could get it on would be 5, and more likely it would be 7. I believe the direction was in this general direction at that time, to shoot for spacecraft 7. I may be wrong. I'm a little hazy there. You may have more information on that than I do.

Hacker: This stuff I have is just as hazy. They were talking about anything between 7 and 10, through '63, but it seems to pretty much depend on who was talking to who.

Andrich: Yes. A lot of the information to North American, even though we attempted to follow up our direction in writing, the writing became minimal - do this - but not why we had decided to do this. And for this reason, I think, there's a lot of gaps in the data that you have, in the documentation that you have. Mr. Chamberlin would maybe go into great detail with Mr. Jeffs on what he wanted. This was prior to Mathews' time and even afterwards, I think, a lot of this was done, where we emphasized what we wanted and why we wanted it, and then when we gave our direction on redirection, it was just the specific item and not why. A lot of this is lost now. A lot of it, at the time - maybe

Les Stewart could possibly fill you in on some of this. I've got a lot of Les' papers, most of his stuff is very, very early Gemini development. In fact, he had the early - he stayed on the program, I guess, until '63, from a program control standpoint, maybe it was even later than that, about '63, I guess, where he tended to phase out of it, going to other portions. But this was the one thing that I found, in trying to just read through it and get some background, that there was a lot of large gaps that had no rhyme nor reason to them. You could talk to three or four people and maybe put a story together and come up with actually why it was done, but sometimes this is pretty hard because a lot of it was personality-type stuff, where "I think my idea is better than your idea and we're going to go this way," because I know several times, after I got on the program, that you'd go and you'd ask about certain things and say, "Do it this way." And they'd say, "Well, we were going to do it that way, but Mr. Rod Rose directed us to do it this way." And unless you were really firm in your convictions, and a lot of times it was again a personality-type deal - "I would have done it this way, from my past flight-test background" - not necessarily that it was the only way, that it was the only right way. And so you'd tend to back off. Later we became more firm as we got a better footing on the thing and also began to see that, in some cases, the contractor was using our ignorance to go his way - not a lot. I think everybody's normally against somebody else's changes. It's just human, I think this was just part of it. They could see that every change we made took money out of the pot. And they only had a certain amount of money, and they could see that it was going to hurt. There were other changes that they could foresee possibly, downstream, due to problems, and also - it changed some of their philosophy when something came up, and they had to rephilosophize, if you want, on how they would get something done. I'm sure they pretty well had a flight test procedure, if not in writing, within their mind's eye on how they wanted to do something. Any time you disturb this, you throw everybody out of line. The development of the vehicles, themselves, the tow test vehicles, appeared to be extremely slow, primarily because of our problems with the control system, itself; the mechanical end of the control system was being developed by Vickers. We ran into several problems. We thought we were going to be able to take an off-the-shelf type piece of hardware which was developed primarily to be used with hydraulics. We converted it to air - to use air to spin the motor. And we ran into leakage problems, valve friction [sticktion ?] problems, and also a problem that, since this was tending toward the flight hardware, and not just

a piece of test hardware, we wanted to - and we were also attempting to man-rate this, since we were going to put a man in it right from the beginning - we had quite a development phase and we spent quite a bit more money here than we had anticipated, again, falling for the pitch that it was off-the-shelf type piece of hardware and it would be minor development. It turned out that it was quite expensive. The changes were small but - if you look at the overall design of the thing, it didn't change much - the big thing was valve controls and stuff like this that caused us quite a bit of problems. This was the mechanism that - you might say the winch - that moved the cables to control the paraglider itself. This was run by passing air through this motor, which essentially was a Vickers prop, primarily, before - the hydraulic type prop - and you drove it with an electrical motor and you pumped hydraulic fluid with this thing. It had also been used the other way, where you actually pumped hydraulics through it to move something. There was a need to develop a planetary gear system for this in order to get this thing from a high-speed piece of machinery to a relatively low speed final drive. We had some gear problems but primarily the problems were in the valving and controls. Once the vehicles were at Edwards - the vehicles were delivered to Edwards minus the flight control system, in order to start the initial checkout - then the flight control system was delivered at a later date. Going back in history again, before Mr. Chamberlin was replaced, in getting a new rundown on the capabilities of the system itself, to actually flare and land, we had a system on the vehicle that - you made an - on attempting to come in and land with it, you had a forward reel that paid out and allowed you to get a high lift just prior to touchdown, and touch down easy. It was - after studying it, and seeing the fine tuning that a pilot had to have in order to be able to, one, recognize the height he was above the ground, and two, to initiate the flare maneuver that was necessary, it looked like he had to move pretty quickly. Also we found that our wing was not meeting the L/D requirement and we were not going to get the glide ratio we thought we were going to get out of the wing. So, based on this, this was prior to Mr. Chamberlin getting off the program, again, we decided to redesign the wing from a high-lobe sail to a relatively low-lobe sail, which increased our lift and gave us a better glide ratio. In doing this, we needed to go back to the wind tunnel. I believe it was - anyway we had to go back to the wind tunnel to reevaluate the lower lobe two ways - when we lowered the lobe we lost some of our dynamic stability - lateral stability of the vehicle. It's like taking a regular airplane and cutting the tail in half, if you want the

vertical - it didn't have any lateral control, because these lobes came up like this and gave you some lateral stability. In going into the wind tunnel, and we went into the wind tunnel with a half-scale model and then with a full-scale wing, and it pretty well showed that we had the stability that we needed, at least from what we could gather from the type of testing that you can do in a wind tunnel. We also increased our lift to drag ratio to the point where we had a better performing wing from the glide standpoint. We also had a considerable amount of information, now, that we had gained through a small simulator that had been built up to look at landing concepts. It was pretty well decided that we'd be better off if we could get away from this one flare concept, where the way it was set up that on his stick he had a - on his grip he had a button that was a flare button. You hit the flare and it allowed the forward cable to pay out and then you had no recourse but to hit the ground. We decided that we could get away from this concept and let him actually land it like he would an airplane and maybe do it in steps, where he could pitch, and also come closer to the point where he wanted this. He could maybe stretch his glide, if you want to look at it this way. With this concept, we were giving up some of our capabilities and we would probably not land at zero feet per second but at a higher sink rate. It was a tradeoff that we made, looking at the total vehicle, at this time, and we were hoping through fineness in design later, as we refined the wing design itself and got some of our weight down within the wing itself that we could possibly do better in the final development. But with the wings that we had, this was a problem that we were running into. This, as you know, proved wrong in the first flight, in that we got into a stability problem - we had lowered the lobe too much - and we got into a dynamic stability problem and the pilot had to bail out and was injured. He was injured, not because he bailed out - he would have been injured either way, whether he left the airplane or not - the vehicle or not - He had decided on a smaller chute. He was a big man, weighed about 200 pounds, and he decided on a smaller, quicker-opening chute. He had some good reasons for it, because of the low altitudes that we were flying and the response rate of the man and the system and everything else; it allowed him more time to get out, which essentially would also make him closer to the ground. It would give him a chance of getting hurt. I don't think he studied this as much as he should have, personally; for a man in the 150-160 pound bracket, that chute would have been fine. And he hit pretty hard, and, of course, he may have hit wrong also, although it wasn't a very windy day, the day he

bailed out. He did injure himself.

Hacker: That was where the vehicle went into a series of uncontrolled turns. That was a dynamic instability problem?

Andrich: That's right - due to lowering the lobes on the sail. We went back late - I should really call Fred Burns in here with me, since he worked pretty close on the dynamics on this in the wind tunnel, testing. But we immediately went into a pretty good soul-searching on the wing, itself, and looked at going back to the whole high-lobe sail. And also looked at a different type sail, which they called a T-sail, which is essentially - it had the same lobe as the low-lobe, if you want to look at how high they stuck up, as far as the angle is concerned, but what you ended up doing is you put a web on the center structure - you recall what the paraglider looked like - you had a deal like this, looking at it from the side - essentially what they did, they extended a web here - now this had been moved up here and the wing was - So you kept the same lobe ratio but you got your height this way - this tended to rise maybe a little bit higher but that was essentially how we got it. In the testing, it was decided not to use the T-sail. We did considerable testing here and it was decided not to use the T-sail, but to stay with the high-lobe type sail. The final testing that was done at Edwards prior to the next manned flight tested both systems on a full-scale wing. However, because of our lack of knowledge and minimum experience with the T-sail, it was decided that we would stick to the high-lobe sail for manned flight, which was done in December of '63 - or was it '64? So this pretty well, I think, gives you basic information on the progression of the TTV. The FSTV - we were having quite a bit of problems with the deployment, itself. We had - if you've seen the pictures on the deployment sequence, you went from taking a can off and pulling out the U-shape and inflating the booms while you were in the U-shape and then going from the U-shape to a flying wing in about a second.

Hacker: I didn't realize it was that fast.

Andrich: Well, let's say it took a little bit longer than that on a pay-out but - about two seconds, 2.3 seconds, I think was what we looked at. We saw that we had a, because of our wing and the dynamics here, hell of a high shock load on the wing itself. In fact, the first time we deployed it, we stripped the sail completely off the frame. It just ripped right off. So we went back and we redesigned - put some doublers, if you want, on critical points on

the rear end of the sail and redesigned several of the places - sewed the sail across in order to essentially make it rib stocks - added rib stocks within the sail - in case it would tear, it wouldn't continue to tear. A lot of these things added considerable weight to the wing. And these were done prior to the manned flight. We, then, went back to fly again and we found out that now we ripped out the forward socket - right out of the thing. So we went into a redesign on this area and beefed it up quite a bit. We, then, had, I think, another phase in this area where we had - at one time, it just came loose by some unknown reason and the next time, after we fixed it so it wouldn't come loose, then we ripped it right out of the - it ripped our valves right out of the nose of the frame. It was shortly after this, or right about this time, that we realized that - if we were going to fly with this particular wing and get the dynamics so that we could handle it - we'd have to go to a drogue-chute arrangement to, one, slow this mass down and, two, to help get the nose up, in order to make a pull-out of this thing, doing the payout of the cable, the forward cable. We went through a series of drops where we progressively increased the size of the drogue chute. I think we ended up with an 18-foot, as I recall, drogue. And this was done for two reasons. One, it slowed the whole mass down and allowed for a more systematic and repeatable deployment of the wing. We also went into a phase where, instead of releasing everything at once as we had done before, essentially what we did, we released the nose and we cut the re-straints on the aft boom-------- on the aft of the booms and the whole thing did this and then the forward cable would pay out and it would slowly pull out. We then went to a phase where we re-leased the aft booms and essentially allowed it to fly in a, what we called an, L. And then slowly released this, in order to get it to a flying shape. And this was essentially what we ended up using.

Hacker: Originally the deployment was directly from U to the flying wing? Later on was when the intermediate L-stage was---?

Andrich: Yes.

Hacker: I didn't know that.

Andrich: I'm not sure that that's too clear anywhere. I'm not even sure I've got papers here that show it now. I think I've got it in that series. But essentially this is what we ended up using, in order to get the thing down to a repeatable system. Some of the early testing - or some of the pictures that depict this show it in

an L, essentially. What they're doing is they're showing it right after we cut the cable and before we had time to pay out this forward payout. This is the other area where we had considerable problems, in that, because of the high dynamics before we went to the large chute to slow this thing down, we were breaking this forward cable. In fact it seemed like we broke it every damn flight forever, and we attempted several methods of controlling the payout system in the front. Again, this payout system was not the one that we had planned for flight, for orbital - return from orbit. We thought that the design that we had, which had within its design the ability to be varied in speed and was a good one from the standpoint of - during this phase, we were attempting to find out what was the best payout rate. Our problem was that we had a design that, again, was a kluge and gave us considerable problems, which I think we could have whipped if we had gone to another method. The wing, finally, I should say the deployment technique was finally whipped after about 18 flights. And then we had some - a couple of instances where we had some boo-boos in the way the packing was done - the packing procedures ---

Hacker: My impression, from looking at the reports on the FSTV flights, was that after about - I don't remember the numbers - say, from 12 on, the deployment sequence wasn't a problem any more, but the - achieving the stable glide ---

Andrich: Yes, this was part of it. But, again, see, what we had done - and we didn't realize it until the man flew - that our problem was that we didn't have a stable wing - we had the low-lobe sail on it and it wasn't until the man had actually gotten in it and saw that he couldn't turn the damn mess and couldn't make it do anything he wanted to do, and once he changed his dynamics slightly on it and got into this kookie dutch roll that he had the damn thing essentially doing - I don't know if you've ever seen any of the movies on it - it was fantastic.

Hacker: It sounded pretty bad from the report.

Andrich: That guy was the coolest cat I'd seen in a long time, because he sat up there fighting this beast and, essentially, he was in a stall, and didn't know it. The wing was actually stalled and fluttering like a leaf - that's essentially what it was doing. It was doing this and coming on down. He didn't realize it until he probably lost a thousand or two thousand feet of altitude. But he kept talking. We

kept yelling for him - actually, I wasn't there at the time - the people there were telling him to get out of it. And he kept trying to analyze it and tell them more. He talked the whole time on the tape. It was fantastic how much that guy put out on that particular flight. I don't recall if his - the transcript was in that first TTV report, or not.

Hacker: I haven't seen that.

Andrich: I used to have a transcript, but I don't know if I have it or not, any more, but it was ----

Hacker: One of the problems I'm running into is I haven't been able to run down copies of the reports - the TTV report or the - well, the final paraglider report, even.

Andrich: I've got a copy of the final paraglider report. It's one of the few that are in existence.

Hacker: I was wondering about that.

Andrich: I don't know what happened. I don't guess they threw their copies away. Nobody wanted - when the GAO started looking, nobody knew anything about anything. I lived with GAO for about two months, it seemed like. Every time I looked up there was one of them looking in my door.

Hacker: There was a GAO study of the paraglider, wasn't there?

Andrich: Yes.

Hacker: When did that happen?

Andrich: When?

Hacker: Was it recently or was it a couple of years ago?

Andrich: Oh, a couple of years ago. This is the only reason I can remember anything - I had to recall quite a bit for them. A lot of the stuff, I just turned over papers to them and let them browse through them and look at it. A lot of that stuff's gone now, incidentally; I got rid of it. Most of that stuff, I guess, it ---

Hacker: It surprises me how hard it is to turn up - paraglider,

in particular - I don't think it's going to be as true of some of the other R&D things ---

Andrich: I'll tell you what you really should do is to contact everybody right now, whether you're not going to talk to them for three months, and I'm afraid it's almost too late now, in a lot of cases, because guys cleaned out files like crazy, stuff that may have been very helpful. In some areas, it won't be a problem. A few of the people kept their personal-type notes and stuff like that, that may be a pretty good record. Others put out a - there's only two or three that I'm aware of along this line - sort of a personal summary of what happened during their development program. I think one of them is Ed Whitacre, who did something on the Agena engines, primary propulsion system engines. He's probably got a pretty good story on the secondary propulsion system, but he did not put out a report on it. The other one is Hilary Ray, who, I think, has put out a report - at least, I know he was thinking about it - on the ejection seat and the recovery parachute system. But he was primarily an ejection seat man. But I think - Ken Hecht is a pretty methodical type guy and he kept very good records. I know when I worked for him, it was a pleasure to look through one of his files, because all his notebooks, his scheduling, have a beautiful record on - chronology - from that standpoint.

Hacker: I'm planning to go over and talk to him this week.

Andrich: Ken - you're going to talk paraglider with him? Ok, he's got quite a bit of background there that I think will be very useful.

Hacker: There were two things I wanted to ask you about. First of all, the people that you would suggest as important to talk to - Smith and Burns, I guess, are two.

Andrich: Smith has gone.

Hacker: But he must be findable - I mean, he must be around some place.

Andrich: He's at North American Aviation - I don't know if it would be worth your effort, there, to get with Bill, because I think there's enough information in-house, during that period. I would say ---

Hacker: He left NASA to go with North American?

Andrich: Yes. He's on the Saturn program. I would say Ken Hecht, Les Stewart, Bert Jackson (James B. Jackson) ---

Hacker: What was he doing? I haven't run across his name.

Andrich: He took over Les Stewart's function as far as program control. He took over before the 1484 contract - I don't recall how far in advance of that, but in that general time period. I'll tell you, one of these days what might be worth more than anything else, once you've had a chance to talk to him - when you talk to Ken, you might ask him - whether it would be worthwhile to have a general bull session with all of us, because a lot of times, somebody will recall to mind something that had come up. Somebody will say something that will trigger something and it'll bring it out. It might be harder that way, in a way, in that you won't be able to follow all the trends of thought; but, maybe, after we spend a couple or three hours with you, we can then summarize what we got out of it. It might mean more to you and to us, too, on what we've got. I'd be willing to sit through a session. Again, it's pretty hard to get us all together at the same time, since we're scattered to the four winds now. Although most of the people are on Apollo Applications; from that standpoint, it may not be as difficult.

Hacker: Yes, that sounds like a good idea. (NOTE: Bart, if you do, I'm going along to monitor and see that they don't all talk at once - because it will be impossible to transcribe, otherwise. Sally)

Andrich: I really think it might be worth while to look at it from that standpoint.

Hacker: Were there any other people that you can think of?

Andrich: You've got Hecht and you've got Jackson - well, Bernardi, Lou Bernardi spent some time, early in the program, primarily in the wind tunnel area. But I think Fred Burns has probably got as much background there and it might be easier just to pull Fred in. He works with Ken, incidentally, right now, so that will simplify that part of it. I think it would be worthwhile, especially some of the earlier history which I don't have. I was getting aboard, but when you - you're awed by the whole massive mess that we had here at that time, and we had quite a bit of dissension in the ranks, if you want, when they reorganized about the time

I came aboard. When I first got on the paraglider program, it was before Mathews came on. Then it was switched from under one man to under another man.

Hacker: I had the impression that there was pretty strong feeling pro and con about paraglider, that you had, essentially, some people that thought paraglider would never work and didn't want to ---

Andrich: Well, it has a lot of disadvantages. The man is in the loop. If, as an example, you had to reenter at night, and land at night, for some reason, he's got very poor vision. And also in any emergency landing that you made at night, it was almost an impossibility, you might say, because he couldn't see anything. We were very lucky in that, in all the Gemini flights, we essentially had a controlled landing, even though spacecraft 8 - when Armstrong had to come in early because of thruster problems - even though we had to land him in an area that we knew nothing about, we were able to land him in the daylight. He didn't land at night. His touchdown point was in a daylight area, although the daylight was getting late. Still, this is one of the things that worried us quite a bit. Several months, actually I guess it was about the time during the GAO investigation after the program was over, and I gathered up a bunch of Les Stewart's papers and, in looking through some of his stuff, I ran across a letter from Chris Kraft---

Hacker: Yes, I've seen a couple of letters from Kraft on the disadvantages of paraglider.

Andrich: ---pretty well brought out some of the disadvantages that I had personally felt the minute I got on it - right when I came aboard - I was very seriously thinking about moving off of it, off the paraglider program. Ken Hecht asked me to reconsider. I, at that time, told him the disadvantages I saw in the system, primarily from a safety standpoint. The idea that you've got to inflate that damn wing, the chance of a break in the wing causing you to lose your gas and lose your rigidity and lose your controllability and such things. The high chances of this happening - that you might snap a cable and throw it up and cut the wing, as we did many times in the deployment phase. These were some of the things that worried me; and I looked at night landings, and I guess I - I expected to see several aborts, really, in the Gemini program, and we were fantastically lucky from that standpoint, in that, in all cases we were able to land in daylight. We had to, in

only one case, land in a remote area. I guess you can say this was, to a great extent, due to good planning by our recovery people, and the whole team that worked on the thing. And not only that, but a hell of a lot of conferences and forethought that went into the way systems failed and everything else within the Gemini program. I know, during Lovell's mission, when they went for the 14 days, and the fuel cells went out, there was a hell of a lot of people panicking - "bring them down early. We've already proved our point. He's been up 12 days. He's been up 13. Why press it now?" But there were enough people that had confidence and they had enough failure rates and failure histories on the system that were readily available, and were talking to Chris and the people that were necessary to get the panickers off their backs, really. I think from this standpoint it showed a hell of a lot of planning along that line. These were some of the things that, at that early stage - they were having as much trouble with the fuel cells, I guess, at that time, on the paraglider - when I got on the paraglider program ,as they were with anything else.

Hacker: Yes, I've noticed a GAO study of the fuel cells, too.

Andrich: I think some of the things that they can look back on, now, within the paraglider and, again, with the fuel cells, is that, in some cases - I'm looking at this from a personal feeling, and I think Mr. Chamberlin would probably tell you the same thing, and Mr. Mathews - we set some standards that we didn't want to give up; we didn't want to compromise, yet, as we went along and ran into problems and really got ourselves with our back to the wall, especially in the paraglider, we finally didn't make it at all - we had the capabilities compromised and not compromised the mission - we just compromised how we did something. An example with paraglider, we had planned to deploy this thing at 50,000 feet, essentially. That would have been a hell of a God damned shock - to start the deployment at, say, between 50 and 25,000 feet. During our test program, because of the capabilities of the aircraft that did the drops, the best we ever did was 33 - but we pretty well proved as we went along - we added the big drogue chute and all these thing to slow it down - we could have, if we had had any sense, we should have realized our problems early - except we wanted this great - by deploying at a higher altitude and with the glide capabilities of the paraglider, which are really poor, if you want to look at it from the standpoint of a glider - it's got the same glide characteristics as a F104 - you recall that airplane with the short,

stubby wings? - as that airplane's got with its engines out - like a rock - that's pretty damn much. L/D of 3 is pretty damn sad when you start talking about a glider, when a glider, I guess, talks in the neighborhood of 20. But we were upset with holding the range capability of this thing, see - if you had an L/D of 3 and you got a wing deployed at 25 or 30,000 feet, and you had a 75,000-foot range, if you want to look at it from that standpoint, you just went straight. So, at 25,000 feet at 3, it's 75,000. So you look at that and you had a 15-mile range that you could use. The later Gemini flights we were within five miles, seven miles, of the target and the last ones within sight - three, four miles, like that - with this kind of a capability, hell, if you had come in - even with us deploying at 10,000 - finally getting the thing deployed at 10,000 or 15,000 feet - with an L/D of 3, in this flat area around here, you had a 6 to 10 mile range and you could damn near land it at Ellington every time if you - we didn't have that capability, here, we were looking at New Mexico more than this area for recovery - but it gives you an idea that - had we had more forethought in this area, we probably could have gotten paraglider aboard. The same thing with the fuel cells - had we realized earlier or taken some advice earlier - in our particular case, the advice never came from the contractor. He had the same obsession we had - the range. But the fuel cells, they kept having the failures in the fuel cells until they changed the coolant temperature. Once they decided that they could change the coolant loop at MAC on the overall cooling system - the ECS system - and get the temperatures down within the fuel cell, itself - granted some of the efficiency fell when you got this down, but you stopped having failures. It didn't keep exploding on you. So some of these things, they finally realized early enough within the fuel cell program and took advantage of this and were able to ---

Hacker: That's an interesting point.

Andrich: This is something that we didn't realize early enough, at least, I look at it back that way. We also had the problems that, within Gemini, you had people that didn't believe in the paraglider per se. Not necessarily didn't believe in it, but you had a system that was damn safe, with the parachute; you had a means of coming in. There were people within our program, I, for one, to some extent, although I think I gave everything I could to try to develop it - more from a challenge standpoint than a - really, I think, part of it is that when you're working on something, sometimes you can get so enthused with the project and the aim that you tend to lose

sight of the practical side of the thing. I think sometimes this is where we miss the boat, by not listening to people like Faget, who very early say "You're wasting your time. You've got a system we know will bring you in anytime." So, one of the concepts of Gemini at first was to do a land landing - to land on land rather than on water. It was one of the few goals that Gemini did not make. In fact, I guess it's really the only one that it didn't make.

Hacker: Chamberlin said that he thought that, even at the end - I guess paraglider was officially dropped in the summer of '64 - shortly after that everything started looking really good for paraglider. The flights were ---

Andrich: It didn't look that good. In the summer of '64, we still did not have a good deployment. We were just then getting into looking at the bigger chute, slowing this thing down, changing our whole concept of how to deploy it. Before,we were trying to look at split-second timing to get this thing out and flying. We then realized that your chances of doing this repeatably were pretty damn slim. It was better to slow this whole mass down and to slow down the whole deployment sequence in order to, one, see what the hell was happening was one of the things. You could damn near miss what was happening even with high-speed tests - and the way that we to be accurate was just with theodolite cameras on the ground that were taking pictures. We had chase airplanes most of the time but they couldn't get close enough - from a safety standpoint, you couldn't get much closer then they got, again, trying to use handheld cameras - if you work this way, it gets pretty difficult. I'm not sure that we didn't have random success. I don't think three deployments - three successful deployments and glides - and they really weren't three successful glides, even, because there was only really one successful glide - or two - as we were still fighting the kookie wing concept - low-lobe - when we got the first decent deployment. I think we were on the right track and I think what we had was, with a few more refinements in the way we packed the wing and stuff like this - I think we had it knocked, as far as that was concerned. But I think what you really needed, the same as developing any parachute system - boy, you look at 50 to 100 successful drops, or maybe 500 successful drops, before you decide that thing is man-rated. I think this is some of the things that Jim didn't want to admit, from the standpoint that, to get 100 successful drops, even though we came up with a system, you never could have gotten more than two a week and you're looking at another year, at the best, of development on this

thing. And I think within that time period we would have had some failures, either due to boo-boos in the packing, which you'd never be able tie down - it was a human error type deal. These are the ones that always get you. And the possibility in a hundred times that you wouldn't have some type of equipment failure. Again, see, we were using the same old hardware over and over again. It wasn't like a Gemini - you go up and you know it's good and the possibility of failure of that particular piece of hardware is pretty nil, when you haven't ever used it before except to develop it and check it out. These are some of the things that worried us. We were at a point where, I'll admit that if the program had been given emphasis and we were insisting - if we had no other feasible way to land this thing, it would have been developed, because we would have thrown enough money into it to develop it. Not that enough wasn't already thrown into it when you start looking at the type of money that went into the program, but I think - during the last phases of the program, both the tow test program and the - primarily the full-scale program, we were scrounging pyrotechnics all over to keep the program alive. It - the procurement on the parts, the expendables, had only been made to cover 20 - 15, I think 15 - it started with 15 deployments. Once McDonnell got out of the act and was told "no more" there, we did not have access to pyrotechnics and stuff that they were developing for the paraglider. That was dropped. We were able to use some of the things that they had - we used the same can - the separation system, and we were able to scrounge more of these out of them. By the end of the program, they were asking for them back, because they were looking for further testing from a reliability standpoint - they weren't going to use them for manned flight, but they were looking for more pieces of hardware to complete their testing phase. And it was stuff like this that was killing us. Hell, I was beg, borrowing and stealing parts from all over the nation as far as pyrotechnics were concerned, for cutters, and stuff like this. I'd call Langley and ask them if they had any of these things and the guy'd say, "Well, I've got two old ones I've got in my desk. I don't know if they're any good. I don't even know if they've got any powder in them." So we'd take them and x-ray them and see if they were okay. And then go ahead and use them. We had fantastic luck. We never had one of these beasts fail. Of course, we had redundancy - we always had a backup. Maybe one of them didn't work, or wasn't as powerful as it should have been. But on every flight we were able to have the redundancy present. But during the final six

months of the program, it was quite a test of scrounging - in fact, old Jim Chamberlin told us one day, "Why don't we just build those pyrotechnics here?" Hell, I don't know anything about pyrotechnics. He said, "Fred Burns might." He didn't know anything about them, either. So he wouldn't take "no" from us. He didn't think that was final enough and he got hold of Kinzler, who runs the shops, talked to him about it. "Hey, how about building some of these?" He practically told him to go to hell; he wasn't going to have anything to do with that type of - that sensitive work here. But this is how - what we got down to in the final phases. And this, to some extent, I'm sure, hurt our operation. I scrounged parachutes out of here for this drogue-type chute. And we scrounged parts. We were shipping these on - at the last minute, practically, as we found we had a problem or ran out of something. We were trying to fly one or two flights a week. It was quite a mess, trying to keep the logistics going on. But my personal feeling is, I think we had a system that would work, but it really wasn't ready to put on a vehicle. But we looked at it late and tried to recall the time - we went through an exercise, me and Bert Jackson and Fred Burns, to take the knowledge we had right then and try to regroup and go into a program - this was about a year, I guess - it must have been that summer that we finally decided it wasn't going to make it, or maybe that spring, and it looked like 15 months was the best we could do to develop and go. This was really before we had the deployment down, when we really felt we finally had something and we were going to have to look into, then, developing a new payout system, which we did not have on the forward deal, and it did not - it had been knocked out of the budget. I think we had a pretty good technique in the conceptual stage, but - it would have been developed in time for the last flight. But then they were talking about they wanted a manned drop from an airplane, several manned drops from an airplane, prior to getting into this. We added several things - this development program, tying a man into it with the TTV, produced several problems that you didn't have with the rest of the systems. One example was that when we got ready for the first manned flight we didn't have a qualified ejection seat. North American's flight test department balked at using that seat in the spacecraft, in the tow test vehicle, and they decided they would rather not have an ejection seat in at all, because of the nature of the Gemini seat, trying to get out of the harness and everything that was developed for the Gemini seat took so much time and it was so damn hard to get this Gemini backpack, that was integrated within the seat - your parachute pack - to get out of that thing was an impossibility. We looked

about taking the backboard out and letting him wear just a back chute, try to rework a pan so he could use the old Wilks (?) chute that they had. And it was finally decided to take that thing out and put another type seat in there. These are some of the things that also hindered us. The other thing that was hindering us was that, in deciding to go to a manned - I should say - as much toward the final configuration as we could, we were depending on McDonnell to build some hardware and furnish some equipment and essentially have that stuff man-rated, like some of the electronic components were being developed by Minneapolis-Honeywell to tie into the attitude control system with the regular Gemini attitude control system, and none of this was coming off the line early enough to - at least, it was falling behind. We finally got them out and got them qualified along this line. They weren't qualified for space environment but they were qualified for - as far as components reliability was concerned. It caused a hell of a lot of growing pains that you normally don't run into in the normal sequence of a development program.

Hacker: That seemed to be true of the paraglider program all along, that there were problems not related to the development of the system.

Andrich: That's right. A good example was the half-scale drops where we had a failure of a pyrotechnic that had a fantastic reliability rate on it. They had not had a failure in 20,000 firings. The first time we dropped one out, the thing didn't work. So something like this - now, whether it got damaged in installation, which we could not prove in any way - it didn't look like anything had happened to it. It had met all the requirements, so far as its production control was concerned, and it was part of a lot that had been approved and accepted by the government and everything else. Our luck was that this one failed.

Hacker: There was a whole business of developing tow techniques which---

Andrich: That's right, which to some extent, I think, we beat a dead horse here for nothing. I don't know if it was people or - I know it was people, but what I'm getting at is - most people felt that the astronauts did not want to be direct-lifted off the ground. As we finally ended up flying the paraglider with a man in it,, he was direct-lifted in this last launch in order to save time and to get away from the tow-off problems that we had ----------

in the early, I guess you'd call it, liftoff with the wing itself - you know, that we towed down the runway. The wing was attached to the back of the vehicle and once you got up to wing-flying speed, you released the wing and it came up to flying position, then you increased your - continued to accelerate until you got total vehicle flying speed. During that period, where any little wind could mess you up and throw that wing off to one side, once you released it - you were so high that - the relative velocity of the winds was so low that your chances of having a wing go to the side and end up in the - on the desert floor were pretty damn good, in fact, we busted a couple of them, trying to use this technique. There were quite a number of people that advocated this other method for quite some time - one of them was the helicopter pilot, because he saw so many problems with this beast that he thought a direct lift was the only way to go. We couldn't seem to convince the North American pilot of this. I think he was convinced more from the standpoint that he looked at it from a training standpoint later - and the problems that you get into of setting this thing up on its side and getting the wing just right and getting everything all arranged just perfectly, and holding the lines a certain way and everything else, to finally get the initial lift off the ground with the chopper and then tie it toward the altitude. He saw problems here ---

(End of side one. End of interview?)