May 1934 - W-1, Our Trials

What a place to have to change socks!

Mel could not move the elevator control, so he merely throttled back and made a glide "socko" landing in the next field, about 25' in front of another row of 25' trees. The trees damaged a portion of the auxiliary airfoil and the front of the fuselage. We all ran toward the plane and saw Mel get out with his hands covering an important part of his anatomy, which even with the safety belt had been stopped in his forward motion by the control stick between his legs. After that we did disassemble it and truck it over to Morgan's little field where we rented space in Nathan Scudder's hangar. It took about 6 weeks to get it flying again with the help of my brother George, who was living with us at the time.

I should have mentioned a bit about the very first flight trials of the W-1. Although Langley Field had no runways at that time, it did have a wide concrete ramp about \( \frac{1}{2} \) mile long running in front of a row of hangars. Bill McAvoy and Mel Gough took turns taxiing the plane up and down this ramp and even made screechingly sharp turns at relatively high speed without difficulty. They then took it off/20 or 30' straight ahead and landed again, trying out the controls. When this all seemed satisfactory, Bill McAvoy took it off
the grass and flew up to an altitude of about 3000', trying out the controls and flight characteristics in a gentle, but fairly complete manner. Not quite complete enough, however, for when he made his last turn to his final approach at an altitude of possibly 300', the ailerons would not bring the wing up out of the bank. He had a scary moment, but finally by use of full rudder with the ailerons, the wing leveled out, and a satisfactory landing was made. The directional stability, found low in the wind tunnel tests, was not sufficient to overcome the adverse yawing moment of the ailerons. The fin area was therefore doubled in size and this took care of the situation satisfactorily.

After the flight tests by the NACA pilots, their main impression was the unusually satisfactory performance of the new landing gear. This thought got around fairly rapidly. At the time we were ready to start our own flight trials, we had no really competent pilot left. Bob Sanders was a competent pilot, I believe in the Naval Reserve at that time, but he was down in Peru. Bob Platt, when he saw that we were likely to be able to fly the plane, took some dual instruction in a Doyle and then soloed in the W-1.

He and I did all of our flying with the W-1, although neither of us had a pilot's license. This was all right at that time, so long as we did not cross state lines and engage
in interstate commerce. When I had flown Jennys in 1923 or so, there had been no government licenses for either pilots or airplanes. One could, however, get a license to fly in air meets through the FAI and in this country its representative, first the Aero Club of America and then the National Aeronautics Association. The W-1 did not require a license, either, but it did require a number, NS67. This was just a registration number.

We kept the airplane in Scudder's hangar at Morgan's little field, but did most of our testing on Langley Field, with the permission of the Army Air Service. After our first familiarization flights, we wondered how small a field the airplane could be operated from as a regular thing. Obviously long the Morgan field, with its 1200'/runway and its 800' short one, surrounded by ordinary fences, was comfortably adequate. In fact, the general run of light airplanes of the time such as the Fleet, Scudder's Doyle, Jake's Pitcairn and Whitney's Buhl-Bull Pup were using the field satisfactorily and without difficulty. And the W-1 could take off and land in a much shorter distance than these airplanes.

For our trials, we laid out a little strip, at first 50' wide and 300' long, and marked it by means of little white flags at each corner. The strip was laid out parallel to the wind of the time for the first trials, but later we tried it at all angles with respect to the wind, even direct crosswinds.
After my experience of engine stoppage when I was just off Langley Field, we still kept the strips near the edge of the field but flew in such a way that we went around the edge of the field and could always land back in Langley, if something happened. (I should have mentioned in connection with the engine failure that it was caused merely by the ignition switch clicking off. We had placed the ignition switch on the left wall of the cockpit and the brake lever was near the wall and somewhat ahead of it. The NACA pilots had noticed that the switch was in a poor position but we had not changed it. I must have brushed against it and put it right on the center of its motion so it could slip off in either direction. While using the brake on a previous landing, then during the flight it merely slipped off due to the vibration to the "off" position. It did not take us long after that to change the location of the switch. From the very first trials, both Bob Platt and I, at that time both certainly inexperienced pilots, had no difficulty taking off within the 300' strip and landing within it and coming to a stop. We pretended that the front edge of the strip was a cliff and that we had to put the wheels down somewhat beyond it. After we got a little sharper, we cut the length of the strip down to 200' and found that we could ordinarily stay within that limit; however, we couldn't always quite do it.

With our very effective shock absorber arrangement, we could land safely if not smoothly with vertical velocities up
to about 20' per second. We made use of this feature to make our shortest landing runs. We would glide in toward a landing at an airspeed of possibly 5 or 6 mph above the minimum, and then when approaching the ground, pull the stick clear back in such a manner that the flight path would level off at 4 or 5' above the ground. Then, with the stick held full back, the airplane would sink to the ground at somewhat less than its minimum gliding speed and with a fair amount of vertical velocity. Then, with the brake held on during the entire manoeuvre, as contact was made, with the airplane pressing down toward the ground with 2 or 3 or possibly 4 times its regular weight, the initial braking would be very effective. Using this method, the total ground run with ordinary winds of say 5-10 mph was only about 60'.

About that time, President Roosevelt appointed a board, I believe it was called the Board, to investigate and report on the state of aeronautics in the United States. The board included Edward P Warner (I believe at that time Assistant Secretary of the Navy for Aeronautics) and Dr. Jerome Hunsaker, Professor at MIT and member of the National Advisory Committee for Aeronautics. These men I knew; and the board included also Mr. Howell, Marlowe and I seem to remember an admiral by the name of Cone. At any rate, the Board visited the Langley laboratory of the NACA and they asked me to give a flight demonstration of the W-1. I decided that the short field performance would be demonstrated more effectively
from Morgan's field, so they came out there. I remember making a short field landing, a normal landing, using only about half of the 800' runway and then going around and making a fast landing, putting the plane down at 60 mph and using the brakes and still using only about 3/4 of the 800' distance. We had limited the elevator control by that time, so that unstalled flight could be maintained with the control full back, either power on or power off. I demonstrated this at an altitude of a few hundred feet, making climbs with the control full back, glides with the control full back, and in both cases, turns in both directions. They appeared to be well impressed by the performance. They asked me to prepare a summary of my thoughts on the light airplane and general aviation picture and about a month later I gave this at a hearing in Washington.

I just noticed a Newport News newspaper article dated 28 July 1934, which shows that the Board consisted of the chairman, Mr. Clark Howell, Warner (editor of Aviation magazine at that time), Hunsaker, Col. J. Karl Cone, F.K. Lane and E.J. Ferris.

Bob Platt and I kept on running a few trials now and then, but after a short time, we figured that we had pretty well proven the capability of the landing gear and of the good stability and reasonable control at all angles of attack that could be maintained in flight, that is the effectiveness of the limited elevator control, both power on and power off. We
were looking forward to the next part of our program, in which we would try out 2-control operation and possibly a flap with spoilers for lateral control near the stall. In the NACA wind tunnel tests, we had worked out a spoiler system with a slot carrying air past underneath it, which we called a Slot-Lip aileron, that looked reasonably promising. About a month after the Howell Board demonstration, I was making some flights and on one, I asked Bob Platt, who stayed on the ground, to check the height I had, possibly in terms of span lengths, as I crossed the northern fence of the 800' runway. I took off and climbed steeply and his report afterward was that he thought I was 3 span lengths high, or 90' as I crossed the north fence. I kept on climbing and at an altitude of about 150' the engine suddenly jerked to a stop, and everything was very quiet. I am told that Henry Reed, who was shooting at a nearby range at that time, looked up and said, "Now we'll see what it can do."

From one standpoint it didn't do very well, but from another it did very well indeed. Just north of the flying field, there was a half-mile race track for practicing with horses. This was an area about 1000' long and I was nearly over the middle of it. The natural place to have landed would have been in the tennis courts, which were just north of the race track, but people were playing tennis, and anyway it was surrounded by the high wire fence with steel poles. To the left was the farmhouse and to the right there was a large area where
the earth had been excavated and taken away to fill in at Langley Field, leaving an area about 8 or 10' below the natural surface of the rest of the earth there, and filled with little mountain ranges left by the earthmoving machinery. I pulled all the way back on the stick and turned about 45° to the right, coming down and landing right on the race track in the turn.

I came down at a very high rate of descent, but the nose was just a little bit below the horizon and the airplane struck the ground in a relatively flat attitude. The nose gear didn't fail, but it pushed the front end of the fuselage up and the right landing gear did fail. The right auxiliary airfoil and the right wing tip were also damaged where they hit the ground and there were a number of other portions somewhat deformed, but all in all the damage was not so very bad.

I was not hurt at all except for a little scratch on my forehead. I really believe that the characteristics of the airplane saved me, because if I had been in an ordinary airplane and pulled the stick all the way back that way and started the turn, it would have stalled and started into a spin and I would have hit the ground pretty well nose down and that would very likely have resulted in a fatal or at least a very serious accident. I don't suppose, however, that the spectators that saw it understood it that way or figured it that way. The tennis players and the range shooters came over, took a quick
look, and went back to their activities. Bob Platt went to
hire a truck to get the plane back to the hangar, which was only
about half a mile away, and I got out my tools and started
disassembling the plane on the spot. One man, Dave Bierman,
who was at the airport with his own airplane, got his tools
and came over with hardly a word and helped me disassemble it.
He was the only one who helped out afterward and I sure
appreciated it. Dave ... his brother Arnold later disassembled
the engine for me and we found what had caused the difficulty.

The master connecting rod of the 7-cylinder radial
engine had pulled right in two. It was constructed of two
pieces held together by means of two large rivets and one piece
had merely pulled through past the rivets. It had been made
too light and this defect was remedied by means of one-piece
master connecting rods in later Pobjoy engines.

This effectively completed the W-1 portion of the story,
and we were ready to go ahead with the next stage involving
flaps, slot-lift ailerons and two-control operation. Before
going into that, however, I'll take the opportunity to go back
and tell about a number of occurrences that took place during
this same time period, but which I omitted in order to give a
fairly continuous W-1 story.

**EXPERIMENTAL STEEL PROPELLER**

During the early spring of 1931, my brother George had
been married to Edna for nearly a year, but in the depth of
the Depression of that time, his job had given out and he was not able to find another one to support them. For some time I had had an idea in mind for a new type of aircraft propeller, but had not had time to develop it. So we invited George and Edna to come and stay with us while he constructed an experimental propeller and we ran tests on it. Possibly it would be worth developing further.

The propeller was fixed pitch and constructed of sheet steel, heavy sheet steel, almost plate. It was constructed mainly of two long strips which went from tip to tip of the propeller. In the neighbourhood of the propeller shaft, the two plates were parallel to it, one on either side. As they went out toward the tips, they were twisted to form the correct pitch angles. The top side of one blade became the bottom side of the other. In the center portion additional layers were added as needed for strength. The parts were to be electric-welded together to form a single piece, the welding where the sheets lay flat against each other to be seam-welded.

George hacksawed and formed the various pieces on a bench in our little garage. He investigated the arc and spot welding that was being done in the Newport News shipyard and made friends with one of the welders. He was interested in the project and did the welding for George in the shipyard. Unfortunately they did not have a seam welder, and a spot
welder was used, which was not nearly so satisfactory. In fact, suitable welding for a project of this nature at that time would have been quite a development in itself. The propeller was designed for about 90 hp and for the testing I bought an old Hisso 180 hp engine and mounted it on the rear of an old Model T Ford Roadster which incidentally I purchased for about $25 or $30. We drove this portable test rig along the sandy beach of Chesapeake Bay at low tide to a point where the land projected a bit somewhat north of Grandview, where people went swimming. When a propeller blade breaks off, it flies out fairly close to the plane of rotation. Out on the point, we placed the rig in such a position that if a blade broke off, it would fly out into the water on either side and would not hurt anything. This saved building a husky guard around the propeller and it was also out in an isolated area where the noise did not bother anyone.

As I remember it, we ran the propeller full throttle for 15 min or so at a time, and then shut down and examined the propeller. After about an hour or possibly a little more, we noticed that cracks were developing near some of the welds. With a little more operation, there was further development of the cracks and we decided to stop. I decided that a successful conclusion would require a great deal of expensive development and that it was far beyond our means. So the project was dropped at that point.
Edna was carrying their first son, Alan, at that time, and had gone home to prepare for the occasion. Shortly after the test, George also went back to Chicago.

A VACATION FROM AERONAUTICS

In mid-summer in 1931, after Edna had gone back to Chicago but with George still with us and before our tests had been made, I came home from work one day so worn out and fagged out that we finally decided that I should take a vacation from aeronautics. That evening we hired a local lady, Mrs. Cook, to prepare the meals and take care of the children during the day. During the night George took care of them. Mrs. Cook charged $5 a week.

The next morning Dorothy and I were off in our 1929 Model A Ford in a completely carefree manner, not even knowing where we were going. We thought we might spend a little time in the Virginia mountains, but that evening found us in Washington, visiting with Henry and Josephine Berliner in their home on Tilden Avenue. This was the first time Dorothy had met the Berliners but naturally Henry and I talked aeronautics almost exclusively.

It was natural to go north in the summertime and we drifted up that way and found ourselves in the neighbourhood of Elmira, New York, where I happened to know there was a glider meet going on at that time. It was the main meet that year and we couldn't help drifting over to see what it was like. In
order to be sure of suitable weather the meet ran about two weeks. The gliders were launched off a ridge or bluff with the wind blowing up over it. Some of the gliders were launched with the aid of catapaults or slingshots made up of rubber shock cord from airplane landing gears, but long stretches of it. Men also ran along at each wing tip, pushing and guiding them. The gliders could soar back and forth along the ridge for some time, and then when the lift gave out, they could glide down and ordinarily land in the Elmira airport in the valley below. We seem to remember that this place was called by a name involving the word "meadow" but it was definitely not the area at Harris Hill, which was later developed as a national soaring spot.

These gliders were in no way competitive with the present-day efficient sailplanes, but they were able to stay up an hour or two with the proper wind conditions along the ridge. I found Commander Ralph Barnaby there, and practically running the entire meet. He had been the officer in the Bureau of Aeronautics in charge of specifications, under whom I would have worked had I not taken the propeller job. He was active in the soaring itself and helped the general activity for many years.

After a couple of enjoyable days watching this activity, we went up to the Finger Lakes and spent a couple of days on one of them, canoeing, swimming and just taking it easy. We
then headed for the Virginia mountains, but stopped off at Elmira to see a little more of the gliding meet. Unfortunately the weather was not very suitable at the time, so after a day or so we went on down toward the mountains. As we were going down through Maryland, it just so happened that our road passed the Fairchild Aircraft Company at Hagerstown. It just happened also that at the NACA we were using a Fairchild-22 airplane in connection with some of our tests of high lift and lateral control devices and that a couple of special wings were at the time being constructed for my project at the Fairchild plant. This was about 9 o'clock and I suggested to Dorothy that I'd like to stop in for a moment and see how the project was coming along. I met George Hardman and Lou Reisner, chief engineer and plant manager, respectively, and they showed me through the plant and then we spent some time talking about the projects they were doing for us. All of a sudden someone said, "Won't you go to lunch with us?" I had no idea that the morning was over already, and that I had left Dorothy in the car all alone the whole time. I'll have to admit she was very good-natured about it, having been reading and writing letters.

We finally did get down to the Blue Ridge Mountains of Virginia, however, in an area near Roanoke and spent several days having a good rest there. Even there, there were quantities of buzzards flying around, and I spent hours reclining and studying their soaring flight. So my vacation from aeronautics included the talks on aviation with Henry Berliner, the glider
meet at Elmira, the Fairchild plant at Hagerstown and finally, watching the soaring buzzards in the Virginia mountains.

End of Side 1

Beginning of Side 2: W-lA Trials to 2-Control Trials by Us.

The year 1932 saw the founding of the Institute of the Aeronautical Sciences by a handful of leaders and scientists, including Lester Gardner (publisher of Aviation magazine), Jerome Hunsaker, Ed Warner, Charles Lawrence, and a few others. I was one of the founder members and am still a member, although some time ago it included the American Rocket Society and is now called the American Institute of Aeronautics and Astronautics. In the following year, 1933, the practice of calling a few of the members "fellows" was instituted. An initial group of 20 was appointed, and I was fortunate to be one of them. It has grown to be a very large and powerful organization. For many years I was quite active, and for a time on the governing council. However, my activity dwindled when it became largely a high-speed and astronomic activity, although I still attend some of the local meetings at Cape Canaveral.

During the early 30's the employees of the NACA at Langley formed a loosely organized social group called "The Green Cow". We enjoyed many dinners and dances with this group.
On one occasion I was the butt of a puppet show. Clint Dearborn and his wife, Becky, had made some puppets, one of me and an airplane, and he demonstrated me making a "socko" landing with the plane.

In the late 1920's the engineering department of the Army Air Services was moved from its original location in McCook Field just outside of Dayton to the new Wright Field, a little farther out. On our family drives to Chicago to visit our folks, Dorothy and I and the kids often stopped off in Dayton so that I could visit the engineering section, particularly Jean Roché, who had designed the Aeronca on the side. He kept playing with improvements and we sometimes flew them around. He had a wife and a couple of teen-age boys in a house in Dayton which I have visited, but with his French background, he also had a mistress. The mistress worked at Wright Field and he provided a house for her, right on the edge of Wright Field. On one of these occasions he took us out for dinner with the mistress and then put us up overnight in the house of the mistress, while he himself went back to stay with his family. While this may have been customary in French procedure, it was somewhat embarrassing to both Dorothy and the mistress, who was American. However, the mistress, Ione, was very pleasant company.

During World War II Jean was moved to Langley Field to oversee the research being done there for the Air Force. I saw him once there, but have not seen him since. I understand that
he later built a house with a hangar in it on the other side of Back River, that he built himself a little amphibian and flew across to Langley Field to work in it. I understand also that his wife died and that he married Ione. I may go to Langley in a month or two to witness some tests and if I do get this opportunity, and if he is still alive, I will try to look him up.

In late January 1934, George and Edna came back to Hampton to live with us again. This time, after a bit, I contacted Mr. John Nichols, chief engineer of the Newport News Shipyard and Drydock Company and with his help, George got a job in the shipyard. After he felt reasonably well established, they rented an apartment in a house belonging to a Mrs. Hudgins, about a mile from where we lived. After Mel's "socko" landing in the W-1 in mid-May, George helped me fix up the W-1 through June and to the first part of July. So we got it flying again.

Let's get back, now, to the progress of the W-1 after its engine failure and forced landing just off Morgan Field and its modification to the W-1A.

It occurred to me that since our personal efforts and money had helped the Bureau of Air Commerce to initiate its private airplane program with no financial return to us, and that it was now starting to purchase airplanes that would be designed and built to specifications based largely on the characteristics of our W-1, possibly we might get some financial
aid for the next part of our program. It should certainly be of some interest to their further development. I then prepared two proposals and sent them to John H. Geisse, chief of the aeronautics development section of the Bureau of Air Commerce. One of these involved the elimination of one of the three primary air controls (elevator, ailerons and rudder). Slot-lip ailerons, a form of spoiler involving a slot, were to be installed on the W-1 in place of the regular ailerons. These gave good rolling moments, especially near the stall, and were substantially free from the usual adverse yawing moments of ordinary ailerons. We had developed them in our NACA lateral control tests. Flight trials were then to be made covering all pertinent conditions.

The second proposal covered the installation and testing of a glide-control flap. This flap would be used both as a high-lift device and as an air brake. When deflected to about 20°, this flap would give high lift and low speeds for both take-off and landing. When deflected to higher angles, the lift did not change appreciably, but the drag increased immensely, and the flap was used as an air brake, which controlled the glide angle to a landing without changing the airspeed. It therefore got instantaneous changes in the glide path angle and should facilitate the approach to a landing to a given spot. I had suggested a price of $2500 for the first project and $2200 for the second. This was October 1934.
John Geisse appeared favourably inclined but Dr. Lewis called me up and said, "Fred, we can't let you sell reports on research work to the Department of Commerce, because you'd be competing directly with the product of the NACA. Why don't you sell them the modified airplane?" We didn't exactly like the idea of letting go of our airplane before we had a lot more fun with it, but ultimately we agreed. On November 15, 1934 John Geisse wrote me a letter, making a definite Department of Commerce proposal to purchase the airplane. The Bureau did not consider it proper procedure, however, for it to purchase a home-built airplane from a group of government employees. It was suggested that we have the repairs and modifications made by a recognized aircraft manufacturer, and that they, the Bureau, would then purchase the airplane directly from the aircraft manufacturer. We made such an arrangement with the Kreider-Reisner Division of the Fairchild Company in Hagerstown Maryland, the very plant that had furnished our special wings for the F-22 airplane and the plant that Dorothy and I (or I, particularly) had visited in 1931, during my "vacation from aeronautics."

The W-1 was immediately trucked up to Hagerstown and the rebuilding and modification commenced according to our basic plans. Detailed drawings of the new parts were made by Fairchild. The work went on during the winter of 1934, '35 and well into the spring. During that period, every 2 or 3 weeks I would drive up to Hagerstown for an interview with the
TAPE 8 (Side 2) engineers, taking along either Carl Wenzinger, usually Carl, or sometimes Bob Platt. We would start at 5 o'clock on a Sunday morning and stop off for breakfast somewhere between Richmond and Washington, and arrive in Hagerstown about 10:30 or so. About 4:30 or 5 we would start back, arriving home late at night. A few times the hills had ice and snow on them and we did not always make them on the first attempt, even though at that time we always had chains along. We spent the time with George Hardman, chief engineer, and another engineer by the name of Lew Funisstock, who led the work on our project. Both of these men cooperated very well with us and the work made good progress.

In the late fall of 1934, a young man, Robert T. Jones, 23 years old, started working in the atmospheric tunnel section on a temporary, 9-month employment as a Scientific Aide. With one year of college at the University of Missouri, he became a designer for the Nicholas Beaasley Company in Marshall, Missouri, and among other things, designed a racer with a Pobjoy engine for the 1930 Chicago Air Races. He was unusual in that he read the Greek philosophers and studied mathematics and physics for his own pleasure and satisfaction. The Depression hit hard and he got a job in Washington during which time without the latter three years of college he took a graduate course in theoretical aerodynamics at Catholic University under Dr. Max M. Munk. When he came to the atmospheric tunnel section with the lowly rating of Scientific Aide, he was given the job of
making simple routine computations, reducing wind tunnel data to coefficients.

After a few days we started using a new type of set-up in the wind tunnel involving an airfoil of large cord and short span, set between two end plates extending entirely through the airstream in the open throat tunnel. I mentioned to the fellows that we needed a new wind tunnel correction factor for this particular set-up, and then I went back to my office in the afternoon and worked out one. The next morning when I went over to the atmospheric wind tunnel, Jonesy, as we called him, sort of sidled up to me and said "You know, you mentioned you needed a new wind tunnel correction factor. I worked one up last night and maybe you'd like to see it." I did, and we both had arrived at the same correction. It wasn't long before I found that he had greater mathematical ability than any of them and I soon got to using him in various theoretical analyses. Two resulted in joint reports by Jonesy and me: one, "The Effect of Lateral Controls in Producing Motion on Airplanes as Computed from Wind Tunnel Data", Report 570; and the other, a resume and analysis of our entire NACA lateral control research, Report 605.

He was obviously very competent and we tried to get him put on a professional rating, but the Civil Service Commission would not let him take the junior aeronautical engineering examination because he did not have a college degree. By that
time, I was writing the aeronautical engineering questions for some of the junior aeronautical engineering exams for the Civil Service Commission because they did not have an aeronautical engineer in their service. I, with the help of Henry Reid, Eng. in Charge, arranged with the C.S.C. people, then, that we could prepare a special set of questions for Jonesy and have him take a special examination. He prepared a set of questions which I sent in through channels and the C.S.C. held an examination on those questions. However, they wouldn't let anyone take the examination who didn't have a doctor's degree, not just a bachelor's. We got another employee who took the examination, but to keep Jonesy on, we had to keep giving him additional temporary employment in one way or another, and always at a very low salary. Finally, after he had had two years' experience with us, he could take an unassembled exam, based largely on his experience and helped by the reports which he had written, and he finally got on as an assistant engineer, or P-2 basis, instead of just the P-1.

The reason for bringing Jonesy into the picture at this point is that he made a theoretical analysis of the optimum conditions for two-control flight, using either the (Dr. R.T. Jones is now famous as one of the world's leading aerodynamicists. This work indicated that a pure rolling control without either adverse or favourable yawing moments would be close to the ideal. The slot-lip ailerons should therefore do quite well. Incidentally, the Pobjoy engine that we purchased from Nicholas Beasly turned out to be one that was purchased for the racer that Jonesy had designed for the 1930 National Air Races in Chicago. It had
not actually been raced because of engine trouble.

As soon as the master connecting rod of our Pobjoy engine had pulled apart, we put in a strong complaint to both Nicholas Beasley and the Pobjoy Company in England. It turned out that about a year before, the Pobjoy Company had put out an advisory saying that the connecting rods should be changed but Nicholas Beasley had not forwarded this information on to us. After some haggling it was finally arranged that we would send the engine back to Pobjoy in England and they would rebuild it, using good parts where necessary. This was done and we got it back in time for Fairchild to install it in the W-1A. They charged us only for the labour without the parts, or for the parts without the labour - I forget which. At any rate it cost us about $500 more.

The W-1A, completed and now belonging to the Bureau of Air Commerce, was trucked down to Langley Field in the middle of the spring 1935. Again, the official tests would be made first by the NACA, after which it would be turned over to us for the tests that we wanted to put it through. I have a picture showing it mounted in the 30 x 60'full-scale tunnel at the time of the 10th Annual NACA Conference in May 1935, with all of the eminent engineers and scientists attending the conference seated in a stand below it. At this time, 42 years later, I don't recognize many of the little faces on that photograph, but I do recognize Dr. Ames, Chairman of the NACA,
Edward P. Warner, a member, Igor Sikorsky, Dr. George Lewis, Director of Research, Henry Reed, engineer-in-charge, Elton Miller, my boss, and yours truly. I remember that Orville Wright and of course many other famous people were there, also.

For that conference I had had a model, possibly 18" long, of a tricycle gear made up with regular small rubber tires and a model of a similar-sized tail wheel type gear made up. Both were the gears alone and the center of gravities in approximately the right place, but without the rest of the airplane. I ran these down a track, a sloping track which flared off and allowed the gears to ease onto a concrete floor, as in a landing. Of course, the tail wheel gear turned ever more sharply into a sharp turn and ground looped just about every single time. It was obviously unstable in taxiing. The tricycle gear, however, with its two main wheels behind the center of gravity and its nose wheel castering, would go straight down the runway every time, even if set at a good angle of yaw as it went down the track. As it contacted the ground, it turned immediately with its nose then pointing in the direction it was travelling, not in the direction in which it was originally pointed. Most of the people had not aware of this difference in taxiing stability and it made quite an impression on them. Some people had found, however, that if they took a model of an airplane having a tail wheel and rolled it along a flat surface like a floor, it would turn to one side and ground loop, but if they turned it around and rolled it backward, tail first, it would go straight.
At the NACA Conference a year later Orville Wright made it a point to tell me that although people were saying that the tricycle gear was just a reversion to the 3-wheeled gears they used on the pusher airplanes before World War I, he wanted me to know that he realized that this was not the case. He realized that the airplanes in those days had fixed nose wheels and he said that the people in those days had operated in open grass fields with lots of help and they had no care as to whether the gear was stable in taxiing or not. He wanted me to know that he thought this was a new line of thought and a worthwhile improvement, all of which gave me quite a lift.

Although I have copies of the official NACA wind tunnel and flight tests for the W-1, I seem to have lost or do not have in my possession the official reports for the flight or the wind tunnel tests of the W-1A. As I remember them, however, the wind tunnel tests were just standard ones, measuring the lift, drag and moments. The flight tests measured mainly the performance of the plane, but they did check that both the glide control flap and the two-control operation were feasible to carry out.

After our preliminary practice flights to get used to the new controls, Bob Platt and I, as in the case of the W-1 flights, more or less alternated and made our notes separately and then compared them. The glide control flap, according to
the wind tunnel tests as reported in my NACA Technical Report #552, attained its maximum lift with a deflection of only about 20° and with relatively very low drag coefficient. It could be deflected further up to 80° with no substantial change in the lift coefficient but with an enormous increase in the drag coefficient. The flap was operated by a separate hand lever which had a notch for the 20° flap angle. Coming in for a landing the flap would first be deflected to the 20° flap angle, giving a relatively low drag and flat glide. The airplane was trimmed to fly hands off at a suitable approach speed, say 55 mph. Then, if a steeper glide path was desired, the flap handle would merely be pulled back further to whatever extent desired, and then instantaneous steepening of the glide path would occur because there was no change in the angle of attack or the lift coefficient. Incidentally, the first NACA trials showed that with a flap angle of 80°, the angle of descent and the corresponding vertical velocity were so great as to be alarming, and the flare-off for a landing had to be started at least 50 or 60' above the ground, unless power was applied to help flare out the flight path. The maximum flap angle was therefore reduced to 60°.

In our first trials of the glide control flap, we found the simultaneous operation of the flap lever and the control stick rather awkward, but after a few landings, the operation became easy and apparently natural. After a large number of trials, we found it relatively easy to land
consistently within a few feet of a particular point. We concluded that the type of landing described was entirely satisfactory and practical as a standard procedure with this type of flap arrangement and that although manoeuvring the W-1 having the auxiliary airfoil to land at a given point was not unduly difficult, it was definitely easier, more direct and more satisfactory with the W-1A having the glide control flap. To make approaching a given spot on the field a little easier I had devised a simple sighting instrument. This consisted of a little pendulum hanging just inside the windshield in front of my eyes, with a cross arrow on it showing the glide path angle desired. This gave a flight path referred to the ground rather than to the airplane and in approach the flap angle was merely changed so that the arrow kept pointing right at the spot on the field where contact was desired. This worked fairly well after some practice, but I think Langewirth's system is simpler and better, with which you just keep a spot on the runway that you hope to touch down on lined up with a certain portion of the windshield. If the touchdown spot on the runway goes up with respect to the windshield, you are undershooting. If it goes down, you are overshooting. This is not only very simple, but it takes the wind into account, also.

My first attempts at 2-control flight were made by simply taking my feet off the rudder bar and using the slot-lift ailerons as the sole means of lateral control. After
some turns in the air, I tried making straight up-wind landings in the 50 x 300' strip. The flap was used to control the glide angle. These landings seemed satisfactory, so I tried jogging in by making an approach to one side and an "S" turn just before contact. This also seemed satisfactory and then I tried landing by throttling the engine at an altitude of 400' and making 180° turns to the landing strip. These gave no particular difficulty, so I tried all types with the landing strip crosswind, also without difficulty. I then tried using rudder and elevators only, holding the ailerons in neutral. I found it easy to make reasonably satisfactory turns in the air, but difficult near the ground where the airplane skidded noticeably in entering a turn and slipped downward noticeably when starting to recover. It seemed to wallow around considerably in the turns to a landing at a particular spot. In these trials I could not prevent myself from using the ailerons occasionally to pick up a wing rather than wait for the effect of the rudder. I had done this before Bob Platt started flying the plane in its modified form. With the idea that I had not given the rudder a fair test because I had become accustomed to the ailerons first, I had him try the rudder alone first. After he had made a few flights with all three controls to familiarize himself with the plane, the ailerons were locked in neutral and he put in 2 hours landing in the 50 x 300' strip. He had the same difficulty I had had in skewing around considerably when making landings from turns. I then tried it again, this time with the ailerons locked,
but the results were no better. We both found that straight landings could be made satisfactorily either upwind and crosswind, but that when we attempted curved approaches to the strip, we wallowed around alarmingly and gave the landing gear an opportunity to prove its worth.

We then locked the rudders and tried the ailerons as the sole lateral and directional control. The cables were removed from the rudder bar so that it was used to steer the front. . . .

End of TAPE 8