September 1935 - W-1A continued

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 wheel only. We found that turns were made with less slipping with the rudder locked than with the rudder free. The entrance to turns was made with no substantial slipping or skidding. The steady portion of the turn was always accompanied by a slight inward slip and the recovery seemed quite satisfactory. Both of us agreed that the control was decidedly better with the ailerons alone than with the rudder alone. We made turns and recoveries, and upwind, crosswind and downwind landings with straight, "S" and 180° turn approaches, all with ease and apparently as well as with all three controls. The flight trials were therefore definitely in agreement with the theoretical results found by Jones. For two-control operation the roll-control with the slot-lip ailerons was definitely superior to the rudder control or yaw control with the wing having 5° dihedral to give the rolling motion. We then tried a wheel control in place of the stick, the front landing gear still being steered by means of control the rudder bar. We decided that we liked the wheel at least as well as the stick, if not slightly better. The wheel control, we thought, would make a very satisfactory arrangement if it were connected to steer the front wheel also for 2-control operation.
At this point I think I'll give the conclusions that I gave in an SAE paper that I prepared following these trials. The paper was presented in Detroit in January 1936 and printed in the May 1936 Journal of the SAE.

"In concluding, I might state that we feel that we have demonstrated to our own satisfaction that each of the features described is a help in making flying both simpler and safer. The stable long travel, 3-wheel landing gear arrangement enabled satisfactory landings to be made almost regardless of wind direction, the air speed at contact, or the manner in which the airplane is flared or guided to the ground. Freedom from the danger of stalling was obtained by having the longitudinal stability and the available elevator control so related that the airplane, or at least the outer portions of the wing, could not be stalled. As long as high-lift wings are used, it seems a basic condition that this relation must hold in any completely successful solution of the problem. The glide-control flap is very helpful in enabling an unskilled pilot to approach and to make contact at a desired point with greater accuracy than a good pilot with a conventional airplane. Sufficient improvement in power plant reliability may eliminate the necessity for extra controls of this nature. With the W-1A and its stable landing gear, 2-control operation using the elevator and slot-lip ailerons is quite satisfactory and sufficient for ordinary flying, including making sharp turns, holding a course in gusty air, and making take-offs.
in all directions and landings with respect to the wind, with straight or with curved approaches. It appears possible to us and we hope that the elimination of rudder control in the manner described may be a definite stride toward reducing the skill, training, practice and general keenness required to fly safely."

Incidentally, in that paper I referred to our stable 3-wheel landing gear as "the tricycle type" for the first time, and the "tricycle" name has stuck with it.

That almost ends the story of the W-1 and the W-1A, but not quite. When the Hammond airplane was completed for the Bureau of Air Commerce competition, the nose wheel of its tricycle gear exhibited upon occasion a very severe shimmy. This appeared to be violent enough to shake the nose of the airplane apart in time, and the Bureau of Air Commerce asked the NACA to study the problem and see what we could do about it. We approached the problem mainly from both the theoretical and experimental standpoints. We handed the theoretical problem to Arky, really Arthur Kantrowitz, a mathematical physicist, who had recently started in the variable density tunnel section under Eastman Jacobs. He made some model experiments also and found that a castering wheel with an air-filled rubber tire would also oscillate or shimmy in a purely kinematic way, due to a characteristic mode of distortion of the tire. He also found that shimmy could be prevented by giving the wheel some lateral freedom, that is, allowing it to
slide laterally on a somewhat curved axle, being lower in the center. Although this solution looked simple, it has never been used by designers. Shimmy experiments were also made with the W-1A. During its flight trials, it had shown no difficulty with shimmy, but this was probably due to the fact that it was connected to the control systems and there was sufficient friction to overcome the tendency with the particular castering conditions and tire pressures that we used. The nose wheel was therefore disconnected from the air controls and the shaft lubricated as much as possible and then taxi tests were made in which shimmy did occur. The airplane now, of course, belonged to the Department of Commerce. The shimmy tests were continued with various modifications for some time until eventually the airplane was no longer considered airworthy. Some time after I had left the NACA the airplane was surveyed, which means that a few useful parts may have been taken off of it and the rest of it was destroyed.

During the year 1935 numerous other events occurred, a few of which I'll go back and tell about now.

During 1935 I happened to be on the contest board of the NAA, the National Aeronautic Association. I do not recall who all were on the board at various times, but I do remember that Blanche Noyes was and that Bud Barlow was. Bud was later Dean of Engineering at Texas A & M College and I was associated with him at that time. The Board decided that
something should be done about starting an event at the National Air Races that would stimulate interest in private flying. I was assigned the job of working out such an event. Being an engineer, I naturally came up with a sort of performance contest. It consisted of taking off over a barrier, a light harmless string with a few flags draped on it, landing over the barrier and stopping in the shortest possible distance, a speed run in both directions over a 3 kilometre course, and a 150 mile cruising run with credit being given for speed and for load carried and debit being given for the fuel consumed.

The National Air Races in 1935 were to be held from August 30 to September 2 in Cleveland. We of the contest board did not want to clutter up 4 very active days of racing at Cleveland with the many tests that would be required for my contest. We therefore arranged to run all of the contests except the cruising contest on a little-used field about 150 miles away from Cleveland and then run the cruising contest from that place to Cleveland. We selected Wayne County Airport west of Detroit, which at that time was about a mile square grass field with only a couple of hangars on it and no runways. It was in open farm country and an ideal place to run the tests. (At the present time it is surrounded and has been expanded into the Metropolitan Airport of Detroit.) The trials at Wayne County were run for 2 or 3 days before the National Air Races started at Cleveland. For a
couple of weeks before that my family and I vacationed at the cottage of Dorothy's folks at the National Music Camp at Interlochen, Michigan. There, sitting out in the open in front of the cottage with a card table and a view of Lake Wabakaness in front of me, I spent many hours preparing the numerous final details for the contest.

The contest, I should mention, had by this time become sponsored by the Detroit News, which offered a fine trophy to become the permanent possession of anyone who won it 3 times. Five airplanes arrived at Wayne County Airport to engage in the contest and they covered quite a wide range. The largest was a Beech stagger-wing biplane with I believe a 300 hp Wright-Whirlwind engine. Next was a Cessna Airmaster, a cantilever monoplane with a 145 hp Warner engine. Then a 2-place Monocoupe with a 110 hp Warner engine, a low wing wire-braced Ryan ST with a Menasco engine, and a Piper Cub with a 3-cylinder Lenape engine of possibly 40 hp. With those airplanes I met a number of fine people, some of whom I have had contact with now and then since. That was the first time I had met Dwayne Wallace, then chief engineer of Cessna, who had with him George Hart, an operator in the Wichita area, to fly the Airmaster for him. Helen McCloskey, the attractive daughter of General McCloskey, had the Monocoupe there and Jack Morrison of Pittsburgh was with her to fly it for her. Howard Rough was there, as the Bureau of Air Commerce inspector who checked over the airplanes for us and made sure that they
were in conformity with the regulations. I don't know for certain that that was the first time that he had met Helen McCloskey but at any rate they were married later. The last time I saw them was at the 50th anniversary of the first flight of the Wright Brothers at Kitty Hawk. We had lunch together at that time. They had a summer cottage in the Nags Head area.

The pilot of the Ryan ST was Tex Rankin who, about that time, was the aerobatic champion of the country. He, incidentally, was so conservative in his take-off and landings over the obstacle that he could not make a competitive score. After the take-offs, landings and speed trials had been made individually at Wayne County Airport, we all started at about the same time on the cruising contest. We had originally intended to land directly at the races in Cleveland, but while we were at Wayne County we received word that there was no suitable open spot for that occasion. We therefore ran the cruising contest to the Akron airport where we could take time to measure the fuel consumption accurately. The next morning early we all hopped over to the Cleveland airport at about the same time. Later that day a combined event to show what had been going on was held at the main Cleveland races in front of the grandstand. In this event the planes all lined up and made a race horse start over a 10' obstacle, then headed for a scattering pylon and made 2 laps around a 5 mile course, all within sight of the grandstand, after which each landed over the 10' obstacle and came to a stop in as short a distance as
possible. Throughout the entire contest Dwayne Wallace had tried very hard, always in a considerate and sportsmanlike manner. He left no stone unturned and his Airmaster got the highest number of points for the contest and won the trophy for that year.

Incidentally, while I was vacationing at Interlochen before these races a glider or soaring meet was going on near Frankfurt, Michigan where they had 400' high bluffs, adjacent to Lake Michigan. With the usual west wind, the conditions were very favourable for ridge soaring. The gliders took off by winch tow, from the beach below the bluff and when they got about halfway up the elevation of the bluff, they could cast off and soar very nicely. A contestant by the name of Stan Smith had a glider there, a 2-place glider, called The City of Utica and he wanted to make both endurance and altitude records and he needed an observer. Although a number of people would have liked the job, I was fortunate to be given it. While we were soaring above the ridge, there were about a dozen gliders soaring back and forth most all the time. Stan Smith managed to get The City of Utica up to a little more than 1000' above the take-off or release point and then it was easy to stay up for the required hour.

In 1935 a publication was initiated somewhat similar to "Who's Who in America", but for men under 40. It was called "America's Young Men" and I was fortunate enough to be included in it.
An event which occurred in 1935 might be called "Rigid Military Security". Don Wood and I were at Wright Field in Dayton regarding some possible propeller research that might be done in the 20' propeller research tunnel. A couple of the officers asked to talk with me regarding some possible research in the full-scale 30' x 60' tunnel. They said that the Air Force was interested in a very large 4-engine bomber that might be built by Boeing. They wondered how large a model could be tested in the full-scale tunnel and whether it could be tested with 4 power plants all driving propellers so they could get power-on tests as well as power-off. I told them that the model could have a span of at least 30' and possibly 35, and that it would be entirely practical to run it with 4 propellers operating. They said that the project was very secret but that I should inform our NACA officials, particularly Dr. Lewis, when I got back the next day and they would make contact officially later on. I did this and in a few weeks the official project came through.

From that time on, only the minimum of officialdom must know about the project: that meant Dr. Lewis, Henry Reid, the Engineer-in-Charge, Elton Miller, Chief of the Aerodynamics Division, and it went from directly from him to Smitty De France, who had charge of the full-scale tunnel, and the few people in his section who had to know about the project. I, being only Assistant Chief of the Aerodynamics Division, did not need to know the project and was not supposed thereafter to know anything
about it. In fact, I and all of the rest of the people at the laboratory were to consider the full-scale tunnel off-limits. This seemed a little odd to those of us who knew that I had had the first contact on the project, but those were the orders as the Air Force desired them and so they were carried out so far as possible.

One day Edward P. Warner, who was at that time if I remember correctly Assistant Secretary of the Navy for Air, came down from Washington to show the laboratory to a distinguished foreign visitor, a Mrs. Sturtevant from Germany. She was in this country ostensibly to sell German airliners to American airlines. The scuttlebutt that went around indicated that she was possibly a spy. So great care was to be taken to show her only things that did not matter. For a portion of their visit I showed them around the laboratory. Then Henry Reed and I had lunch with them at the Officers' Club. There, it turned out that many of the officers knew her socially, and called her by her first name (which I have forgotten). Some asked her if she was going down to the air manoeuvres in Miami next month and she said she probably would. When we left the Officers' Club in a car to go to another part of the lab, about an eighth of a mile away, too far to read any signs, there were two flatcars on a railroad siding, each with a large box on top of it. Mrs. Sturtevant piped right up, "Oh, there is the model of the 4-engine Boeing bomber than you people are going to test in the full-scale tunnel shortly." Security is a marvelous thing.
The model was of the B-15, the first large 4-engine bomber of the services. Only one airplane was made of this model, but a somewhat smaller version, the B-17, saw great service later on in World War II. On another occasion during this same general period, Ed Warner had another visitor down at the laboratory, this time an American lady, Amelia Earhart. Again, I spent some time with them around the laboratory and when Ed Warner was called away by a phone call or something, I had a for an hour or so very pleasant chat with her/outside the propeller research tunnel, sitting on something or other, I don't remember just what. She was a very pleasant, outgoing person and easy to talk with, about flying at any rate.

During this same general period of time, a young man by the name of Doolittle, flew a little airplane called the Arup from Indiana to Langley Field for the NACA to run a few tests, on. This man turned out to be the nephew of Jimmy Doolittle. The airplane was a very unusual all-wing design with a very thick, very low aspect ratio wing. The leading edge was straight, but the rest of the wing in plan form was half of a circle toward the rear. It was powered, as I remember it, by a Continental A-40 engine delivering about 37 hp. Small flaps at the center in the rear were used as elevators and small flaps farther out toward the tip were used as ailerons. The wing was so thick that the pilot was entirely enclosed within it, in a supine attitude, except that his head projected above
it from a small open cockpit. He had no view at all downward and to the sides, but had a fair field of view forward. As Charlie Zimmerman's wind tunnel tests had shown on low aspect ratio wings the lift coefficient was quite high, but it occurred at such a high angle of attack that it could not be used in take-off. In landing, the plane squatted down at a steep angle and rolled only 50 or 70' to a stop. A very interesting design, but it was not quite competitive with the conventional configuration.

I believe it was in 1936, possibly 1935, that Captain Frank Courtney, a noted pilot of large seaplanes and autogyros from England, visited the NACA laboratories. He was designing a small amphibian for Curtiss-Wright and came to get pertinent information for his design. He had heard about and aimed to use our tricycle gear and I gave him all the information we had on it. I remember that he had lunch at our house and established a record: he drank more cups of tea than anyone had ever had in the Weick household. The amphibian was designed and one was constructed. It appeared to be a good design, but none were produced. Frank Courtney then went back to England where I saw him a year or two later.

Another man from England who visited the laboratory, first in 1930 and again in '35 or '36 was Dr. G. Lachmann, who had invented the wing slot in Germany at about the same time that Handley Paige had invented it in England. They
had gotten together and Lachmann was now Handley Paige's chief of design in England. During his second visit, he was particularly interested in the design of a tailless airplane. He was disappointed in my view that you could get a higher performance, that is, a greater range of speed from minimum speed to maximum speed from a design with a tail than from a design without one. The reason for this is that the highest lift coefficients so far have been obtained with flaps at the trailing edge that deflect downward, whereas tailless airplanes need a certain amount of upward deflection at the trailing edge in order to have longitudinal stability. On this account, the total area of wing plus tail with a conventional flapped airplane is less than the total wing area of a tailless airplane and the skin friction drag is therefore less.

One interesting little incident occurred while Dr. Lachmann was having a meal at our house. Dorothy told him that I had mentioned his name to her from time to time. He immediately stood up straight and bowed to her from the hips, keeping his back straight. It's odd, the little things that make an impression on us, isn't it?

During the period from 1930 through '36, our NACA research, both wind tunnel and flight, on lateral controls, high lift devices and flight characteristics, had continued to roll on. The wind tunnel research in the 7 x 10' tunnel comparing lateral control devices, particularly at high angles
of attack, had been completed with 13 separate reports. These were:

1. Ordinary ailerons on rectangular wings (Report 419)
2. Slotted ailerons and Frieze ailerons (Report 422)
3. Ordinary ailerons rigged up 10° when neutral (Report 423)
4. Floating tip ailerons on rectangular wings (Report 424)
5. Spoiler and ailerons on rectangular wings (Report 439)
6. Skewed ailerons on rectangular wings (Report 444)
7. Handley Page tip and full-span slots with ailerons and spoilers (Technical Note 443)
8. Straight and skewed ailerons on wings with rounded tips (Technical Note 445)
9. Tapered wings with ordinary ailerons (Technical Note 449)
10. Various control devices on a wing with a fixed auxiliary airfoil (Technical Note 451)

End of Side 1

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11. Various floating tip ailerons on both rectangular and tapered wings (Technical Note 458)
12. Upper surface ailerons on wings with split flaps (Report 499)
13. Auxiliary airfoils used as external ailerons (Report 510)

Each of these reports was written by me and one of the other men in the atmospheric wind tunnel section who had been in charge of the testing on the project. During this project Jones and I
prepared a report on the effect of lateral controls in producing motion of an airplane as computed from wind tunnel data, which was published as Report 570. Then finally we produced a report, resume and analysis of NACA lateral control research, Report 605.

I see it's too boring to go into this much detail.

During this same period, 1930 through 1936, we made about a dozen investigations in the 7x10' wind tunnel of high lift devices of various kinds. These included automatic and fixed leading edge slots, and fixed slots throughout various portions of the cord. Also, various shapes and sizes of fixed auxiliary airfoils located ahead of the leading edge of the wing. Also plane flaps, split flaps, split flaps moved to the rear, slotted flaps and slotted flaps moved to the rear, such as Fowler flaps. There were also 3 more investigations on lateral control, one on the effect of length of Handley Paige tip slots on the lateral stability factor, damping in roll. In addition, some of the wind tunnel lateral control findings were checked in flight and the effect of lift, drag and spinning characteristics of sharp leading edges on airplane wings was checked in wind tunnel and flight.

In the work mentioned up to here I was author or co-author of the report. There was of course, much additional work under my program carried on that was reported individually by the members who did the work themselves. For example, Charlie Zimmerman produced two stability reports which were very useful
to designers: one on longitudinal stability and one on lateral stability. These included dynamic stability as well as static stability. The work on spinning went on continually, both in the wind tunnel and in flight.

The 5' vertical tunnel in the Atmospheric Tunnel in Section, which the forces and moments were measured with the airplane in a spin, did not produce very useful results. We then learned that the English had devised a free-spinning tunnel, I believe about 12' in diameter, with an upgoing vertical stream of air in which the models could be spun freely. The recovery from the spin with various control deflections could be studied satisfactorily. We then built one at the Langley lab under the direction of Charlie Zimmerman. All of this work on stability, control, flight characteristics, spinning and high lift devices of course represented but a small portion of the total work of the laboratory.

During this period, Eastman Jacobs and his men in the Variable Density Tunnel Section had been carrying out a large systematic series of airfoil tests. The airstream in the Variable Density Tunnel happened to be highly turbulent and he worked on means for correcting the airfoil results for this turbulence. This led to the design of low-turbulence wind tunnels, giving results similar to those in free air. Also, to the study of the effect of pressure distribution over the airfoil and the boundary layer, and it led then to the development of the low-drag laminar flow type of airfoil,
now widely used. Also, during this period Dr. Theodore Theodorson, a theoretical physicist, had been added to the staff, in charge of the Physics Division. The theoretical output of the laboratory increased at once, particularly in regard to propellers and engine cooling with complete cowling.

The S.A.E. paper that I had given in January 1936 on our W-1 and W-1A projects had received fairly wide public notice. It had been published in other countries and translated into different languages. I had originally entitled it simply "A development toward simpler flying". The S.A.E. people suggested changing this to "Everyman's airplane: A move toward simpler flying". I thought this rather corny but reluctantly agreed to it, and I suppose the new title accounts for a fair amount of the public reception it received. The aeronautical press started referring to it in many cases as "a foolproof airplane". This I objected to strenuously because I knew that there was no such thing as a foolproof airplane and I didn't want our work associated with one. I have since learned definitely that you can make an airplane that is easy to fly safely, but the ultimate safety depends on the operation by the pilot.

In early October, I believe it was, Henry Berliner invited me to join his company, the Engineering and Research Corporation in Washington, D.C. as Chief Engineer. My main project would be to put the ideas we had developed in the W-1
and the W-1A into an airplane to be put into commercial production for private use. Although I would have been glad to stay with the NACA and carry on research work for the rest of my life, I could not resist the opportunity. On December 1, 1936, I started with the Engineering and Research Corporation and our family moved up to Washington. Actually, we rented a house in the suburb of Bethesda, Maryland, at 408 Old Georgetown Road. All of the shares of the Engineering and Research Corporation were owned by Henry Berliner himself, except for a few held by members of his family. He then had complete control. At the time I started there were 85 employees working in a factory at 6100 Sligo Mill Road, Washington, D.C. This was just inside the N.E. District of Columbia line. The building was rented.

At that time the products consisted entirely of machines for making airplanes. These were propeller profiling machines, sheet metal flanging machines, sheet metal stretch presses, and automatic punching and riveting machines. Henry was President and Chairman of the Board. Les Wells was General Manager; Milton King, Henry's brother-in-law, was Secretary and Legal Advisor, and Bill Carroll was Treasurer. There were three men in Engineering. One was Lee Marchant, who had invented the automatic punching and riveting machines and did the engineering work on them. For the general engineering there were Frank Lane, at that
time Frank Levy, and a Spaniard by the name of Alfero. Both of these men had done some previous aircraft design work.

I started immediately on the design of our new plane, which had not yet been named, but it was carried out under Job #310 so we called it Model #310. And Bob Sanders, who had been one of our original group at Langley, came back from his job with Pan American-Grace in Peru, to join our group.

My original idea was that a private airplane should take a person from one small field to another small field, in a short time at good speed, and with economy and ease and comfort. Henry's idea, however, differed from mine. He had had a couple of airplanes of his own and had used them for his private transportation. He had a cottage on Rehoboth Beach, Delaware, on the Atlantic and flew back and forth from Washington to Rehoboth Beach during the summer months. He could have landed on the beach, close to his cottage, but he did not make a practice of this because he could not get service there and he thought that in general the owners of private airplanes would want to land where they could get service and where the airplane would be taken care of when they left it. He therefore thought that we should design it so that it could get in and out of the smallest ordinary airports, but not for extremely small places that would be used very seldom. A smaller wing could then be used and extra
speed obtained for cruising flight. After some deliberation I went along with this idea and I have been glad ever since that I did.

Henry also thought that it would be much easier to sell a tractor airplane than a pusher, which had the disadvantage of having the engine behind the people in a crash. It would be much more difficult to get satisfactory effective limited elevator control, both power on and power off with a tractor than a pusher, but I agreed to try with the understanding that if we did not make it with the tractor, we would go back to the pusher design. The tractor also had the disadvantage that the tricycle gear would not be so effective, because to get the wheel base we managed to get with the W-1, the nose wheel would have to be out in front of the propeller, and this could not be done structurally. This has been a disadvantage of all tractor single-engine airplanes. At this point, I believe I can say that I have never seen a tricycle gear on a production airplane that is as effective as that of the W-1 and W-1A in making a wide variety of types of landings, both easily and safely. The full-size nose wheel was far out ahead; the gear had 12" of shock absorber, and it was designed much stronger than was thought necessary. We could afford to go overboard in this direction because with the pusher arrangement the balance was helped by a little extra weight in the nose and with the 18" of shock absorber travel for the rear wheels and with a smooth landing surface, I believe that a safe landing could be made
almost regardless of the manner in which the airplane was brought to the ground, as long as the vertical velocity was not in excess of about 20' per second. With the wing tip clearance that we had with our high wing arrangement and 5° of dihedral, we made satisfactory landings while still turning with a 30° bank and the airplane merely straightened out by itself. Of course with a tractor arrangement, it is almost impossible to get the full potential of the gear.

Time out for another set of races, this time in Miami in the middle of December 1936. The second run of the Detroit Trophy Contest was to be run at the Miami Races, and I had previously agreed to be there for them, so shortly after I started at ERCO, I had to leave to go down to Miami. A friend of mine by the name of Kirschbaum from the NACA Langley lab was there to help me this time. The races were run at the main Miami airport which at that time was at the location of the present Opalaca airport, north of Miami. The schedule was not so tight but that we ran all of the tests right on the field, except for the cruising contest. The Beech stagger-wing and Cessna Airmaster were the same as the year before, but the other planes, a half a dozen of them, were all different.

I knew almost nothing about Florida, so for the cruising contest, I asked the local people where there was a good landmark that could be used as a pylon about 75 miles
away, so we could make our 150 mile run by going there and back again. The Red Lighthouse at Jupiter Inlet on the east coast about 75 miles north of Miami was suggested and it looked good to me, so I drew a straight line on the map from the then Miami airport (Opalaka now) to the Jupiter Lighthouse and figured we'd go there and back. When the Bureau of Air Commerce inspectors saw this, however, they objected and would not let us use it. They said that if anyone went down on that course, it was almost all soft swamp and he might never be found again. They made us fly straight east to the coast and up the beach to the lighthouse and then dogleg back the same way. Now, 40 years later, that entire area is drained and built up with business buildings, factories, and residences. At the Miami races Dwayne Wallace flew the Airmaster himself and again he won the event. Later he got permanent possession of the Detroit News Trophy and it has been on display in the entry to the main office of the Cessna plant for many, many years.

Back at the ERCO plant, I guess I forgot to mention that the trade name for the Engineering and Research Corporation products was ERCO, or "Erco". Back at the Erco plant we had crystallized some of our ideas. Summarizing, the aim of the new design was to make it particularly suitable for the private owner by having it unusually simple and easy to fly, quick to learn to fly and free from the difficulties associated with stalling and spinning. It was also to have a
good field of view for the pilot and a cruising speed of 100 mph with a low-powered engine.

The general arrangement of the new airplane was more or less obvious to me from the start. In order to have the limited elevator control effective both power on and power off it was desirable to have the propeller thrust line as high as possible and the wing drag in as low a position as possible. With power on at low speeds, the tractor arrangement gives an extra tail-depressing reaction for two reasons: the extra slipstream velocity gives an extra downwash over the center part of the wing and the extra velocity over the elevator tends to depress the tail down further. Thus, a low-wing arrangement was dictated with the propeller axis located as high as possible, still giving the pilot a good view over the nose. The pilot's field of view was helped by using a flat or an inverted engine rather than a radial, because then there was very little engine structure above the propeller axis itself. I, in effect, raised the thrust line still higher by inclining the engine down at the nose so that the line of thrust had a larger moment arm about the center of gravity of the airplane.

In selecting the airfoil section for the wing I went over all the latest work of East Jacobs and his cohorts in the Variable Density Tunnel and by cross-plotting, came up with the NACA 43013 airfoil as optimum for our conditions at
at that time. This enabled the approximate wing area to be determined and a series of performance computations. Climb performance computations with different spans led to the selection of 30' as the span. I wanted an engine of about 60 or 65 hp but none was available at that time, so we started designing the first experimental airplane to use the available Continental A-40 engine which really delivered about 37 hp, to be used only until a more powerful engine could be obtained.

Bob Platt in the meantime had been transferred to the Washington office of the NACA and I used him nights and weekends to make stability and control computations, determining the size of the stabilizer and elevator and the vertical tail surfaces.

Erco had sold many of its machine tools for the construction of aircraft to most of the important countries of Europe, but in Russia and Germany they could not receive payment that could be transferred into American dollars, but had to make some sort of trade arrangement. In Germany, they got the right to make Schwarz propellers in this country. They were being manufactured in large quantities by the Schwarz Company in Germany and also by Airscrews Ltd in England, which had a licence there. The Schwarz type of blade consisted of essentially a main core of laminated spruce
or other light wood, which merged into a root of impregnated and compressed hardwood that we called "Compreg". The compreg root was threaded and screwed into a steel Farrell which supported the blade in the hub. The remainder of the blade was covered completely with a heavy coating of reinforced cellulose acetate sheet, the leading edge also being armoured with a flush strip of metal. Thus, the wood core was well protected against climatic conditions and warping, as well as against injury due to contact with pebbles, cinders, sand, rain, hail, seawater, and so on. These propellers are described in detail in an S.A.E. paper that I gave called "Composite Wood and Plastic Propeller Blades" and printed in the S.A.E. Journal for June 1939. Their main advantage lay in their light weight, as compared with aluminum alloy propellers in the large and very large sizes which were being used with the most powerful of the reciprocating engines at that time. In production they were finally replaced by hollow steel blades.

Henry wanted me to go over to England and Germany so that I could learn as much as possible about the design of the propellers and their manufacture so that Erco could manufacture them in this country.

In England I spent 2 or 3 days visiting Airscrews Ltd and was very happy to meet Dr. Henry C. Watts, England's
foremost propeller authority and a director, really the technical director, of Airscrews Ltd. He had written a book called "The Design of Screw Propellers for Aircraft" which was published 10 years before my book "Aircraft Propeller Design" and of course in that time it became obsolete, as mine really was 10 years after it was published. Dr. Watts took me home to his house for dinner one evening. He recently had built a large 3-storied brick house with about 15 rooms in it. It was on a good-sized lot with nicely arranged landscaping and Dr. Watts and Mrs. Watts took me on a tour to show me the outside surroundings. Although this was wintertime, the grass was fairly green and there was green shrubbery all around the edges. As we rounded the house to the rear, I ventured "Your back yard is very attractive". Mrs. Watts came back instantly with "Our rear garden is made up of . . ." and so forth and so forth. I was obviously just a bumpkin from the Middle West.

At that time, most of the houses in England were heated by fireplaces. Of course the English winters were not very severe and the people dressed very warmly with wool underwear and wool suits, but Dr. Watts was very proud of the fact that he had a central heating system in his house, which he wanted to show me. While going through the first floor of the house, with its living room, dining room and kitchen, we went from the kitchen into a little sort of pantry with shelves, and a small coal stove about 4' high,
with a little coal scuttle and a small hand shovel in it
standing next to the stove. I noticed a couple of small
pipes leading from it and was just about to say "Oh, I see
this is your hot water heater" when I suddenly thought and
stopped, and just in time, for this was the central heating
plant for the 15-room house. I noticed later that after
dinner we stayed close to the fireplace in the living room
and in the propeller plant in Weybridge I wore a topcoat most
of the time.

I flew from London to Berlin on the KLM Dutch airplanes
with a stop at Amsterdam. The good old Douglas DC-3's had
just come out and this was my first ride on one. The stop
at Amsterdam was long enough so that I got a quick look at
the Fokker factory, which was on the airport.

In Berlin, on Henry's recommendation, I stayed at one
of the better hotels, the Adlon, which was on the main
boulevard in the city, Unter den Linden. It was a quaint old
hotel with good service. It had an open elevator going up
from the lobby, the passengers being protected by fancy iron
grillwork. The hotel was destroyed later by our bombing
during World War II. Now I believe the Russians have that
part of the city.
The Schwarz propeller plant was in a suburb north of Berlin, which I called Oranianberg. I never could get the guttural German pronunciation while I was there. I spent the working days of about two weeks in the plant. It was easy enough getting along with the engineers because they could speak English, but in the shop only a few people could. However, with my two years of German in high school and with a week's practice with a young German on the ship coming over, and with a small English-German dictionary and with a lot of hand-waving and sketches, I got along fairly well there too.

One of the first things I did after arriving in Berlin was to look up our United States Air Corps attaché. I believe his name was Captain Kaiser. He arranged for me to see a couple of the aircraft plants nearby and I wanted particularly if possible to see the D.V.L., the equivalent of the NACA aeronautical laboratory at Langley, but directed in a more military aspect. He said almost immediately, "Oh, I'm sorry, that's impossible. That's out of the question because it takes at least 6 weeks of preparatory paper work." I asked him to give them a ring and try it anyway, even if he got turned down. Of course, he didn't want to do this.

End of TAPE 9 (Side 2)