OPEN FORUM

SELF-ADAPTIVE CONTROLS

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PANEL: Dr. J. Aseltine, Space Technology Laboratories

Mr. R. Bretoi, Minneapolis-Honeywell Regulator Company

Mr. E. R. Buxton, Autonetics Division

Mr. M. Dandois, Convair, Fort Worth

Mr. M. Marx, General Electric Company

Mr. S. S. Osder, Sperry Gyroscope Company

Captain R. R. Rath, Wright Air Development Center

Dr. J. G. Truxal, Brooklyn Polytechnic Institute

Mr. W. P. Whitaker, Massachusetts Institute of Technology

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Dr. Li: Gentlemen, this is a panel discussion. Several of the members have not made presentations; therefore, each of them will be allowed three minutes to express his views. Rather than limit individual presentations, the three minutes will be taken as a statistical average time for the speakers to give their point of view about the subject matter. After the presentations, anyone who has a question, please stand. You will then be recognized by the chairman and the military guides will bring you one of the four microphones located on the floor. Please state your name and association before asking your question. I will ask one or more panel members to answer. I will now ask Captain Rath to start this panel discussion.

Captain Rath: Thank you, Doctor Li. I think most people are familiar with my viewpoint on adaptive systems. I have been with the program for three years now and I would like to feel that the present state of the art is such that these adaptive systems have demonstrated that there are adaptive techniques which are capable of being utilized by control system manufacturers to provide a control system which gives us the desired response throughout the flight regime. The using people, airplane manufacturers, the Air Force, and so forth, should have enough confidence in the capabilities of adaptive techniques so that they will use them in their airplanes. They will not have to go through a long component development, because, after all, the mechanization of adaptive techniques, as pointed out in the last few days does not involve anything radically new. It is only the idea that is new.

We are still using feedback techniques. We may have come to a point where we are using a more sophisticated type of feedback technique, but it doesn't require anything new in the way of product development to utilize them at the present time. I personally hope that in the very near future these types of techniques are utilized to give control systems the capability that they should have at the present time. Thank you.

Doctor Li: Thank you Capt Rath. Doctor Truxal.

Doctor Truxal: This will be the low side of the statistical three minutes. My only reaction is, I think this is a very interesting, enthusing, and exciting way to look at the design of the feedback system, to try to learn how we might design systems that we could not have designed before. As a teacher who competes with my colleagues for graduate students for research contracts, it is real nice to have something to go into the students and talk about that will compete with plasma electronics and some of these other glamour terms. I think this is a wonderful development.

Dr. Li: Thank you, Dr. Truxal.

Dr. Aseltine: I think I have already said pretty much what I wanted to here, but let me say it again just to emphasize it a little. I feel rather strongly that the term adaptive ought to be defined. I have a fear that we are tending to define it in such a manner that it becomes all inclusive and I really don't think that this is of any particular help to those of us who are working in the field. I would like to tie it down a little bit more. As far as what is going to happen in the future, it is a little harder for me to comment on this. I don't see any immediate remarkable progress in sight. In fact, we seem to be moving rather slowly toward an objective which is yet to be clearly defined. I do feel that work should be done on basic limitations. Once we know what we want to do, we ought to be able to say more clearly just what we <u>can</u> do. The things that I have in mind here are considerations wuch as the time necessary to measure a system performance before anything can be done. I suspect maybe there is an uncertainty principle operating in an adaptive system which changes the performance to do a task and must use some finite time to do this. I think these limitations must be tied down. I don't see why theoretical work in this direction cannot be done. These two points, definition, and some way to bound the performance of an ideal adaptive system have a particular interest for me. Thank you.

Dr. Li: Thank you, Dr. Aseltine; Mr. Bretoi.

Mr. Bretoi: There are two or three aspects of this conference that have interested me particularly. One of them relates to the interest and attitude of the aviation industry toward adaptive systems and non-linear techniques. One of the big stumbling blocks which many of the control engineers have - I shouldn't say stumbling block, it is more of a mental block really, relates to their hesitance to consider non-linear systems. In my past experience there have been many engineers who were reluctant to consider something other than a linear system because they tended to assume that a linear system was, indeed, the best way of accomplishing a control mission. Probably this stems from the fact that the non-linear parts of the control system are those parts which have given them very much trouble in their past experiences. For example, such things as control dead-spots, thresholds and so on. However, more recently I have gotten the impression that we are adopting the philosophy of Christine Jorgensen that if you can't fight it, join it. Well, somehow, I feel that we are making a rapid rate of progress now in the control field. There was much interest in automatic controls and a rapid build up of effort about ten or fifteen years ago and in the early phases of this effort there was rapid progress being made. I don't know exactly the rate of progress but it has slowed down in the past few years. Somehow, I feel that we are just about to begin a period in our control development techniques and so on, where we will probably have another rapid rate of progress. One of the significant aspects of adaptive controls, among many of the other mentioned here, relates to the fact that now we are at a point where we can achieve the desired performance goals more conveniently and more easily than in the past through the elimination of dependence on accurate dynamic data and air data scheduling. I think the significance of our ability to eliminate the dependence on these quantities is that we can start concentrating on design criteria relating more along the line of such things as reliability and simplicity of mechanization. As for future efforts, I think that Dr. Li touched on those very nicely - I would just like to mention that I think some of our future programs should be conducted, or at least people should be thinking seriously about, what kind of performance we do actually want and what kind of criteria we should use to get this performance.

An effort should be directed to study the effects of such things as noise sources, parathetic and non-linear effects, and control of higher order systems. We have been concentrating pretty heavily on the lower order systems and assuming we can separate second order roots from the influences of the other system roots. However, when we try to control vehicles of higher order that have roots which are more nearly equal to each other in some applications, we will have problems. I think that we will be encountering vehicles which will require this consideration. The design trend is in a direction where the structural modes of the aircrafts have natural frequencies which are near the control modes. So that we are not able conveniently to separate the roots of the systems.

Dr. Li: Thank you Mr. Bretoi, Mr. Buxton.

Mr. Buxton: So much has already been said that I'm afraid I can add very little to the ideas that have already been presented. I am on this panel as a sort of consolation prize. We did submit a paper; however, Capt Rath looked it over and thought it was fine but that it covered some areas that were already being covered and so here I am. As a matter of fact, though, it is very interesting point that the systems that we have been working with represented actually a combination of two or three of the systems that were presented. We find after looking at a great many of these systems that there are possibilities of combining to an advantage a number of the principles of the basic techniques or solutions, which recently have been presented. If one is ruthlessly objective, he is usually identified as a negative thinker. There has been much discussion regarding the advantages of these systems, but there has been little said about the disadvantages that might accompany a few of the techniques involved. I am sure we will get into this area on questions from the floor, but I would like to add one more advantage that nobody has yet mentioned and that is the auto-pilot salesman must recognize that these adaptive gimmicks are the best sales pitch in twenty years.

Dr. Li: Thank you very much, Mr. Buxton. Are there any questions from the floor:

Question from the Floor: My name is William O'Neil, Douglas Aircraft Company. Instead of putting these in the form of a question I will put these in for form of a statement to be commented on. First of all, without disrespect to the speakers or the quality of the papers, I feel a good deal of what has been said here in the past few days has not dealt with the fundamental theories of adaptation, such as the general process information required, and the solution time which must occur after acquisition of information before action is taken by the control system. In other words, the really basic items which we need to design any adaptive system, not just an adaptive airplane control system. So I would state that I feel that investigation of fundamentals is important and necessary. That is item one. Item two, no one has agreed to mention the effect of changes internal to the control system. We allow the gain, aerodynamic or otherwise external to the control system,to vary by large factors, ten, one hundred, a thousand; occasionally this happens inside too! A complete adaptive control system program should look at every element of the system as being the same in this sense. Every element can have a variable gain, including, by the way, the germanistic models which we built. The inputs should also be considered. That is item two.

Item three is people want or have given definitions of adaptation and I would like to break them down - one is program adaptation such as we have in present flight control systems; the second is logic adaptation including non-linearity, attempting to achieve some specified condition; and the third, a random method which has been discussed mainly by Ashby. I think the general theory fundamentals that I discussed should be emphasized more strongly regarding all three methods in the future.

Dr. Li: Thank you, Mr. O'Neil. I wonder whether Captain Rath would like to comment on these statements?

Captain Rath: Well, I am the first to agree with Mr. O'Neil that more work in the basic theory is required. However, we started out with the idea that we wanted to get techniques that could be applied immediately and then after we had demonstrated that adaptive controls are feasible, to support these techniques with the basic theory so that you can actually synthesize systems rather than build them on an experimental basis using the empherical data that we had gotten from experiments. I think a review of the work that we in the Flight Control Laboratory have planned would show that we are going to do more basic research. There have been questions raised about whether our plans are really for basic research. The best I can say is this. We welcome having people or organizations that feel that they have legitimate approaches that should be pursued to send in proposals to us. We intend to evaluate these fairly and try to prevent duplication of effort, if possible. Then, based on what comes out of that, those that can be funded are funded, so I would say then that if you have proposals that you think will help us, please send them in.

Dr. Aseltine: I hate to let Ashby get by without taking a crack at him. The homeostat that Ashby built is perhaps a pioneering step in a long range direction; however, there are a couple of things about it that I think perhaps explain why it has not been applied on the programs that we have been hearing about. In the first place this is a device which searches in a random way for a stable operating condition and in general the steps that it goes through in order to get to a stable condition may lead it violently unstable for a sizeable time. This is one of the problems that Ashby was worried about and he does have some solutions, but the system may be badly behaved and objectively so if it were an airplane. The other point about Ashby's system is that it serves no useful function and I think I am quoting him almost directly, " Except to run to a state of equilibrium, it demonstrates purposeful behavior but it does not do anything else" . I have thought a lot about how you could make it do something else, I'm not deprecating this, I think it is a device that is nice to look at and it is a nice device to get ideas from but we shouldn't be mistaken in thinking that it can be put in an airplane, yet. I agree that this is a good area to look at.

Dr. Truxal: To go back to the comment on the program itself, the basic theoretical questions that are involved here are exceedingly diffficult. I think if you look at other fields of science you don't find general theories on engineering that are built up early in the game. We didn't have a feedback theory until we had a lot of feedback systems operating. We didn't have communication theories until after a lot of radios were operating. It seems to me we need a lot of different types of adaptive systems operating before the poor theorist can decide what the theory ought to provide. You can't go to a university person or a basic research person and ask hom to develop a general theory on the grounds we have so far established. That doesn't mean that we shouldn't work on it but I think this will be a longer time coming than the applications of these techniques.

Mr. Whitaker: I might only say that in answer to the second part of your question certainly no one believes more in Murphy's law of random perversity than I do. Anything that can go wrong will go wrong, but I didn't gather from any of the presentations that anyone was excluding the failure inside the loop either. Particularly, if you have a measurable criteria performance of some kind then no matter what is upsetting the performance then presumably that index is going to change. You aren't limited just to environmental changes.

I might also comment on the third one. There has been some philosophy that we could make an aircraft system completely self organizing; however, I feel that this requires too much predetermined mechanics and it seems to me this is unwarranted complexity. I think in most cases you definitely know what you want the airplane to do. To make the system decide what its loop configuration is is unwarranted complexity.

Mr. Buxton: I might comment on the first one. Mr. Barron from Dodco gave a very interesting and very fundamental approach I thought in some of the basic principles. Variational calculus is a powerful tool to attack at least the optimization problem and surely represents at least one area where some fundamental development work is being done. I, for one, would be anxious to follow the work that these people are doing at Dodco.

Dr. Li: Mr. O'Neil's opinion about inside and outside is all relative. All your trouble comes from the outside. You may always name some region which is not inherent in the system. And if you draw a black diagram, depending upon where you put an error it can be outside.

Question from the Floor: Mr. Frank Barnes, Missile Electronics and Controls Department of RCA. One of the things that I haven't heard very much about here is the effect of disturbances on adaptive control systems. In particular, I am concerned about the kind of system that adjusts itself on the basis of the difference between the model performance and system's performance in the presence of a continuous series of disturbances at a time when there may not have been much of a form of a desired input to the model. Since the model doesn't know about these disturbances I am wondering if the error criteria that is used to optimalize the system will drive the parameters of the system in a direction to optimize the system in these cases. I feel that there should be some attention put on what kind of performance the system has when affected by these disturbances, especially in the simulation and computer work.

Captain Rath: I think I understand the question. The systems under consideration are those in which the adjustment takes place based on some criteria applied to an error between the model and the actual output. He feels that in those cases where you have a disturbance into the airplane other than through the model you would generate an error signal which, in turn, would call for some adjustments, although the system may already be at an optimum setting. Actually, I think there are two types of system that we have considered to date. One is where we do actually have a physically adjustment of a gain based on this error. I think Mr. Whitaker could best answer the question regarding this type of system. You do not get any adjustment unless there is a command input to the model. He will probably amplify that if there is any further question. The second type of system could be represented by the Minneapolis-Honeywell technique where, except for the gain change based on the amplitude of the limit cycle, there is no physical adjustment in the system other than the more or less spontaneous gain change due to the non-linear characteristics of it. Therefore, when there is an error between the model and the actual airplane response the performance parameters do not change in the respect that the question referred to. Is this right?

Mr. Bretoi: As long as the disturbance is not at the same frequency as the characteristic frequency of the limit cycle oscillations then the effect of noise is not expected to have a strong influence on the performance of the system except as it might relate to providing so much noise input to the system that the signal is too small relative to that noise. I might mention that work has been done in another area which is called an adaptive noise filter which we are using in connection with automatic beam following systems and this work does relate to the question posed here. The problem was to follow a beam, in this case an ILS beam, to an airport. This beam, however, is not necessarily straight. It contains bends and it varies with respect to time in many cases. The best gains for the system depend upon the bends in the beam and the time variation of the beam center. If you can measure the noise characteristics of the beam it is possible to adjust the lead and gain parameters of the system in such a way that you do get a satisfactory performance. Some work has been done along these lines. It will be interesting to see how some of these same ideas could be adapted or applied in conjunction with some of the adaptive techniques which are being investigated at this time.

Mr. Whitaker: I think I had better comment on this one. One of the fundamental features of the type of system that we are proposing was that you were making adjustments to an existing control system to optimalize its performance. Now, I was somewhat crushed by Dr. Truxal's comment much earlier in the day when he said that he had to redraw the diagram in order to show that we in fact had a linear system. I tried to draw the diagram so that this was obvious in the first place. The only thing that we are doing in this particular application is changing the parameters in the system on a basis that is closed loop with respect to its performance index. Now we had exactly the same system and we changed exactly the same parameters before only they were changed on a program basis. In relation to this particular question, we were happy with the response of the programmed flight control system before when it was flying through gusts; there is no reason why the system has changed. All that has changed is the manner in which you adjust to the optimum state. Now in getting to the flight condition in which you encounter a particular turbulence then presumably you have put inputs into the system that have caused the system to seek the optimum response. From then on the controller is there as a closed loop system and still has the same characteristics of service as the programmed system. Does this answer the question?

Mr. Osder: I think I ought to comment on that too because one of the first problems we considered was the effects of turbulence or noise on our performance computor which attempts to measure our so-called high frequency oscillations. From the very beginning we applied atmospheric turbulence to the problem and tried to develop techniques which could discriminate between the impulse we were applying and the turbulence. Actually, there is some slight effect in the system we worked with. It becomes more appreciable at high speeds where the spectural densities of turbulence appear in a higher frequency range. The worst effect that we ever got due to the turbulence at a high gain level was a slight change in gain of about two DB. Actually, it lowered the flight control system gain two DB's in turbulence and some people think it may be a good idea to lower the gain of the control system in turbulence. So from that point of view, as far as turbulence is concerned, we have not encountered any problem.

Dr. Li: Thank you very much.

Question from the Floor: Lieutenant Hoffman, Dynasoar Project Office. I had three questions but you have covered two already. That makes it very simple for me. I wondered if any of you gentlemen have any feeling regarding the relationship between the inner loop or stabilization loop adaption and the outer loop adaption. For example, the flight path or guidance loop adaption and what the relative payoff might be.

Mr. Marx: I think the reason why most of the people here have considered inner loop adaptation rather than outer loop adaption is that for the most part, your gain changes or variations are in the inner loop. For instance, if you are controlling attitude and you make the attitude or the rate loop adaptive you have included the effects of elasticity, change of c.q., Mach Number, and what have you on the performance of the system. If you extend this to the flight path loop, really, the only parameters which change are the airplane path-time constant and an airspeed term. The path time constant usually can be neglected if you have complete control of your inner loop. The airspeed term can best be provided by a program. The only payoff you you would get by using adaptive control then in the outer loop would be compensation for this airspeed variation. Now, it is necessary to answer a question here, can you actually measure the airspeed or do you have to resort to adaptive techniques?

Mr. Bretoi: Our particular approach to this aspect of the problem is to provide an inner loop which has the kind of dynamics which we desire. Then, depending upon what type of outer loop control that is desired, the inner loop is either an acceleration control or it is a pitch rate control. The closing of loops about either one of these is straightforward if you choose the right one. In the case of pitch attitude control, pitch rate inner loop is a good one to use because pitch rate is displaced just 90 degrees from pitch attitude and there is no gain compensation required if you have the same pitch rate response throughout the entire flight regime of the airplane. In the case of normal acceleration control, normal acceleration is quire directly related to flight on a vertical flight path, depending on the units with which you measure or define the flight path. If you define your flight path in terms of linear quantities such as feet per second or feet per second squared, the normal acceleration does have a constant relationship between these quantities. If you define your flight path in terms of an angle, then it would be more convenient to use an angular inner loop such as pitch rate.

Dr. Aseltine: I have just a short comment. I suggest that, if I understand the question properly, in many cases the guidance loop is almost an adaptive system by my definition already, because it measures performance of the vehicle regarding its orbit by comparisons through terrestial or inertial means and it does something about correcting errors. Perhaps, if you get too far off you might tighten the loop up a little bit and then this would be purely an adaptive system in my estimation. I think we are rather close to this now in an outer loop guidance system. This is what I understand the question to be.

Mr. Bretoi: I just want to point out that when you consider outer loop control, together with the inner loop control, as a part of an adaptive system, or when you try to make the outer loop part of a mechanism adaptive in addition to the inner loop control, you are dealing with a higher order system. This is an area where we should do more basic work. The question is, how do you make a higher order system adaptive?

Mr. Osder: I think that this is a very important point because I think it might indicate an area where possibly we try to push our adaptive systems too far. There are certain gain schedules that we use, such as the airspeed schedules mentioned before that are invarient; that is, they don't depend upon unknown aircraft characteristics. For example, take the case of the altitude outer loop. Basically, you have a true airspeed requirement on a parameter control if you have an inner loop which is a pitch attitude system. If, however, you convert your inner loop to a normal acceleration system you possibly can get away from needing true airspeed but the question is do we really gain simplicity. We never had a problem if we had available the airspeed data and it seems that in a manned aircraft the true airspeed doesn't have to be too accurate; as a matter of fact, Mach number always did the job quite well. If the true airspeed data is available or the Mach number data is available, you have a fairly simple system whereby you can vary the gain of your altitude error and retain an inner loop pitch attitude system. If you use a normal acceleration system, you will have problems such as how do you hold altitude in a turn? Unless you compensate the normal acceleration data to make it true vertical acceleration you are going to go into a dive. If you try to inertially compensate your altitude error data you will use h error and h. If h is derived from a normal accelerometer then it has to be compensated for the one minus cosine function. Here in the compensation problem you run into a lot of difficulties regarding accuracies. Small errors in the balance of the circuit can give you errors to either make you dive or climb and it usually results in complexities. This might look very clever and you might possibly get away with it but the point here is that this has never been a problem before. There are certain dangers in switching outer loops and inner loops. We have the problem of reliability in switching and we have transient problems. The transient problem occurs when we switch from a normal acceleration loop to a pitch rate loop with an outer loop such as altitude control engaged. So the word of caution that I would like to offer here is that we ought to try to apply the adaptive techniques primarily to those areas where there is some uncertainty in our gain programming or in those areas where there is absolutely no chance of getting the kind of gain control information required. As long as there are indicators that display speed to the pilot, such things as Mach numbers, for example, are being computed and they should be utilized to the best advantage in the flight control system.

Mr. Dandois: I would just like to add one comment. One type of self adaptive control that might be used as an outer loop would be a self optimizing type which seeks a maximum or minimum. For example, in guiding an airplane or a missile from one place to another we might want to maximize the range or minimize the fuel consumption. Usually this is programmed but it might be possible to devise some means of controlling the aircraft so that the maximum range is obtained automatically by maximizing his climb.

Captain Rath: I would like to comment on Steve Osder's remark. It is true that for certain applications where we do have available air data that the mechanization can be simpler and therefore more reliable and should be used. However, it is necessary to look to those applications which are not too distant in the future where we do not have the capabilities of getting accurate air data information and we should now begin to try to determine ways of operating without programming. I think although we may not use these techniques immediately in the future they will definitely have application and we should be working on them.

Mr. Buxton: I want to amplify on Captain Rath's comment. Basically, we are seeking a greater reliability in these systems. In most cases we have to use adaptive techniques when we don't have anything with which to schedule our knowledge at all. I would say a design consideration should be that if you can establish any reliability at all over a system that formerly used scheduling techniques then you should use adaptive techniques rather than try to use something that we haven't had trouble with before. I can't help but feel that one of the major causes of our control difficulties has been our scheduling devises. If we can improve the reliability I would sincerely vote for adaptive techniques in the outer loops as well as in the inner loops.

Dr. Li: All right, I think we have had enough answers to this question. I might add a little bit; that is, right now we are trying very hard to introduce adaptive techniques to this outer loop. As a matter of fact, when the Russians try to shoot at the moon, or we try to shoot at the moon, we schedule everything. Why do we schedule everything? Because there is no feedback. Why don't we have the feedback? The reason may be that we don't have a way of measuring the signals. Just like in the old days, when the house is too cold we put a shovel of coal in the stove. Later on we invented the thermostat and so we had feedback. If you don't invent the thermostat then you don't have feedback. If we can measure the so called ballistic missile and close up the loop we certainly would be glad to do it. Sometimes measurements are very important.

Question from the Floor: R. N. Clark, University of Washington. I would like to say three things: First, I think this conference has provoked a lot of material for discussion, but I will spare you gentlemen my opinions on most of this material and limit my remarks to two things. First, I would like to say if anyone is in Seattle, I would be very happy to continue this discussion in the electrical engineering department there and could probably arrange for a seminar type of discussion. Secondly, I would like to direct a question to Mr. Whitaker. Would you discuss the problems which you have encountered, if any, in making your system work with inputs other than step inputs, perhaps ramp or sine wave or statistical type inputs? A third thing that I would like to mention concerns performance criteria. Performance criteria have not been discussed here and I feel we have need for work in this area in the future. I think that we have to search for a criteria beyond that which is found in our text books now. In particular, we have to find a criteria that is compatible with realistic type input signals, in the aircraft business, the kind of signals our auto pilots really do get. I think we should make an attempt also to incorporate in our performance criteria the same sort of subjective evaluation to which we subject our control systems on the analogue computor. In particular we look at the response and say this is better than that. I think the attitude with which the Dodco people have approached their work is a good indication that we are getting out of the text books for a criteria.

Dr. Truxal: I agree we need more knowledge. I agree with that wholeheartedly.

Mr. Whitaker: I am not sure how things are on the frontier in Seattle but we would be happy to have anyone come up to the hub of civilization in Boston. In regard to the second question, the questions of input, I knew that if we tried to show you something here that, as Dr. Li says, has some sex appeal - that we would be accused of having a system that only works with step functions and I would like to assure you that this is not the case at all. In probably the most typical operational use, environment is changing in a continuous manner and the parameters would also change in a continuous manner. Actually, it isn't very glamorous to see the same response all the time. In order to have something that would show what would happen if the parameters were way off, we are using the step function to define a parameter called convergence time. You could define convergence time as the time it would take to come back to the optimum state from some non-optimum condition. Similarly, you could define a similar time for changing an environment if the conditions were fixed and then the environment was changed, and the system allowed to converge. It is not necessary to only have step function inputs. This is the prime reason why the sampling for the system is keyed to the input and output relations of the model. If the input changes the sampling also changes. Now our flight test program had some of the characteristics of an Egyptian Mummy, we were pressed for time. We weren't able to present you with complete quantitative data on the other types of input. However, we have some flights in which we just let the pilot fly around and do normal maneuvering. I can't give you quantitative information on these flights; however, the system appeared to give satisfactory flight response as he did that. Another type of test was one in which we simulated a dive bombing mission. If we just asked the pilot to nose over wings level and not touch the stick, then, of course, there would not be any input in and the system would not change, but if we give him the task of tracking a target on the ground, then his tracking inputs will be sufficient to give the system enough information to find its own optimum point and to keep the gains changing as the environment changes. A dive bombing mission is the most severe type of environmental change you will get. It involves starting from high altitude at a low Q and getting down to sea level in a hurry. Unfortunately, dive bombing isn't very popular anymore and I am not quite sure what the most stringent environment change would be or what the most stringent requirement for this system would be. In summary, it is not necessary to have step inputs and what input data we do have indicates the system works quite well on the basis of just the normal flying input that you would have in traversing the sky. We did do some simulator work in which we used a cockpit simulator and scope presentations, and had someone sitting in the cockpit trying to track the scope in an effort to simulate the same type of thing.

Question from the Floor: G. G. Moss, Convair, Pomona. Something has impressed me about the design criteria. It seems that we all want something that is adaptive but there is a slight difference in some of the approaches. One that Minneapolis-Honeywell has taken is where the switching occurs and you try to get a sort of a least time optimization. The approach by Dr. Aseltine and others is to adjust the system rather slowly so that we have perhaps a least square criteria. Now the two different approaches seem to have different merits. For instance, in the least square criteria, where we slowly change the parameters of the system, you don't introduce erratic signals and perhaps excite modes that you don't know anything about, but at the same time you don't take full advantage of the least time approach where you can respond more quickly to large errors. I would think that these differences should be emphasized and something said in defense of the particular approaches. Also, it seems that more could be said about the possibilities of improving the reliability and dependence of the system on components, but I suppose there is no use in doing that now. I prefer the contest.

Dr. Li: This is a very broad and general question and touches on almost every type of system that we have tried to analyze and classify; however, I will turn this over to the panel for three answers and ask that the answers be limited so that we can get another question in.

Dr. Aseltine: I might remark that in reference to what was called "least square", this was really a method of evaluation which was based on looking at the transient response and measuring areas above and below the line that was obtained by noise variance and "least squares" played no part in it. Incidentally, it is also a method which is not in any text book. It was an attempt to find some description of the system. It is based on a property of the system which is a measure of performance and that is impulse response. In special cases it probably is true that the Flugge-Lotz method would be faster and I think this is important. I think also that one ought to realize that the general application of the switching method to an unknown system is something I don't believe has been studied. When you get into a second order system, such as the studies that Flugge-Lotz and Taylor have made, you can probably get very fast results, but I am not sure that with an unknown system this would be possible. These responses are not based on anything that you know about the system itself; they are based on the way it works. I think the speed is good and I think Flugge-Lotz has a good way of going after it but a lot more work is needed there. I just wonder again if there isn't some minimum time that it takes to do this job.

Question from the Floor: Hugo Shuck, Minneapolis-Honeywell. I would like to comment in that general vein and sort of second the motion that Dr. Aseltine has indicated. We need to get down to a fundamental thing like Heisenburg's uncertainty principle to establish what kind of accuracy can be obtained as a limit and as a function of the length of time required. There are several techniques that have been mentioned in which integration is used, and in a way, we are sort of shaping down to integration versus no integration, cross corrolation and so forth all implying that there is a time integration. In Fourier transforms you have an integration that must go on out into infinity which isn't very useful because you want very recent information. So you have to truncate to some extent. Now it is this truncation process which is significant in this application as in many others. There hasn't been too much study on it and certainly the suggestion that we ought to try to get something definite down is very much in order. In fact, to put a question tinge on to this, I would like to ask if any of the panel members know of any optimization of form of the time waiting function which is applied to this integration process? In other words, do you cut it off exactly on a square chop as you do when you try to average breakfast food weight or do you use the exponential draw up that you can get out of a simple RC circuit or is there some other optimum method?

Dr. Li: The heart of his question is that he would like to know in a general way what is the best way of chopping off the so-called pulse response in order to give you enough information. I wonder if any of the panel members want to answer this question?

Dr. Aseltine: One brief comment is that in the particular method I just talked about this thing is sort of taken care of because the thing you are integrating tails off generally exponentially, at least if you are anywhere near the operating point that you would like to be near, so there isn't any necessity to worry about it.

Mr. Whitaker: I would like to make one comment. When using a model the characteristics of the model such as its rise time and exclusion time may be used to regulate any sampling. Integration in that case actually is performed in potentiometer servos. When you are integrating you are changing the gain at that time and the change in the potentiometer position is the value of the integral. The criteria of the integral should be zero so that when you reach your final state over any sampling time the actual change in the potentiometer is zero. You are changing the gain while you are doing the integration.

I don't know whether this is quite what you had in mind or not. I know the effect of the sampling time is to weigh the error. I think Graham and Lathrop suggested as a criterion the integral of time times the absolute value of the error. In a sense, using a definite sampling time is also using a weighing function. It is a weighing function at a constant level for a given time and then zero from then on.

Question from the Floor: Leo Chattler, Chance Vought Aircraft. I would like to make some statements which can also end up as a question in regard to the self adaptive systems that were discussed at this meeting. For one, I think we have to divide our thinking between a manned vehicle and an unmanned vehicle, because for one I don't believe that any pilot in an aircraft of the type where a pilot will hold the controlling lever in his hand, will accept any feedback into his controller. That has been proven time in and time out and has been the point of many discussions with many military flyers. So for those systems where you contemplate that there will be feedback into the controller, I would recommend that you not seriously consider their use for fighter or attack type airplanes. To use the systems that give you a saw tooth output or residual amplitude at the output of the actuator to the degree that was discussed here, I think we will have to get a new line of bearings for our aircraft. I think if people will remember back when the acceleration switching servo valve first came into the picture one of the problems with it - I am speaking of the time modulated valve - was the fact that before you would even get the system tested, if you had significant time modulation you wore all the bearings out.

Now the other point of consideration here is the fact that you still must look at the mission of your vehicle. Do you really want to put all this equipment into a vehicle that may have a transient gain change and then spend a great deal of time at a fixed gain that would give the vehicle the response and stability that is desired for a point of the mission? It may be better to try to devise a scheme that will affect a gain change from an optimum within a certain area of the flight envelope to an optimum at another point of the envelope. I think Mr. Osder can allay his fears about people going too far with the systems. I proposed this on one of our vehicles. I said, " Wouldn't it be wonderful, we don't need air data" The answer was, " Who won't, the pilot will still need air data" . So this will still have to be in the airplane although the order of magnitude is different.

Actually as was pointed out here previously the vehicles that we are looking at today can use this adaptive system and one very pointed question that I would like to have answered is, are people designing some one of these schemes into the vehicles that they are working with today? Thank you.

Mr. Bretoi: There are several questions, Mr. Chattler brought up the fact that you have to consider manned aircraft in a different light than you do unmanned aircraft in many respects. One aspect of this relates to the fact that you do have a pilot aboard and your performance criteria for the system will depend on how you use that pilot in the system. He could be used in a control stick steering mode or used to identify targets or track targets, or used to provide a navigational function, so the performance of your system does depend upon your pilot and it suggests that what we need to do is to know more about what the pilot's transfer function is. Work is being done by a number of agencies under military sponsorship to find out this transfer function; and maybe one of the values of non-linear techniques is that these techniques will give the engineers a better feeling for non-linear systems and possibly permit them to devise better approximations to pilot performance in terms of non-linear characteristics. The pilot is, of course, a non-linear mechanism.

Another question related to the amplitude of a limit cycle motion when you are using a system with a discontinuous signal. We, of course, have been worried about this problem also and we have looked into it. One of our approaches is to make this amplitude as small as possible and the amplitude of the motion which we are shooting for is just barely above the threshold of our instrumentation which picks up or senses the motion of the control servo. The amplitude which we are shooting for is in the order of a tenth of a degree. To find out how objectionable these limit cycle amplitudes are we compared them with the amplitude of motions that we would get from a conventional system. This comparison was made from flight test results of both systems. We have found, and this is somewhat surprising, that the amplitude of the surface motions using the non-linear system are no greater than the amplitude of the motions that you get with a linear system and in many cases they are smaller. The linear systems we investigated included the control systems in the F-94, F-100, and F-101 airplanes. We have quite a bit of data on these. As far as the amplitudes of residual motions are concerned, the non-linear systems do compare favorably with the linear systems.

Dr. Li: Thank you. Well, gentlemen, as the moderator of this last session, I would like to represent you, the audience and the participants, to thank our host, the Wright Air Development Center and especially Captain Rath and Lieutenant Gregory for this opportunity to gather together. I am looking forward to other opportunities to have this same type of meeting. I wonder if Captain Rath has something else to say?

Captain Rath: I certainly want to thank everyone for their patience and cooperation in making this two day symposium a success. Thank you very much.