# PHYSIOLOGICAL FACTORS CAUSING STRESS-SUSCEPTIBILITY

by

D. N. Marple*

The problem of stress-susceptibility was first described by Topel *et al.* (1968) and came to light in the mid to late sixties when producers and researchers in the Midwest noticed that apparently healthy swine were unable to withstand various types of stressful situations. In fact, the stress of sorting animals for market, vaccination, estrus and breeding, and exposure to conditions of high temperature and humidity as seen during the Summer months often resulted in the death of certain animals, which upon post-mortem examination, were found to have no pathological abnormalities. However, these animals often yielded PSE carcasses during the post-mortem examination. Since Ludvigsen had proposed that swine yielding carcasses with "muscle degeneration" were suffering from adrenal insufficiency, adrenocortical function was one of the first physiological variables to be examined. Although a number of research groups monitored plasma levels of glucocorticoids in slaughter blood, in an attempt to relate adrenocortical function to the incidence of PSE, no significant or consistent relationships were found.

The glucocorticoids had been shown to play an important role in stress-adaptation (Selye, 1950) and although no data were available to demonstrate an adrenocorcidal insufficiency, many researchers still believed that some type of relative insufficiency existed in stress-susceptible swine. Since the catecholamines are also involved in stress response, research efforts were divided between examining the effects of catecholamines and the role of the pituitary-adrenal cortex system in stress-susceptible swine.

Attempts to determine whether the proposed adrenal insufficiency was of primary or secondary origin were unable to detect any differences in plasma cortisol levels between stress-susceptible and normal swine (Marple *et al.*, 1972). However plasma levels of ACTH were significantly elevated among the stress-susceptible swine. In addition, stress-susceptible swine were found to have a lower ratio of plasma cortisol to ACTH than normal swine. Therefore, it appeared that stress-susceptible swine may have had a defect in the adrenal cortex which prevented adequate response to stimulation by ACTH. This hypothesis was examined by Sebranek *et al.* (1973) who estimated the *in vivo* response of the adrenal cortex to exogenous ACTH. Significant increases of plasma cortisol were noted in both stress-susceptible and normal swine. However, the magnitude of the increase in stress-susceptible swine was less than that observed in normal swine. These results could be interpreted as being indicative of an adrenal insufficiency but the possibility remained that the lower response of the stress-susceptible swine could be due to an increased rate of metabolism of cortisol instead of decreased synthesis and release of cortisol by the adrenal cortex.

To examine the possibility that stress-susceptible swine were metabolizing cortisol more rapidly than normal swine, Marple and Cassens (1973) compared the metabolic clearance rates (MCR) of cortisol in normal and stress-susceptible swine. Their results indicated that stress-susceptible swine did in fact metabolize cortisol approximately five times faster than normal swine. Therefore, the higher levels of plasma ACTH in stress-susceptible swine may be necessary to maintain adequate circulating levels of cortisol. It is of interest to note that although the adrenal glands of stress-susceptible swine are apparently under greater stimulation by ACTH, few researchers have been able to find any corresponding changes in adrenal morphology or size that usually accompany increased stimulation by ACTH. This may be explained by the observation that the excessive stimulation by ACTH found in many pathological states is often more than a ten-fold increase in ACTH whereas ACTH levels are only elevated by a factor of 2 to 3 in stress-susceptible swine.

While attempting to relate the above findings of increased plasma ACTH concentrations, variable plasma cortisol concentrations, and an increased cortisol metabolic clearance rate, it was noticed that similar conditions are characteristic of hyperthyroidism. Therefore, circulating levels of thyroxine were determined in normal and stress-susceptible swine. As in the case of plasma cortisol levels, no consistent trend could be established. Eikelenboom and Weiss (1972) reported significantly elevated thyroxine levels in Pietrain gilts but no differences were noted between normal and stress-susceptible barrows.

*D. N. MARPLE

*Department of Animal and Dairy Science, Auburn University, Auburn, Alabama 36830.*

Thyroxine injections to normal swine resulted in an increased rate of post-mortem glycolysis as indicated by a more rapid rate of muscle pH decline (Marple *et al.*, 1975). Results from the same study revealed that thyroidectomy significantly retarded post-mortem lactate accumulation in muscle.

Similar results were noted by Kraeling and Gerrits (1973) and Kraeling *et al.* (1975) who monitored the rates of post-mortem glycolysis in swine with varying levels of pituitary function. The rate of post-mortem glycolysis was found to be positively related to pituitary function in that hypophysectomy significantly retarded glycolysis in otherwise normal swine. These authors concluded that pigs that display PSE musculature may hypersecrete one or more pituitary hormones.

To test the hypothesis that stress-susceptible swine utilized or metabolized thyroid hormones more rapidly than normal swine without any major alteration of circulating hormone levels, Marple *et al.* (1977) determined the metabolic clearance rate of thyroxine in Halothane-sensitive (HS) and in non-reactive littermate control swine. Data from blood samples collected at periodic intervals after the injection of $^{131}$I-thyroxine provided information to calculate the biological half-life and metabolic clearance rate of thyroxine. The results indicated that the half-life of thyroxine was significantly shorter in HS pigs. Similarly, the $T_4$-MCR was significantly greater among the stress-susceptible swine indicating that they metabolized thyroxine more rapidly than their stress-resistant littermate controls. A follow-up study on the MCR of $T_3$ in HS and control animals indicated that the stress-susceptible pigs also metabolized $T_3$ at a greater rate than their littermate controls. Since the rate of metabolism of both $T_4$ and $T_3$ is significantly elevated in HS swine, it may be proposed that stress-susceptible or HS swine suffer from a defect similar to that of thyrotoxicosis.

Thyroxine has been reported to influence sarcoplasmic reticulum (SR) function in that Ash *et al.* (1972) noted that the administration of thyroxine to cats resulted in a reduced ability of the sarcoplasmic reticulum to bind calcium. Therefore, if the peripheral tissues in stress-susceptible swine are exposed to, or utilize more thyronines, then perhaps the sarcoplasmic reticulum fragments from HS pigs would also have a reduced ability to bind calcium in the presence of ATP. In a comparison of the calcium accumulating ability of HS and normal pigs, it found that SR fragments from HS swine were less able to bind calcium than fragments isolated in a similar manner from normal swine (Marple *et al.*, 1977). Similar trends for calcium binding have been reported by Isaacs and Heffron (1975) using SR fragments from normal and HS human subjects. Therefore, it could be proposed that the defects in the sarcoplasmic reticulum may be due to adverse endocrine stimulation.

Up to this point, little has been said regarding the involvement of the catecholamines in the mechanism of stress-susceptibility. Many research groups have examined catecholamine levels in normal and stress-susceptible pigs and in general, no consistent trends have been found to suggest that stress-susceptible pigs have either a hyperproduction or deficiency of catecholamines. Early work by Judge *et al.* (1968) reported no significant differences in urinary levels of catecholamines among animals yielding normal or PSE carcasses. Later work by Weiss (1971) indicated lower levels of catecholamines in the plasma of stress-susceptible pigs. Similarly, Althen *et al.* (1975) were unable to find any significant differences in plasma or tissue levels of catecholamines between stress-susceptible and normal swine. However, Weiss *et al.* (1974) were able to demonstrate that pre-treatment of stress-susceptible swine with the beta blocking agent propranolol resulted in lower levels of plasma lactate after a period of forced exercise than stress-susceptible pigs pre-treated with an alpha blocking agent.

Williams (1974) has proposed that the conditions of stress-susceptibility and malignant hyperthermia are caused by an excess of norepinephrine activity. Williams was able to demonstrate that stress-susceptible pigs pre-treated with alpha-methyl DOPA were protected when challenged with halothane and succinyl choline. Treatment with reserpine, a catecholamine depleting agent, also protected these animals from the potentially harmful effects of halothane and succinyl choline. He therefore concluded that the release of norepinephrine was the primary triggering step in the development of the stress syndrome. Williams has proposed that the norepinephrine accumulates in the blood and tissues as a result of a deficiency of monoamine oxidase or COMT, enzymes responsible for the biological inactivation of norepinephrine.

The theory of increased catecholamine production does have many merits. The effect of catecholamines on the peripheral vessels may explain the blotching of the skin that occurs due to vasoconstriction which in turn might prevent heat loss and thus lead to hyperthermia. Further support for this hypothesis is provided by the observation that beta blockers reverse these adverse effects. Propranolol, a beta block-

ing agent, has been proposed for use in the management of malignant hyperthermia in human cases and has met with varying degrees of success. It is of interest to note that beta blockers have also been used successfully by Galaburda *et al.* (1974) and Levey (1976) to manage cases of severe thyrotoxicosis. There is adequate clinical and experimental work to suggest that thyrotoxicosis can increase catecholamine activity although the mechanism is unknown. On the other hand the condition of pheochromocytoma or excess catecholamine production is seldom accompanied by production of thyroid, pituitary, and adrenocortical hormones. Therefore the altered catecholamine activity present in stress-susceptible animals could be a result of a thyrotoxicosis-like condition present in these animals.

The mechanism is actually not known whereby some animals are unable to withstand exposure to various stressful situations or to pharmacological triggering agents. The results of Kraeling and Rampecek (1976) would suggest that the stress-susceptible like condition can be induced by the injection of pituitary extract which would also stimulate the adrenal cortex and the thyroid. Their studies were, however, unable to induce halothane sensitivity in the animals receiving the pituitary extract. Therefore, the conditions of halothane-sensitivity and stress-susceptibility may not be identical. Rather, it may be proposed that the halothane response may be triggered in the muscle at a receptor complex sensitive to halothane while stress-susceptible swine exposed to physical or environmental stressors undergo a series of responses prior to the stimulation of heat production in the muscle. Although the mechanism for heat production may be the same in both conditions, there may be more than one way to trigger the system responsible for thermogenesis.

Thus, the mechanisms responsible for the overall problem of stress-susceptibility and halothane-sensitivity remain obscure. The alterations in muscle enzyme activities and membrane function may be the direct result of specific genetic defects. However, these changes may be in response to genetically induced alterations in the endocrine or neuroendocrine systems.

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DISCUSSION (Physiology and Biochemistry Session):

E. D. ABERLE: While we are waiting on someone to raise his hand for the first question, I would direct one to Marion Greaser. Marion, on one slide, you indicated that calcium uptake by sarcoplasmic reticulum can be reversed so that you actually can have a net synthesis of ATP by the reversal of that process is intriguing. I wonder if any evidence or data show that this process occurs during the initial post-mortem period at least to help maintain a high supply of ATP; and I also wonder what kind of conditions would be necessary for this to be of importance?

MARION GREASER: Well, first of all I think that this mechanism, the leaking out of calcium from loaded vesicles to power the synthesis of ATP from ADP and inorganic phosphate, is not going to produce enough ATP to keep the system going. I think the problem here is that the calcium that is leaking out is

going to activate the myofibrillar proteins, stimulating a rapid ATP breakdown. I guess the point that I would like to make is, in considering calcium release mechanisms post-mortem, where the speed of release is not nearly as important when we are thinking about the minutes stretching into hours that occur after death, part of the calcium that is leaking out of the mitochondria might come out by just a reversal of the pump as the ADP and inorganic phosphate levels increase. But the synthesis of ATP by this mechanism is not going to compensate the more rapid breakdown of ATP by the myofibrils that are occurring as a result of the calcium activation.

MIKE DIKEMAN, Kansas State: Question to Dave Topel. You indicated this lack of control of calcium is probably a genetic lesion. I guess the first part of my question would be, what is the latest thinking on the heritability of the stress syndrome in swine? And secondly, would you care to extrapolate, let us say in the bovine, whether there is a similar genetic lesion in the bovine?

DAVE TOPEL: That should stimulate the first stage of the syndrome I guess. There is quite a bit of contradiction in the literature on the heritability of this problem. I did not express much as far as other species in my talk, but this problem actually exists in most species, including humans, as many of you know. The heritability in humans may be different from that in pigs. It is not as easy to do good heritability studies in humans. The reason I am bringing this out is you can go in the literature and find quite a bit of data that says that it is a dominant trait in humans with varying degrees of penetrance, whatever that means. Well, in the pig there has been a great deal of work done on really some extremely good studies, well designed studies, that will give a very good indicator of the exact heritability. Dr. Christian at our research station in cooperation with Dr. Rasmusen at the University of Illinois have clearly shown it is a recessive trait. Loren just came back from a trip to Europe last Friday and in visiting with the researchers in Holland and in Denmark, they also have clear-cut evidence to show that it is a recessive trait. Their data is in 100% agreement with the Iowa State data, so I'm positive it is a recessive characteristic.

Now in beef cattle, it is an interesting question. We are currently doing some research on double-muscled cattle to determine their stress susceptibility, and one of the tests we are using, of course, is the traditional blood levels of creatine phosphokinase. If you subject double-muscled cattle to five minutes' exercise—a pretty good running exercise back and forth—it is enough to trigger CPK levels as high as 300-400 Sigma Units, from basically 40-50 up to 300-400. That is exactly the same response you get in a stress prone pig. Many of the other characteristics have been recorded on muscle from double-muscled animals would tend to say that maybe we are talking about a similar genetic defect. On a limited data (we've collected data now on about 20 animals—10 of each type), it appears to be maybe the same abnormality, at least the response to CPK levels are the same.

JOHN SINK, Penn State: As long as we have Dave on, I would like to ask Dave what are you talking about when you are talking about genetic lesioning?

DAVE TOPEL: Well, basically, what I am saying is it is a heritable trait and expressed in such a way it has to be triggered by a certain environmental stimulus and the lesion is a result of an inherited trait. It is something you know is wrong that can be transferred by a genetic mechanism. What it is is a mystery. That is basically what I am saying.

AL PEARSON: That is what the biochemist calls an inborn error of metabolism.

JOHN SINK, Penn State: I would like to direct this question to Al Pearson, and I appreciate, Al, that you said, whenever you were showing us the transparencies, the data was preliminary. If I remember right, you were talking about the fall in data on the first transparency you showed, and yet the interesting thing that appeared to be on that transparency was when you went from pH 7.0 to 6.8 in both bovine and rabbit, the values dropped. When you hit 6.2, they rose again and then they dropped again down into the 5's. I was wondering if you might comment on that consistent increase or rise at pH 6.2.

AL PEARSON: I have the data here in front of me. I brought it up just in case anybody asked a question because I am not as familiar with it. But it does show a rise at pH 6.2. I really do not understand that. Of course we have not worked with mitochondria before and I am not sure. Another thing I should have pointed out is this system is different from the one we used with sarcoplasmic reticulum. In the first study we did not use oxalate and in this study we are using oxalate (whether this makes a difference I do not know). Certainly, the sarcoplasmic reticulum does not show the same pattern in this study as it did in the other and I suspect we may have a problem here in our methods which we are going back to try to thrash out before we go any further with it.

DENNIS CAMPION, USDA: I have a question for Marion, please. At least three ATPases have been described in the sarcoplasmic reticulum, the magnesium-

calcium ATPase, the magnesium ATPase, and then a calcium ATPase. Are these three different proteins?

MARION GREASER: No, they are all three the same protein. If you measure the ATPase activity of this 102,000 Dalton protein in the absence of calcium and magnesium, a small rate of ATP splitting results. If you add magnesium to this, you get an increase in activity, and this would be the so-called basal ATPase activity. If in addition when you add calcium to the system, you can increase the ATPase even further. We are talking about the same protein just under different situations in regard to ion activation.

AL PEARSON: You only get it on one place in the gel too, which is further proof for what he says.

MARION GREASER: Yes. There is no confusion in the literature from people in regard to different proteins being responsible here; it is just a different ionic requirement for ATP splitting and getting different activities with different ions present.

DON KROPF, Kansas State: OK, I'll try this. Dave Topel, is malignant hyperthermia in humans necessarily restricted to the muscular type of people or is it evident sometimes in people who are not very muscular?

DAVE TOPEL: Well, I just have to base my answer, Don, on observations, pictures, etc. No question arises if you go into the classical work recorded 10-12 years ago showing pictures of people who were responding abnormally to halothane. They were of double-muscled, weight-lifter type musculature. Because it is out of my area a little bit, I would be a little reluctant to say they have to be of that type. I do not think they have to be, but if they are of that musculature type, certainly they are very suspect. At a meeting we attended in Denver in April, they again reviewed this and certainly emphasized again the fact that a high proportion of the people who have the problem are very muscular but they do not absolutely have to be.

DON KROPF, Kansas State: One further question. With the fact now that halothane reactivity and stress susceptibility may not be the same, what do you suggest as the best diagnostic tool? Any one of you.

DAVE TOPEL: Well, I guess, probably, Don, I have thought about that for quite a while and four or five years ago I probably would have said there probably is a chance they are not the same abnormality. But today I would say they are the same. I really believe they are exactly the same problem. The reason you get differences in expression, etc., is probably due to the fact it is clearly undecided and unknown what all can trigger the mechanism and under what conditions it can be triggered. Just because it does not trigger it one time does not say that it would not trigger it again. But the defect is probably the same and many, many factors could be involved as Dr. Marple mentioned. When a certain critical point exists (you may use the term when a biological limitation exists), then it is triggered. But I think the true defect is really the same.

DENNY MARPLE: We can work on that later. One of the things I think we do need to consider is the triggering mechanism and the fact we can trigger it pharmacologically. But, we are not necessarily sure when we trigger it physiologically, we are working at the same level. I do not understand how we could trigger it physiologically and be able to cause the same chain of events a pharmacological agent interacting with a specific receptor could cause. So I think this is still an area on which we must work.

E. D. ABERLE: Paul Addis, did you have a comment on Don Kropf's first question?

PAUL ADDIS, Minnesota: Yes, I did. I would like to agree with what Dave said that many of the people who have MH or malignant hyperthermia are muscular, although there are many who are not. However, all reported cases I have known about do have some muscle abnormality like strabismus or scoliosis or something like that (terms that Bill Stringer I am sure will enjoy). I do have a question for Dave about the genetics of MH. I know there are much data showing it is recessive, but I just might make a comment. Most of these data have been collected using muscle rigidity as a primary criterion for malignant hyperthermia and using a five-minute halothane screen. Now knowing in some cases we might have animals that don't react immediately or within that five-minute period but still could be positive reactors with a longer screening period, and, realizing there are other factors in the stress syndrome besides rigidity—such as hyperlactemia, hyperthermia, high blood $CO_2$, etc., etc.—if all of those are taken into account, do you think in studies that have been done in humans or have been reported by Britt recently, possibly other genetic mechanisms could be postulated from those data other than a recessive form of inheritance?

DAVE TOPEL: Well, I have to tread on very thin water here because I am not an animal breeding person and not trained well in genetics. I will base my answer, Paul, on what my colleagues indicate. In testing the inheritance, it is important of course to have an extremely good classification system so that

when you make the matings you know exactly the genotype of that mating. In the work Dr. Christian and Dr. Atkinson are doing, they have an extremely good classification so when they mate a pig that is a known stress-susceptible animal (they also have I think information where they have extremely good reliability if it is a known stress-resistant pig) they make the various crossings you need to make, it is not a question so much, Paul, of when they express it—if they express it immediately or if it takes two hours to express it, they have the ability to express it—so what they have is, really then, known genotypes. Then by making the back crosses, etc., they are establishing the heritability. From that standpoint in the pig work at least, it will stand very well on its own. Now it is not possible to do that in humans. They study pedigrees, you know, etc., of different families, which makes the estimates of heritability somewhat difficult because you can not truly test the inheritance. I think it has some valid usefulness. The tests that have been used are acceptable for determining the true inheritance.

HAROLD HERRING: Armour: I would like to direct this question to Dr. Topel and Dr. Marple. Do either of you know whether the incidence of watery pork and of the stress syndrome is increasing or decreasing in the United States at the present time?

DENNY MARPLE: I do not really have any concrete evidence. I would assume, based on just information we get from producers, etc., the incidence of stress susceptibility per se is decreasing whereas I do not really have any information on the incidence of PSE. Maybe Dave has something on that.

DAVE TOPEL: I have nothing recent. Approximately a year ago, after talking with several packing plant management people in Iowa, about 8-10% of the pigs slaughtered had some degree of PSE muscle, either one or two on the color scale. In talking to our Extension people in the state of Iowa, and they are in contact with our producers constantly, I do not think the incidence of the stress syndrome is decreasing as much as some people think. The people eliminate it in a given herd and then it will pop up in another herd. We have switched, in Iowa, basically from the purebred industry to the commercial industry. Now when we get the commercial producers informed about the problem and how to select against it, I would predict a greater drop, but we have just shifted the emphasis in a matter of five years.

CURTIS MELTON, Tennessee: We have talked quite a bit about PSE with the experts on this subject. Does dark, firm, and dry enter into this picture? I know in our packing plants in the southeast we see sometimes about as much dark, firm, and dry carcasses as we do PSE and all of a sudden it will switch. I would like to get your comments on how this is interrelated.

DENNY MARPLE: The problem with dark, firm, and dry is analogous to what you have in dark cutting beef, that is the animal would be exposed to a long-term stress depleting the muscle glycogen stores, leaving little or no substrate for post-mortem glycolysis and you end up then with the dark, firm, and dry condition. Generally we would think the animals who would yield the dark, firm, and dry carcasses would be animals who would be stress susceptible who were unable to tolerate the long-term stress of being hauled to market, etc.

MELVIN HUNT, Kansas State: Dr. Pearson, what is the fiber type distribution in the sternomandibularis muscle and is that distribution uniform throughout the muscle?

AL PEARSON: We have made some photomicrographs of the muscle. As beef muscle goes, it is a red muscle. It would be a redder muscle, for example, than the LD, which would be classified more towards the white side. Now we have not looked from end to end to the muscle and done an extensive study to try and determine whether it changes from place to place, but in general terms it would be classified as a red muscle in the carcass. It is very high in connective tissue which makes it appear somewhat pale when you cut through it, but it is connective tissue rather than the fibers themselves that give the pale appearance.

MELVIN HUNT, Kansas State: Do you have any idea what percentage of alpha-red type fibers are present?

AL PEARSON: Well, I do not happen to be a disciple of alpha-red, so I will not comment on that.

E. D. ABERLE: Other questions? Several of you have commented *in vitro*, mitochondria will become engorged with calcium. Is there any indication of just how much calcium mitochondria will bind under *in vivo* conditions? Marion, any idea?

MARION GREASER: On the one slide that I showed, essentially three different conditions are mentioned: one with no permeant anions, one with phosphate present, and one with phosphate and ATP. Well, clearly, in muscle under living state, the inorganic phosphate level is fairly low so that you do not expect to get the sort of massive accumulation that

can occur in an *in vitro* system when you add phosphate. So you are talking about reasonably small levels of calcium uptake. Now, the other problem is we really do not know what the resting state of calcium concentration is in muscle. It is thought to be of the order of maybe 2 x $10^{-7}$. The question is how much calcium can be bound by mitochondria when the calcium concentration outside the mitochondria is at that level? I have not seen any data to that effect, but I would think it would be fairly small. However, when we isolate mitochondria and then measure calcium binding, we do not know whether we have hurt the mitochondria or not so it is rather much up in the air. It is clear, though, that mitochondria are capable of taking calcium away from troponin or myofibrils. They do have a high enough affinity so that they can remove calcium from the contractual apparatus. It appears at least some calcium binding must be in muscle mitochondria under normal conditions.

BRUCE MARSH, University of Wisconsin: I would like to ask Al Pearson about one of the earlier results he quoted. Al, you showed that lower pH's and lower temperatures both increase the release of calcium from sarcoplasmic reticulum. If we try to understand cold-shortening solely in terms of sarcoplasmic reticulum without involving, say the mitochondria, would this not imply we should get an increasing cold-shortening as glycolysis proceeds to produce a lower pH rather than the decreasing cold-shortening which is observed in practice?

AL PEARSON: Would you mind repeating that; I didn't quite follow the logic there, Bruce.

BRUCE MARSH: Possibly there is not much logic in it, Al.

AL PEARSON: There may not be any logic in the answer either, Bruce.

BRUCE MARSH: Your results showed pretty clearly as both pH and temperature are lowered, an increasing calcium released from the sarcoplasmic reticulum. Now if we try to interpret cold-shortening solely in terms of sarcoplasmic reticulum without invoking say mitochondria, would your result not imply that cold-shortening should increase with increasing time post-mortem because the pH is falling, whereas in fact cold-shortening is greatest early post-mortem at high pH values?

AL PEARSON: Those implications certainly may be, Bruce, because you do get that increase, as you pointed out. It is dependent, of course, upon what proportion of it is released at such and such a time. The times we are working with are relatively short and we are working in an artificial system, of course, and whether this would actually hold true in the intact system, the model system we are using, I cannot answer. I guess I do not make sense in my answer.

MARION GREASER: I would think the same situation would be a problem with mitochondrial calcium release. If mitochondria release calcium it should also stimulate the ATP breakdown and so on. I am not sure whether we can distinguish between the two systems by that means.

RONALD DALRYMPLE: American Cyanamid: Denny, I would like you to comment on the possible relationship between these thyroid abnormalities in the stress susceptible pig and the fact these are leaner type of pigs.

DENNY MARPLE: Well, this is a possibility we have thought about a number of times. It would fit in with the hypothesis we have developed that the stress susceptible pigs which have this hyperfunctional thyroid system usually are leaner sorts of animals. This would fit right in with the general function of thyroid hormones. I think it adds further support to the whole thing. Also as we come along then with the effects of thyroid hormones, they do have effects on mitochondria and this is something that we really did not get into today. Further interactions are there with the thyroid hormones, carcass leanness, sarcoplasmic reticulum, and mitochondria.