SIR JOHN HAMMOND, C.B.E., F.R.S.

AN INTERVIEW*

Professor Parkes. John, I've often heard people express great interest in the way your scientific ideas and work developed. Would you tell us something about it?

Sir John Hammond. Well, of course I am primarily interested in agriculture. I was sent to Cambridge to do agriculture in 1906, but my tutor advised me to take a degree in pure science first, so I took the Tripos before doing agriculture. After that my interest became centred on animals.

I got a 3-year scholarship from the Ministry to work with Marshall on fertility and milk secretion. That's where I made a start. My scholarship ended in October 1914, just in time for me to go off to the Army. During the 3 years, I absorbed a great deal of science from Marshall, and I attended a lot of lectures in the University — Assheton, for example, on Embryology — anything related to my field.

P. And Marshall was lecturing in the Physiological Laboratory?

H. Yes, but he was lecturing also in the School of Agriculture, and that was where he first interested me, as an undergraduate. He came down from Edinburgh and started agricultural physiology; in addition he was lecturing in pure physiology and reproduction.

P. Did Marshall influence you in the direction of reproductive physiology?

H. Yes. During all that time I was acting as demonstrator for him in his physiology classes, as well as doing research. We had not much money in those days and I had to work mainly with the rabbit. For the larger animals, all I could do was to go to slaughter houses and collect reproductive organs and study them. I remember one of my first efforts was describing a hermaphrodite pig. Assheton interested me very much by saying that during embryonic development some embryos die. I was always looking out for an agricultural application — I have always gone into pure science looking for an application to farm animals and Assheton's remark at once brought me to the idea that perhaps fertility might be affected very largely by this. I later found of course that it was so.

P. After the war you were demonstrating in the School of Agriculture, I think, because that's where I first met you.

H. Yes, when the war finished I was demonstrating, but my real job was at the Institute in Milton Road — a place which had been set up by four professors to house animals. Marshall was one of the directors. It was called the Animal Nutrition Institute, but it was really animal production generally that Marshall was engaged in, and so was I — reproduction and development. In fact, in Marshall's first book the chapters on growth, stages of development and so on, interested me as much as those on fertility.

P. That was the first edition of The Physiology of Reproduction?

* Recorded in Cambridge on 14th November 1961 with the assistance of Dr B. A. Cross.
An interview

H. Yes, I lived on that in the early days. Marshall knew all the underlying theory, and I used to worm the scientific principles out of him and then see how they could be applied. When I came back after the war they gave me a few heifers. I had four acres and three heifers, I think, at Milton Road.

P. Is that where your rabbit colony was originally?
H. My rabbit colony was there, yes.

P. I see, and you chose the rabbit because it was a small animal and you couldn’t do unlimited work with larger ones.
H. Well, I tried various animals. But because of ovulation being induced by copulation you can time things beautifully in the rabbit, far better than in rats with which you have to sit up all night to watch them come on heat. I could mate rabbits any time of the week I liked, and so we took to the rabbit — this was before the war — for fertility work. In fact, Reproduction in the Rabbit describes work all done before the first world war. Some of it was written by Marshall.

P. In that book on the rabbit you developed the idea that the development and functioning of the gonads was dependant on a ‘generative ferment’ of extra-gonadal origin.
H. Yes.

P. Did that idea derive from Heape?
H. What I wanted to discover was what made the follicles ripen and shed eggs. I always work for something to use in farm animals — that is my object, not just to follow scientific fashion. Now, Hunter took one ovary out of a sow and found that the same number of pigs were produced. Quite a number of people had found that there was no difference in the number of oocytes in high or low fertility animals, and so it was evident that the control must be from outside the ovary. We clinched the matter by taking one ovary out and finding exactly the same number of follicles as before, so that the controlling factor had to be something common to the whole animal. Looking at the literature, the only appropriate thing we could find was Heape’s generative ferment, and so I took that name. Those were the days of ferments, and I took that name because Heape started the idea that some substance in the blood was involved, and of that I was certain.

P. You didn’t have to wait very long for verification because your book on the rabbit was published in 1925 and the anterior pituitary gonadotrophins were discovered soon afterwards. You mentioned growth just now; how did such work arise as distinct from that on reproduction?
H. The last chapter in The Physiology of Reproduction interested me very much, and my family were interested in showing cattle, fattening cattle. When I looked into the literature I could find very little about growth. I wanted to apply scientific work to growth in farm animals, but I couldn’t find any work of this kind, and so I had to do some myself. All through, when I wanted to know something that wasn’t in the scientific literature, I have had to go into pure science myself to dig out the facts so that I could apply them.

P. I have been told that you once said that science was not science until it was applied. Did you really say that?
H. I meant, until it works. So much of science is just theory, and when you
try to make it work in practice it doesn’t. Major things are not got out by statistical analysis, only minor things, and when you try to apply one of these minor things in practice it won’t work.

*P.* Marshall used to say that if it’s a good result, you can see it. Do you subscribe to that?

*H.* Absolutely; if it is going to work in practice you can see it without statistics. It is only when you come to small differences that you need statistics. I always plan experiments so that you have a point at both extremes and one in the middle, and then you can see the shape of the curve.

*P.* And as part of the policy of going to extremes, you crossed a Shetland pony and a Shire horse?

*H.* Yes, I was after maternal inheritance. I am not altogether a believer in straightforward genetics. There is much more to animal breeding than genes, and I wanted to see what maternal influences there were.

*P.* May we go back for a moment to the growth side of your work. You developed techniques for estimating the relative growth of parts in so far as it affects carcass value?

*H.* Yes, we killed animals at different stages of their growth to work out total body growth per unit of time, and proportionate curves for the different tissues. They all have differently shaped growth curves. Just as the whole animal has a sigmoid curve, so all the tissues have their own curves of growth. Brain grows fastest at first, then bone, muscle and fat. That order is seen in all animals.

*P.* And every now and then you used to mobilize all your research students and have them dissect a carcass completely.

*H.* It was one of the most interesting, and I think very valuable, experiences for a research student. All the research students were pulled in whatever they were doing, and we sat round dissecting fat, muscle and bone … and the talk that went on!

*P.* I heard some of it!

*H.* We developed quite a lot of theories during those sessions. One man would say that his bone was thicker than that of the sow he did last week. What was the cause of that? Was it sex hormones, or what? During that period we came across things that were quite contrary to what breeders were saying, such as that their improved breeds were all fine-boned. In actual fact, they were coarser-boned than the wild breeds, which were very fine-boned. That follows in a general way from the planes of nutrition.

We got on to a great thing when McMeekan was rearing animals on different planes of nutrition. If you feed *ad lib.*, every tissue gets what it wants or what it is genetically capable of taking. As soon as you limit feed, then priorities are set up, and the priorities are in the same order as the tissues grow in normal development. You can starve a pig from 300 to 200 lb and its brain and bones will grow bigger despite the fact that it is losing weight. These tissues have a very high priority. Fitting into the same picture is the foetus which has an overriding priority in the middle stage of development. That priority is lost just before birth. I believe priority in this case is due to trophoblastic cells, and so whether you starve a ewe or feed it heavily during mid-stage pregnancy the weight of the lamb, or twin lambs, will be exactly the same, but in the first case the mother
will be just a wreck of skin and bone and in the second she may be as fat as a pig. The trophoblast, like the cancer cell, has priority of nutrition over all tissues except, probably, the mother’s brain. The priority is lost as soon as the trophoblast cells fade away some 2 months before birth. Then the foetus has got to contend with all the other tissues, and the size of the foetus is going to vary according to whether the mother is well fed or badly fed at this time.

P. Your interest in the relative growth of parts at different times and under different conditions was directed to producing the perfect carcass?

H. Yes. A high plane of nutrition in the early stages while the muscles are growing will give good muscle and bone, and if you later lower the plane at the time of life when fat is being put on, you can get a muscular animal with little fat. Do the reverse and feed it poorly at the beginning and you get a small animal with a stunted skeleton; then, when you feed it well later, a mass of fat is put on. Thus you can alter the composition of your animals.

P. But there is presumably a limit to what the mother can do for the foetus, as you found when you crossed a Shire stallion with a Shetland mare.

H. Yes, and the explanation is that the amount of nutriment the young can obtain depends on the size of placenta. The placenta is a nutritive organ and if it is very small the foetus won’t obtain much nourishment: if it is big the foetus receives a lot of nourishment.

P. So that in the Shetland mare, the size of the placenta is necessarily limited, and that limits the size of the foetus.

H. Yes, and exactly the same thing happened with the large South Devon cattle and the tiny Dexter.

P. If I remember rightly, you found that by the time they were adult the reciprocal crosses in such experiments were about the same size.

H. Well, that depends on the stage of development at which the young are born. In the horse, the young are born fully grown from the knee and the hock downwards, and therefore their ultimate size is determined early. But in cattle the ultimate size is only about 80% determined at birth; in sheep, it is only about 60% determined. And so, as you go down the scale, you find greater possibilities for making up afterwards.

P. So that in the case of the Shetland/Shire crosses, the ultimate result of the reciprocal crossing was not the same?

H. Oh no. I was ordered to disperse the animals during the war because horse breeding wasn’t required, but I had a little cart made for them and used it as my transport instead of a van, and so I was able to keep them until they were 14 years old. And there was quite a big difference in size, almost the difference between a mule and a hinny.

P. And that is where you came to regard the gene as of not much importance.

H. Well, I won’t say ‘of not much importance’, but there are other things than genes that affect inheritance.

P. Have you had any experience of affecting growth by the use of hormones?

H. A little. We tried during the war. Young had some growth hormone which we put into pigs, and there was a slight effect. But I didn’t go on with that because it hadn’t a commercial application. My job was to put science into practice and I didn’t do anything with growth hormone simply because I saw
no possibility of getting supplies that would be reasonably cheap to use in animal production. Of course, I have a feeling that something like growth hormone is there in the uterus.

P. Ah, tell us about that.

H. That’s still a shot in the dark, rather like generative ferment used to be. I started with foetal atrophy, but it wasn’t until I retired that I began to realize what might be the cause of it. Earlier on, I found out for certain that the cause of foetal atrophy was not bacterial. (I took, by the way, a diploma in Public Health bacteriology in my early days.) It was quite evident the atrophic foetuses were sterile; there was no question of infection. But it was not until we had done a lot of work with egg transplantation, hormones and all that, that we realized what the cause probably is. The eggs are perfectly all right: it’s the uterine environment that’s wrong.

P. It is a limited supply of some foetal growth factor then?

H. Yes. ‘Substance X’ I call it at the moment. It’s a growth substance, the supply of which limits the number of embryos that can develop, very much like the supply of FSH limits the number of follicles. This substance is in higher concentration in sheep than in cattle, and in even higher concentration in pigs, but what the substance is, I do not know except that it is a sort of growth hormone.

P. This means, then, that there must be a natural limit to the increase of fertility, or increase of litter-size, that you can get by causing superovulation with gonadotrophic hormones.

H. It depends on the species and I believe you can select for it, just as you select for twinning and triplets in sheep, by putting up FSH.

P. You haven’t made any attempt to get this growth substance into a bottle?

H. No. You see, I’ve retired.

P. Well, now, can we talk about some of your practical applications. Everybody recognizes your great work in applying science to practical agriculture. What would you say was the most important contribution you have made there?

H. I should say probably the one with the greatest effect is artificial insemination. It’s had more practical effect than anything else. I first got into it because in the old days before the first world war they had travelling stallions going about the country, serving mares at different places. Heape had done a lot with artificial insemination to cure sterility, and Marshall said, ‘Now look here, these travelling stallions are very infertile. Why not let’s try some artificial insemination, as Heape did?’ We used to have mares brought into a farm out along the Huntingdon Road, and the stallions served them there. I noticed that, when the cervix was rather small, some semen ran out and so I would draw it off with a pipette and reinsert it — the ordinary thing in horse breeding. Then I said to myself, ‘Hello, why shouldn’t we keep this stuff?’ And I can well remember that in the early days I thought it was necessary to keep the semen warm, so I fastened a tube of it under my armpit with elastic. And I used to cycle out to the University Farm to inseminate mares from this tube of semen under my armpit. It wasn’t very successful!

When I came back from the first world war, I could see there was need for rapid improvement in livestock, and that artificial insemination would be the thing. If we could inseminate large numbers of cows from our best bulls we
should be able to get better dairy herds quickly. I had no facilities for doing anything with farm animals, and so I had to work with rabbits. The main thing we were after was storage of semen outside the body. I used to collect it from the vagina of rabbits that had been mated and keep some in a refrigerator, some at room temperature, and some in an incubator. Soon after I’d started that, Walton became available. He was working with Crew in Edinburgh, on the movement of spermatozoa, and it seemed it would be better if we joined forces. He came down and repeated what I had done, but he kept the semen under liquid paraffin, to exclude oxygen. We found we could best keep semen cool, not ice-cold, but somewhere about 4º C, and we sent a thermos of rabbit semen off by ordinary post to Crew in Edinburgh. I think that was about 1922, and he inseminated some animals and got young born. By then we had a few sheep about the place and we sent some ram semen out to Poland, where I had a friend, and that was successful. We published papers on how to keep spermatozoa alive outside the body and sent them to Milovanov who was doing a lot of experiments in Russia. Later, they applied the method to farm animals and developed the artificial vagina and other equipment, and they sent us some of this gear. Walton and I tried it out and it worked perfectly well with cattle.

Then a cattle man came over from Denmark and we showed him the equipment we had got from Russia. We had also made it here with some modifications of our own. He said, ‘Oh, we’re going to try this in Denmark. We’ve got proven bulls and we’ll do a whole island’. And so they had an island on which all the dairy cattle were bred by artificial insemination and they were perfectly all right. Then a man from America came over to see it. He was very taken, and when he returned he started the practice of artificial insemination in America. But it wasn’t until the second world war broke out that we got it going in this country. Hudson, who was Minister of Agriculture at the time, came down to see us. He was a very blunt man. He said, ‘Now look here, what have you chaps got for today? No good thinking about tomorrow — may not be a tomorrow!’ (That was about Dunkirk time, you see.) So I immediately put up artificial insemination. Standing behind him was one of the Ministry men who in earlier years had turned it down twice on the grounds that though very spectacular, it had no commercial use. But Hudson said, ‘Yes, all right, I’ll get you some money. Set up a laboratory here in Cambridge and we’ll see how it works, and if you can make it work here, I’ll take it’. And it did work. So then he said, ‘Now I’m going to put through legislation to control it. I want to use it to benefit, not to exploit, the farmer. But I’m going to get opposition from two sources, will you help me?’ ‘Yes, I’ll do anything I can’, I said. ‘First’, he said, ‘the bull breeders are going to object, because it will prevent them selling a lot of bulls.’ So, I met the Cattle Breeders Association, and I pointed out that it was far better to sell one bull for £500, instead of ten for £50, because there was much more profit in it. They agreed, with no opposition. It turned out to be true, too — later on, the bulls went up to such a price that the Minister had to put a ceiling to it. ‘And then’, he said, ‘the other opposition will be the Church — on moral grounds. We’ve got to meet them.’ So I met the Committee of the Church Council, I think it was, and put the case. They attacked me first by saying that it was artificial, and nothing artificial could be of any use. It was
going against nature. I had my answer ready for that one. I said, ‘Do you drink milk?’ They said they did and that it was very good for them. I said they were being artificial — milk was meant for the calf, not for them. That settled artificiality! And then the second objection was on moral grounds. I said, ‘I am not going to argue human, I am going to argue cattle, which we want it for. Humans are not my line, but do you know what happens now with cattle? In lots of villages throughout the country, there is a long village street and a man at one end keeps a bull and a man at the other end keeps some cows. When a cow comes on heat the owner leads it along the village street to the bull at the other end. Everyone knows what is happening and all the small boys come and watch. That is what happens now. Under my conditions, the owner would phone up, a car would appear, and a man with a little black bag would do an insemination before any boy in the village knew anything about it. Which is best for morals?’ And that settled them! There was no opposition. But it is only in war-time that this sort of thing can happen; if it hadn’t been war-time, we would not have got it through. Scotland did not take artificial insemination until years later.

P. It’s going well enough now. Was it 2 or 3 years ago that the Milk Marketing Board gave a banquet to celebrate the artificial insemination of the ten millionth cow in this country?

H. Oh, it must be 3 years, I think. And, of course now it is just starting in pigs. The problem is more difficult in pigs, because ovulation occurs during the heat period, not after the end.

P. Artificial insemination enables you to exploit the reproductive potential of superior males: what can you do about superior females?

H. Well, we’ve started that. As soon as the gonadotrophic hormones came along we could produce far more eggs than could be expected to develop within the mother. But we realized that if you take them out and put them into other animals, they would have a chance of developing. And so we’ve done a good deal of work on the possibilities of egg transference. Well now, that is all right in rabbits, but when it came to sheep eggs, we found that it’s not so easy to keep them in a viable condition outside the body. Methods have been developed, since I retired, so that sheep eggs can now be kept for 3 to 4 days outside the body in blood serum, held at a much higher temperature than you hold sperms. However, it has also been found that you can introduce these sheep eggs into rabbits, and leave them there for some 5 to 6 days, or thereabouts, and then take them out and transfer them into sheep.

P. An account of this will appear in the special number of the Journal. But about the egg transfers, does it mean a surgical operation to obtain the eggs?

H. Yes, in sheep and small animals such as rabbits. But in the cow they can be obtained by washing out with special apparatus. You put a catheter in the uterine horn, just as the eggs are coming into the top of the uterine horn — that’s the timing — and you can wash them out quite easily.

P. How can you put them back?

H. Well, that is the trouble. We’ve made many attempts at putting them back — by much the same method as you use for insemination — but the first thing that happens is that you infect the uterus straight away. Awful trouble,
infection. Recently, it has been found that with the use of antibiotics, there need not be any infection, but unfortunately the egg is turned out. As soon as you touch the cervix, there are contractions, and this result precludes putting the egg in from that end. It is most important not to touch the cervix. If you put the egg into the ovarian end by surgical means, there are no contractions.

P. Would you say there's a future in egg transfer as a practical matter?

H. Oh, yes. You see, what we are doing at the moment to get beef in this country is to cross a beef bull with a dairy cow, and we get half beef. Most of our beef is coming that way now. But, if we could put a pure beef egg into a pure dairy cow, we could get very much better beef. The objects of egg transfer are, first, to increase the offspring of valuable females, and secondly to make our dairy cows produce beef calves. You couldn't do it with all your cows. All your best cows would have to be pure, you see, and you would make your inferior cows, kept for milk purposes, produce pure beef calves, rather than half-bred calves. That is the object of going on with it practically. And, of course, another thing is export. They sent those rabbits to South Africa at the cost of a few pounds, as compared with perhaps £100 or more for transferring the living animal. In fact, in a little tube in a thermos flask you could send a whole herd of cattle to Australia, for a matter of shillings.

P. Yes, I can understand that. Well now, you were talking about Australia, South Africa and places like that. You have been into almost every part of the world in an advisory and consulting capacity, haven't you?

H. Well, a lot of them, yes. I think China and Japan are about the only two really big countries I have never been to.

P. What has been your most interesting experience on these tours?

H. Moving about with the nomadic tribesmen in the Sudan, I think, was the most interesting and unusual experience.

P. How did that come about?

H. Through the United Nations Food and Agriculture Organization. Since I retired I've done two jobs for F.A.O. — making pilot surveys. I go out, survey, see what's wanted, write my report and then somebody else follows up and stays there for 3 or 4 years to do the work. I went to the Sudan first of all to organize a milk supply in the Gezira cotton-producing area — an irrigated area — and drew up plans for artificial insemination to provide a milk supply. Then, F.A.O. wanted to see how the herds of the nomadic tribes could be improved for beef production and so I travelled with the tribes to see what they were doing and I drew up a plan. All the cattle were going together and consequently the old bulls got the best choice of food, while the ones who really wanted it most — the little calves after weaning — didn't get very much. And so I suggested they should divide up their herds and let the calves go first and the old bulls last — things of that sort — and later produce some artificial grazing where the tribe could remain for a time.

Then I went to India, to Bombay State, to see how they could improve their milk production. They had adopted artificial insemination but had no good bulls to provide the semen. Just the technique and the idea of artificial insemination. Their most urgent need was for a supply of proven bulls. And behind it, of course, they wanted a good stud farm. They have no private breeders.
there; they must have a State farm of sufficient size to be able to produce really
good bulls of a milking strain.

I think my first visit abroad was one of the most interesting — at the time of
the old Empire Marketing Board. They asked me to go out to Jamaica and
Trinidad in the West Indies, to see why it was that we had sent out there millions
of pounds worth of cattle with practically no result. I went out and found that
our cattle weren’t heat tolerant; they had degenerated in the hot climate.
You have to put in some Zebu blood, then you get heat tolerance in the next
generation, and a terrific spurt in milk production. I wrote a report on that.
Eventually, they developed a new milking breed there — a combination of
Jersey and Zebu — which they call Jamaica Hope. It was Hope Farm where the
breed originated.

And of course I have been to the Continent many times. I went to Russia
in 1935 to a Physiological Congress and to the Argentine in 1936 to help them
with their meat production. In 1938 I was asked to go out to Canada and
Australia. I told them all that we had been doing in the way of research on
animal production in England and found out from them what their chief
problems were and all about their production. I had a very good time.

P. One final question. Over the years a very large number of research students
has passed through your laboratory, many of them from abroad and many of them
with little or no previous experience of research. How have you handled them?

H. Very simply — made them stand on their own feet. When a new research
student arrived, I suggested two or three different problems to him, showed
him where he could read up about them and told him to come back when he
had decided which he would like to tackle. When he’d done that, I gave him all
the advice and facilities I could and left him to get on with it.

P. Yes, I’ve heard that you used to throw them in at the deep end. Did you
have many casualties?

H. Very few that mattered. If a chap is going to be any good for research,
he doesn’t need spoon feeding. Anything else?

P. I don’t think so. Thank you very much indeed, John, for giving us this
most interesting and illuminating interview.

CURRICULUM VITAE

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Downing College, Cambridge.
Natural Sciences Tripos (Physiology, Chemistry, Botany and Geology) 1910,
and Agricultural Diploma 1911, Cambridge University.
Agricultural Research Scholar, Ministry of Agriculture, 1911 to 1914.
Captain, Norfolk Regiment, and Staff Captain, 1914 to 1918.
Inspector of Livestock, Ministry of Agriculture, 1919 to 1920.
Physiologist, Animal Nutrition Institute, School of Agriculture, Cambridge,
1920 to 1943.
Reader in Agricultural Physiology, University of Cambridge, 1943 to 1954
(retired, 1954).

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