A Commemoration of The Fiftieth Anniversary of the First Recovery of a Bovine Embryo

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This article was prepared at the invitation of the Editor, American Association of Bovine Practitioners in honor of the Toronto Meeting, 1980.

The professional lives of most members of this Association have by now been affected directly or indirectly by embryo transfer in cattle, so it seems fitting that we should not let the golden anniversary of the collection of the first bovine embryo pass without note. The recovery was made in 1930 and published the following year, first by Hartman, Lewis, Miller & Swett, and then again by Miller, Swett, Hartman and Lewis (see Fig. 1). This will not be the first appreciation of the event for, within two years of its accomplishment, the achievement was compared to the climbing of the Matterhorn (Buchanan Smith, 1932).

It would be futile to try to improve upon that vivid contemporary account of the events themselves, written by a University of Edinburgh geneticist for the Ayrshire Cattle Society's Journal. His readers would have been livestock breeders who (Buchanan Smith felt) gave the lie to the allegation ". . . that farmers are not interested in the basic facts of Science". He allowed that he could have been mistaken in this belief but continued: " . . . in case there are any readers of the Ayrshire Journal who are interested solely in what will affect their balance at the bank, let them read no further. In what follows I propose to relate rather a fine adventure in scientific research, but the tale *has no immediate bearing upon the practice of breeding better Ayrshire cows" (my italics).*

Buchanan Smith went on to compare scientific exploration with geographical exploration and to explain that scientific deductions are often made, and used in practice, without having been definitely proven. One such unproven deduction was that calves came from fertilized eggs:

"Physiologists, as the scientists who deal with these matters sometimes call themselves, had long ago guessed that this is what happened. The deduction was made from a variety of facts all of which helped to show that this was the case. But never had anyone seen the egg of the cow either before or shortly after fertilization until . . .

It was 1:10 p.m. on the 15th day of March, nineteen hundred and thirty in the Carnegie Laboratory of Embryology in Baltimore, Maryland, that there was recovered from the Fallopian tube of a cow a two-celled egg. Here at last was proof that, what we guessed did happen, actually took place.

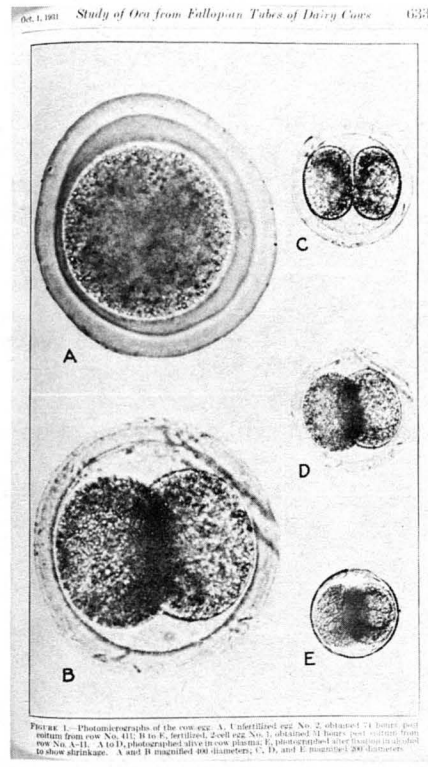

Fig. 1. The plate which, in virtually indentical form, illustrated the two 1931 accounts of the first recovery of a bovine embryo (by Hartman *et al.* and then Miller *et al.*). This photograph is of the plate in the Journal of Agricultural Research.

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The cow, No. A 11 was one of the herd belonging to the Bureau of Dairy Industry of the United States Department of Agriculture at their experimental farm at Beltsville, near Washington. She was chosen since she seemed to be a regular breeder. Her sixth calf was born in April 1929, and she came in season on February 20th, 1930 and again twenty-one days later on March 13th. At 10:05 a.m. on that day she was mated to one bull and to another some five minutes later .... Early on March 15th .... the animal was killed and her genital organs were, with the greatest care, removed, placed in an insulated, pre-warmed box and despatched (sic) by car to the Carnegie Laboratory in Baltimore. At 1:10 p.m. the egg was recovered. The explorers had reached to top of the Matterhorn".

The Ayrshire breeders read on of how the egg was filmed (in sterile cow plasma) and then fixed after it failed to divide further during four hours of observation. They were told of two more recovery attempts and of the analogies used to convey its miniscule size—the smallest visible grain of sand; 2,000,000 eggs would fill a thimble. Buchanan Smith added a size analogy of his own, saying that the egg's diameter was roughly the thickness of two pages of the Ayrshire Journal, before concluding:

> **"It is a fine bit of research. To capture such an egg is every bit as exciting as the shooting of a tiger—not to mention grouse. The finding of those two tiny eggs is every bit as wonderful an adventure as the discovery of a new land. And every bit as important, too, for it puts us right on the threshold of finding out more about the time to serve cows if you want to get calves . . .**

But that is a matter of practical importance and I said that in this issue I'd steer clear of that subject".

Buchanan Smith can well be excused not foreseeing the relevance of that "fine bit of research" to the eventual transfer of embryos because the first thoughts along these lines were only then beginning (Marshall & Hammond, 1946) and the birth of the first calf from a transferred embryo was still twenty years away (Willett, Black, Casida, Stone & Buckner, 1951). This makes it all the more interesting that Buchanan Smith, and his readers, were able to accept "scientific exploration" as research for research's sake.

The cooperation between the embryologists at the Carnegie Institution and the agriculturists at Beltsville that produced that first embryo was not new: classic studies on the early embryology of the pig (Heuser & Streeter, 1928) had been made on material obtained from the USDA. Neither did it stop in 1930, for there are further reports of cow egg recoveries in the USDA Bureau of Dairy Industry's reports for 1934 (an unfertilized egg) and 1936 (one unfertilized and one four-celled). The 1936 report also recorded the release of a motion picture "Ovulation, Fertilization and Early Development of the Mammalian Egg", but it is not clear whether the filmed bovine embryos were starred in it. Hartman, in the 1931 paper, credits the successful cooperation to his own director (Dr. G. L. Streeter) and to the Chief of the Division of Breeding, Feeding and Management of the Bureau of Dairy Industry, Mr. R. R. Graves.

The slow rate of progress in accumulating cattle embryos can be taken to show that it was not of top priority for any of the investigators involved. To understand why, we have only to consider where the "explorers" were in their widely different careers in March of 1930.

Carl G. Hartman (Fig. 2) was undoubtedly the principal "egg man" for he had already published extensively on the eggs of a wide variety of mammals (Hartman, 1929). He was

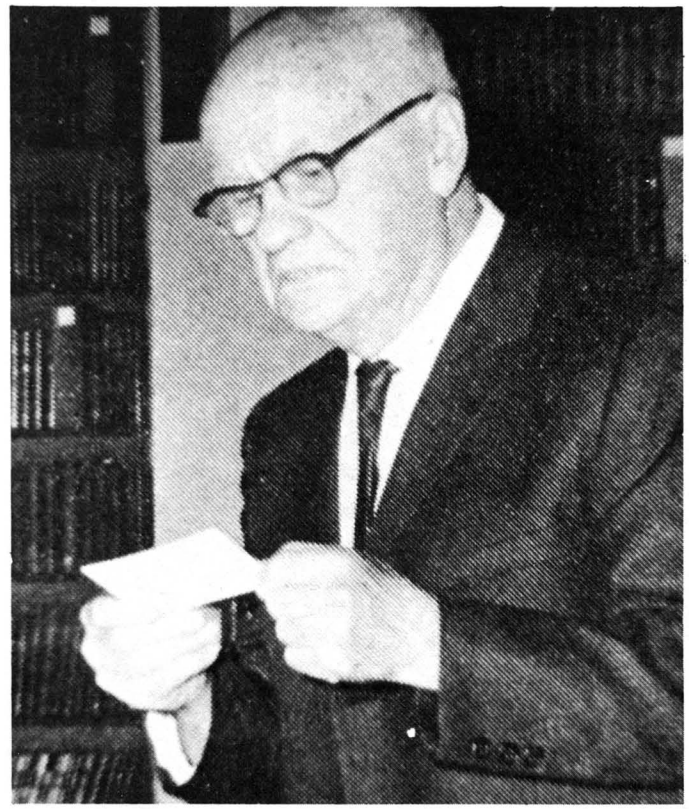


Fig. 2. Carl G. Hartman (1879-1967) during the presentation of the Marshall Medal of the Society for the Study of Fertility to him in 1964. (Reporduced, with permission, from J. Reprod. Fert. (1965) 9, 395-397).

50 at the time but most probably felt considerably younger for, in a letter to his future director Dr. George L. Streeter in 1925, he declared himself to be "46 years old according to the calendar (half that according to blood pressure and endocrine efficiency)". He was a prolific letter-writer and his correspondence with Dr. Streeter (always "Dr. Streeter", never "George") amplifies the considerable amount that has been written about this colourful giant among reproductive biologists (e.g. Biggers, 1970). His enthusiasm fairly jumps from the pages, especially when spurred by his own

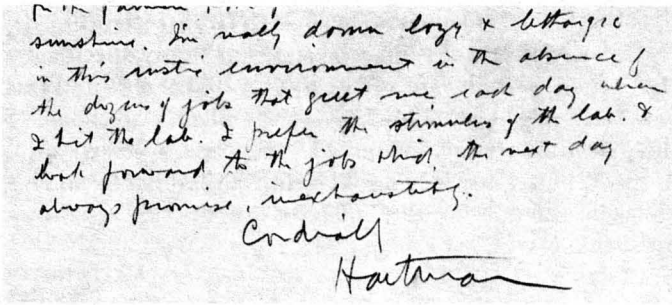


Fig. 3. Extract from a letter from Carl G. Hartman to Dr. G. L. Streeter, written on holiday in Lancaster, New Hampshire, Aug. 24, 1932, in his own pencil "kalligraphy":
"I'm really damn lazy & lethargic in this rustic environment in the absence of the dozens of jobs that greet me each day when I hit the lab. I prefer the stimulus of the lab. & look forward to the jobs which the next day always promise, inexhaustibly.
Cordially
Hartman

"kalligraphy" (Fig. 3). He reads like Mark Twain and was able to extemporise in a style which, for most of us, would represent finely-honed prose, e.g.:

"Various newspaper articles have appeared concerning my connection with the monkey business. Yesterday a St. Louis paper wired the Austin American for an extended interview with me. I have assiduously avoided newspaper men, and while I am vain enough to want the paper to mention my leaving the city, yet I am giving out no interviews and in fact shall refer newspapermen to you. I know your attitude in the matter and there is certainly nothing to say at present, for what we do not know, while it amounts to a great deal, cannot be discussed very intelligently, so if the New York American carries my picture and that of my contemporary ancestors in the magazine section, you will know that the writers who contribute this section have over-worked their imaginations".

This was from a letter to Dr. Streeter as Hartman was leaving Austin, Texas in 1925 to begin his important reproductive work in a newly—established monkey colony. In Streeter's words, he had "... been chosen as high priest to lead us up out of the wilderness of guesswork" at a salary of $5000 per annum while, for increments, he was advised by telegram to "Trust in God and Carnegie Institution". Earlier contact with the Institution had been made by Hartman in 1921, when he offered to provide opossum eggs for one of the earliest studies of chromosomal involvement in early embryonic death.

By 1930 Carl Hartman's career had already embraced the supervision of school teaching as well as biological research on trees, spiders, solitary wasps and opossums. He was to continue to stimulate reproductive research for the remaining 36 years of his life (see, for example, his "Inventory of Unanswered Questions", 1960) and the depth of his perception can be gauged from applying modern terminology to a single sentence from his notes for a presentation at a Woods Hole conference in 1934:

"One even sees references to such far-fetched but not impossible ideas as 'the internal secretion of the uterus' and 'the internal secretion of the mammary glands'."

In 1980, all of us recognize the existence of the former and exploit it in some way when we use prostaglandins; few of us would even have thought of the existence of the latter and even fewer may know of very recent evidence that even that far-fetched idea is probably true (Peaker and Maule Walker, 1980).

More conventional functions of the mammary gland were a major part of the life-work of one of Hartman's co-authors (Fig. 4). Walter W. Swett was 38 in 1930 and was Senior Dairy Husbandman for the Division of Dairy Cattle Breeding, Feeding and Management, Bureau of Dairy Industry, United States Department of Agriculture. He was a New Hampshire man who had been educated there and at the University of Missouri before starting his distinguished Beltsville career (of almost 40 years) in 1922.

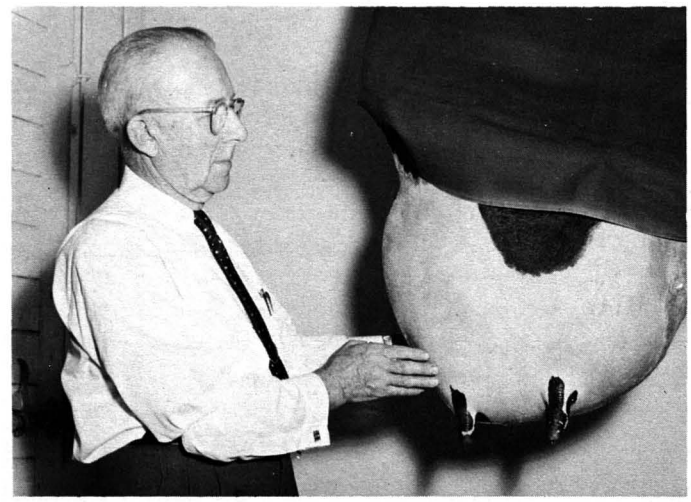


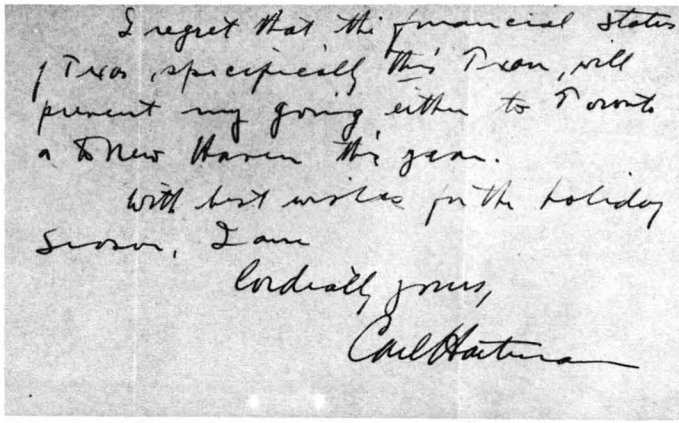
Fig. 4. Walter W. Swett—the man and his work—photographed in about 1957 (USDA photograph).

Fred W. Miller, also 38, was the clinician of the team and was styled as Senior Veterinarian and Physiologist for the Bureau during his work at Beltsville between 1925 and 1938. He had obtained his DVM from Ohio State University in 1916 and, before coming to Beltsville, had taught at Oregon College. Parasitology was another of his interests.

Warren Harmon Lewis, originally from Connecticut, was an MD who had been professor of physiology and anatomy at Johns Hopkins University since 1913. It was Lewis, 59 when the work was done and a research associate at the Carnegie Institiution since 1919, who presumably filmed the

egg. Some years previously he had pioneered the cinematography of the developing rabbit egg by combining tissue culture with motion picture techniques (see Hartman, 1929) and also applied analogous techniques to the study of normal and malignant cells. Among his other research interests, he listed the development of the arm, eye, ear, muscular system and head in man.

Thus the "explorers" were a diverse group, each member having extensive commitments to other endeavours. They were brought together, I would guess, more by personal initiatives and appreciation of what they could achieve by taking an opportunity to combine their talents than by any "mission-oriented" research programme imposed upon them. Their success certainly started something of more widespread practical application than perhaps any of them would have anticipated. For its part, the bovine embryo could hardly have had a more appropriate group of "discoverers".


I regret that the financial status of Texas, specifically this Texan, will prevent my going either to Toronto or to New Haven this year.

With best wishes for the holiday season, I am

Cordially yours,

Carl Hartman

Figure 5

In 1921, Carl Hartman, in a letter to Dr. Streeter (Fig. 5), regretted " . . . that the financial status of Texas, specifically *this* Texan, will prevent my going . . . to Toronto . . . this year". He would probably have enjoyed being welcomed to this Toronto meeting, not quite 60 years late, and this we do, in spirit, by acknowledging the contribution that he and his colleagues made to the modern practice of veterinary medicine.

Acknowledgements

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