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# A Promise Kept

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## Abstract

This article is largely biographical and relates to my experiences of the past 67 years in research and teaching, both of equal importance in my life. I was fortunate to start at the beginning of the development of instrumental methods of analysis and have eagerly embraced each new methodology as it became available. This paper is dedicated to all those students and colleagues who taught me much and whose efforts are mainly responsible for what has been accomplished in our work with food science and technology. The research focused primarily on trying to find out the "why" behind the problems that food, and especially the dairy products area, encountered over the past 65 years. The teaching has tried to foster thinking and problem solving.

## WHERE IT ALL BEGAN

I grew up in West Lafayette, Indiana, where my father was on the faculty of the Animal Science Department at Purdue University. I started at Purdue in the Department of Dairy Manufacturing in the fall of 1941. In January of 1942, because of the war, Purdue changed from two to three semesters a year, and later in that year I went to work as an undergraduate research assistant for Professor William Epple. I worked approximately 20 hours a week without pay because I wanted to find out if research was an area in which I might want to work in the future.

This was at a time when the U.S. Food and Drug Administration (FDA) was in the process of eliminating the practice of farmers setting out cream at the road for delivery to the butter factory. Cream was evaluated on the basis of high levels of water insoluble fatty acids (WIFA), which were believed to be caused by the contamination with yeasts and molds. The FDA analytical method was very time consuming and not suitable for industry quality control. Researchers at Cornell University had just published a colorimetric method, and I was asked to evaluate it. After several weeks of failure in making the method work, Professor Epple said, "I am sure you are not that stupid," and decided to try the method himself. When he failed also, he walked into his office and took a bottle out of a cupboard and said, "Try this." The dye was alpha-naphthol phthalein. In time, this method did work, colorimetric standards were prepared, and the results were comparable to the official FDA method. Work on the method was interrupted by my being called to active duty in April 1943, but later revived and published (Harper & Armstrong 1954).

## THE WAR YEARS AND A PROMISE MADE

Because of the fact that Purdue was on accelerated schedule, I was in the Advanced ROTC when called to active duty in 1943 and was sent to Ft. Bragg, North Carolina for basic training prior to going to Officer Candidate School (OCS). However, when I failed to pass an eye test I was sent to Camp Lee, VA for another basic training followed by cook and mess sergeant training. I had just received my orders to head up a kitchen for truck convoys in Italy when I got countermanding orders to rejoin my ROTC group. A few weeks later, the word came down that my eyes would not permit me to become an officer in the Quartermaster corp. Not knowing what to do with me, they put me in the basic Army Specialized Training Program (ASTP) in Engineering at Purdue. I took a loss of rank to private to do so, but it sounded like a good deal. However, five weeks later the basic ASTP was shut down and 80,000 prior noncommissioned officers were sent from five universities to fill the ranks of infantry training divisions. Thus, I became a member of Company G, 407th Infantry Battalion, 102nd Infantry Division. I had a third basic training and then combat training. In August of 1944, the division was sent to Europe. Subsequently, I was wounded in action in November 1944 just inside Germany and was told at the time that I was the first "sub-total gastrectomy" in the European battle zone. When I was injured, there was no one within 100 feet, but I heard a voice that said, "Everything will be all right." My reaction was to say the Lord's Prayer and promise that I would spend my life teaching and searching for new information.

In June of 1945, I was given the choice of taking a medical discharge or a 90-day furlough and report for action as a replacement for the pending invasion of Japan. I took the discharge!

## BACK TO PURDUE

Three days after my discharge, I went back to school at Purdue University. At that time, there were no graduate students in Dairy Manufacturing, so Dr. Paul Elliker was looking for an undergraduate student to do work on the efficacy of a relatively new germicide called quaternary ammonium

chlorides, nicknamed "quats." He offered me an assistantship to work for him on the project. The owner of a quality control laboratory, Bill Mosely, provided funding for the project and was especially interested in the development of a rapid, quantitative method for the measurement of the quaternary ammonium germicides. In the presence of an acid/base indicator (phenolphthalein), they would turn colorless with addition of an acid. However, all the acids I tried (phosphoric, hydrochloric, sulfuric, acetic, and lactic) failed to give reproducible quantitative results. One night, while working in the lab, I spotted a bottle of citric acid crystals and thought, "Maybe this would work." After making up a 0.1 N solution, I found that it gave reproducible results. I couldn't figure out why and contacted two organic chemists at Purdue whom I knew (M. G. Nelson and R. F. Mellon). Neither of them could answer why only citric acid worked. To this day I have never answered why it worked. The test became a quality control test for measuring the concentration of the "quats," and was used in the dairy industry for nearly 10 years (Harper et al. 1948).

On September 20 of 1945, I married my high school sweetheart, Eloise, who by that time was also in Dairy Technology. We had been engaged since 1943. After the wedding in the middle of the semester, we took a week-long honeymoon. After our return, I received a letter from the VA telling me that I was AWOL from the university. I was on the GI Bill at the time.

Dr. Elliker became my mentor. One weekend, he took me to his home in Madison, Wisconsin and when we drove by the University, he told me that this was where I was going to go to graduate school. He planted the seed that led me to Wisconsin for graduate study in the summer of 1946.

## ON WISCONSIN

When I arrived, the graduate students were few in number (12), but all had been in graduate school before service or had been found 4F. I felt rather stupid in comparison. My advisors were Dr. Arthur Swanson and Dr. Hugo Sommer. Swanson had just recently joined the department from industry and Sommer was "a legend in his own time." He had published several books and numerous scientific papers and was just 50 years old. Approximately a week after I started on the MS degree, Dr. Sommer brought me a piece of Cheddar cheese with small white specks in the seams. He explained that Arthur Dorn and F. L. Dahlberg of Cornell University thought that these specks were tyrosine and that Stewart Tuckey and others at Illinois thought that they were calcium lactate. Both used quite different methods: the Cornell work was based on chemical analysis for tyrosine, and the Illinois work was based on the use of X-ray diffraction spectroscopy. Dr. Sommer wanted me to find out who was correct. The Soils department had X-ray diffraction equipment that they would let me use. Subsequent analyses were made of white specks from six different aged Cheddar cheeses ranging in age from 14 to 15 months (Harper et al. 1953, Shock et al. 1948). Many contained both calcium lactate and tyrosine. Tyrosine was not detected by X-ray diffraction at concentrations less than 20%, but was detected by microbiological assay. Neither calcium lactate nor tyrosine was detected in one cheese that contained leucine and cystine. Overall, the results suggested that any compound that exceeded its solubility product could form the white particles.

Work on the amino acids associated with the white particles in Cheddar cheese got me interested in their role in the ripening of cheese. Paper chromatography had been recently reported by Martin & Singh in 1942. I tried to use this method on water extraction of cheese, using ninhydrin to visualize the amino acids. Attempts at that time to quantitate the amino acids were not successful. A graduate student friend in biochemistry told me that a microbiological assay for amino acids was being utilized. It turned out that one of the key developers of the methodology was Dr. Esmond Snell, who was a professor in the Biochemistry Department. By 1946 the method had become well established. It was based on the use of lactic acid bacteria mutants that were deficient

in an amino acid required for growth. Acid production was used to determine the growth of the organism and to establish the concentration of the amino acid in the sample. I used water extracts of cheese, and each sample was evaluated at five different dilutions. It was necessary to build an automatic filler for the media and an automatic titrimeter for titration. Using the microbiological assay, we were able to show that amino acids were related to flavor, which was evaluated by a sensory panel consisting of five faculty and staff. Approximately two years into this research, I began to wonder about the availability of simple peptides to provide the essential amino acids required by the bacteria. I ordered several di- and tripeptides from Germany for evaluation. All the peptides gave higher values as lower concentrations. Glycyl-glycine had the greatest response, approximately 1500 times that for glycine at low concentrations. I went to see Dr. Swanson about my findings. He told me, "I think you will have to pick another problem." I then went to see Dr. Sommer. Sommer lit a cigarette, as he always did, and gave me a 15-minute lecture on the value of negative research. Then he asked me to get my data book. To this day I would swear that he only opened the book and handed it back. Finally, he said, "I think you should take a longer look at your data. You may not have a problem." That was on a Friday afternoon. I looked at the data all evening, then again on Saturday and also Sunday. It wasn't until Sunday afternoon that I saw what he meant. Water extracts of cheese were analyzed at five different dilutions. Results for the five dilutions were always converted to give a concentration per unit volume. Whereas glycyl-glycine gave 1500 at the highest dilution, the response decreased at lower dilutions. Although glycine and the di- and tripeptides gave a "dose response," this was not observed for glycine or for any of the amino acids in any of the cheese extracts. I learned a valuable lesson from this—never jump to conclusions and reserve judgment until you have all the facts. This is just one of the many lessons learned from Dr. Sommer; others included tolerance and patience.

In 1948, I gave my first presentation on the role of amino acids in cheese ripening at the American Dairy Science Association annual meeting in Raleigh, North Carolina. After I finished, both Dr. Dahlberg and Dr. Kosikowski took turns, telling me that what I had reported couldn't be true. It was quite a shock to a "green" graduate student to be challenged by people well established in cheese ripening. I never knew what I replied, but I was told later by several people that in essence I said, "If you had paid attention, you would understand." I have to admit that it was with some satisfaction to have Dr. Kosikowski confirm our findings five years later. To my knowledge, this was the first study to relate free amino acids to Cheddar cheese flavor, albeit we did not understand the full significance of the relationship (Harper & Swanson 1948, 1949; Harper 1949). Today, it is well recognized that amino acids play a role in the taste of Cheddar and other cheeses, and that the degradation of amino acids leads to important aroma compounds.

As I was finishing up my PhD degree in 1949, one of the offers I received was from the Ohio State University. When I asked the opinions of the professors in the department, I was told by everyone but Dr. Sommer not to go there. Sommer told me that although Ohio State's Department of Dairy Technology had an excellent teaching program, they had never had a significant research program. However, he believed that their new department chair, Ira Gould, was an excellent researcher and would seek to change the direction of the department. He then said that Ira Gould "would be very difficult to work for, but he would be fair." Finally, he told me that he thought that I wouldn't find a place in the country that could provide a better opportunity for professional development and to build a strong research program.

## BECOMING A BUCKEYE

On July 5, 1949, as a new PhD, I joined the Department of Dairy Technology at the Ohio State University. Although I had a nominal appointment as an Assistant Professor in Dairy Technology,

**Figure 1**

K. S. Ramachandran, Earl Poling, Ira Gould, and Jim Harper discussing chocolate research, 1960.

most of my funds came from a United States Department of Agriculture (USDA) research grant that Dr. Ira Gould (**Figure 1**) had obtained to start building a research program. Actually, I was an "agent" of the USDA assigned to Ohio State to determine why the Italian cheese industry in the United States had lost the ability to produce flavor in their Italian cheeses after the Food and Drug Administration had put an embargo on rennet paste because of unsanitary practices.

On that day in July, I really didn't know how to proceed with the project. And certainly I did not know that, except for an 11-year absence, I would still be active at Ohio State in 2009, 60 years after I started.

## RESEARCH AT THE OHIO STATE UNIVERSITY: 1949–1980

### Early Years in Research

To try to understand the nature of the problem causing the loss of flavor of Italian cheese, it was necessary to become familiar with the Italian cheese industry. The Italian cheese industry was centralized primarily in Wisconsin at that time. The major cheeses included Provolone, Romano, and Parmesan. Dr. Walter Price at the University of Wisconsin helped me make contact with manufacturers of Italian cheese, and for the next three months most of my time was spent in Wisconsin. Joe Sartori of S&R Cheese took me under his wing, and I learned how to manufacture both Provolone and Romano. In comparing cheeses by sensory evaluation that were made before and after the rennet paste embargo, it became apparent that the cheese made without rennet paste lacked butyric acid. Rennet paste is the product that contains both the stomach and its contents. Rennet extract made from the stomach lacked the stomach contents. During my time in Wisconsin, I became reacquainted with Merle Farnham, who was a frequent visitor to both Sommer and Price. He was making a product in his garage from a gland at the base of the tongue of calves and kids that he claimed contained the key to what made rennet paste so useful. This product was thought to be rich in lipase. Just after this, Merle formed Dairyland Food Products, which later became a major supplier of oral lipases.

To prove that fatty acids and lipase in the rennet pastes were essential to Italian cheese flavor, we needed a method to measure the lower molecular weight fatty acids. No suitable methods were

available at this time. Money was not readily available for equipment, and we learned to make much of what we needed. The glassblowing skills gained in Sommer's lab were of considerable value.

After looking at and discarding paper chromatography, we set out to develop a column chromatography method. By today's standards, this was a very crude setup. Over a two-year period, a silica gel method was developed to separate the lower molecular weight fatty acids, permitting measurement of acetic, propionic, and butyric acids individually and caproic, caprylic, and capric acids as a group. We used silica gel and a gradient of butanol in chloroform. The room we used was not ventilated, and my wife frequently complained about the odor when I got home. The column was 30 × 300 mm fitted with a glass frit and located over a small funnel. Initially, we collected 10 ml fractions in 100 ml graduated cylinders. It became immediately apparent that we needed a fraction collector. We couldn't afford a commercial fraction collector, so we built our own. This was based on Masonite circles with holes drilled for holding 40 tubes, a clock spring, and siphon to activate a solenoid to release 5 ml into a test tube and allow the collector to move one position. Eventually, we built 10 of these at $25 each. The silica gel method for fatty acid extraction became the standard method in the dairy industry around the world until it was replaced by gas chromatography (Harper 1952, 1953; Harper et al. 1956).

Using this method, we confirmed that lower molecular weight fatty acids were indeed responsible for the unique flavor of Italian cheeses and that lipases in the rennet paste were positionally selective for the high proportion of the lower molecular free fatty acids formed. The lipase from the tongues of ruminant animals was the source of the lipase. Kid lipase was especially good because it produced a high percentage of butyric acid (up to 40 mole percent of the total) (Harper 1955, 1957; Harper & Gould 1952; Harper & Gould 1955). Subsequently, it was shown that the Italian cheese flavor resulted from a balance of lipolysis and proteolysis (Harper & Long 1956; Long & Harper 1956a,b).

The proteolysis was related to the starter cultures. We found that heat-tolerant lactobacilli were essential to the ripening process, leading the starter culture industry to provide the cultures that replaced the practice of using the whey from one batch of cheese to make the next, thus minimizing bacteriophage problems (Harper 1970a; Harper et al. 1952a,b).

## Clean in Place (CIP)

We moved to Vivian Hall in 1951. During the 1950s, there were major changes in the dairy industry, including the introduction of refrigerated tanks on the farm, coupled with delivery of milk from the farm to the factory in bulk tank, a shift from vat to continuous high-temperature, short-time (HTST) pasteurization, homogenization of milk, introduction of paper cartons, and the beginning of cleaning in place (CIP). The changes made it possible to move milk large distances and eventually eliminated the home delivery of milk that was common prior to this time. My office was next to that of a graduate student and instructor of our engineering courses, Dale Seiberling. Together we were involved in the early work associated with the development of clean in place practices in the dairy industry and later the food industry. This research resulted in only one trade publication and a *Journal of Dairy Science* abstract (Seiberling & Harper 1957a,b). Reviewers of major journals at that time rejected the research as being "too speculative." However, it turned out that this research was, in my opinion, my most significant because it helped to form the basis for the automation of the dairy and food industry.

This research had its beginning over a cup of coffee in 1952. Dale had just returned from a class in industrial engineering and joined Chuck Tibbets and me in my office. Dale excitedly stated, "We can do everything in the dairy industry automatically but make cheese." Tongue in cheek,

I replied, "We can do that too." Chuck Tibbets, a technical sales representative for the Taylor Instrument Company, made monthly trips to Ohio State to consult with Dale and continued to ask, "Where is your automated dairy factory? Where is your automated cheese process?" After a number of such trips, the idea took place that these things could indeed be done. Dale considered that the key to automation was to develop an automated valve that could be cleaned in place. One evening over dinner, Dale drew the basic designs for three different valves on the paper tablecloth. A formal research proposal made to the department chair was denied. He concurred with the view, common at that time, that "no automated valve could be designed that could be cleaned in place; if it could, no one would buy it because of cost; and if anyone would buy it, the health department would never accept it." The decision was made to continue the research with Chuck Tibbets's assistance. By this time, I was very much involved in the early stages of using radioisotopes as a research tool. It seemed to us that this could give us the sensitivity required to prove cleanability. Initial study showed that $P^{32}$ was not satisfactory because it bound irreversibly to the stainless steel; $Ca^{45}$ was found to be suitable and could be expected to be an integral part of the milk soil. A test rig was designed for the study. Tibbets provided the flexi-timer and contracted with Tri-Clover for construction of the valves. Although approved by the university radiation safety officer, I have no doubt that we would never get permission today.

In addition to the work with CIP, an automatic cheese coagulation method was developed and patented (Harper & Seiberling 1957). Because of regulations in place at the time barring exclusive licensing of patents, it was never commercialized.

## Other Method Development Work

Other chromatographic methods were also developed or adapted in subsequent years and applied to cheese and other dairy products, including ion-exchange chromatography for free amino acids (Hamdy et al. 1955), paper chromatograph for carbonyl compounds (Bassett & Harper 1958, Harper & Bassett 1958), and our first attempt at a gas chromatographic (GC) method for fatty acids and amines in a homemade gas chromatograph that used an automatic titrimeter as the detector (Hankinson & Harper 1958, Harper et al. 1961, Cole et al. 1961).

## Use of Radioisotopes

Radioisotopes became a useful research tool starting in the 1950s and continuing to 1981. I became involved because it looked like an easy way to get at complex information, especially as related to chemical changes and microbial metabolism. The use of $Ca^{45}$ to prove the cleanability of automated valves was the impetus to expand the radioisotope research. Most of the work utilized $C^{14}$ and $S^{35}$. As usual, lack of equipment made initial work difficult and slow.

The first application of radioisotope to cheese-related work utilized $C^{14}$ in a simple cheese slurry system to accelerate ripening. In the beginning, we made uniformly labeled amino acids using $C^{14}$ glucose and bean sprouts. The radioactive free amino acids were isolated by ion exchange chromatography. This was the first of a number of subsequent studies to begin to expand our knowledge of the biochemical changes occurring during cheese ripening. As an outgrowth of a study of carbonyl compounds in cheese and cultures, investigation was made of the ability of the fermentation system to utilize different substrates, including acetate, citrate, succinate, malate, glyoxylate, and oxalacetate using $C^{14}$-labeled compounds (Bassett & Harper 1958).

One area of research centered on $S^{35}$-labeled milk using sodium sulfate, sodium sulfide, or barium sulfide introduced into the rumen of a cow maintained at Ohio Agricultural Research and Development Center (OARDC) in Wooster (**Figure 2**), or by the injection of $S^{35}$-labeled

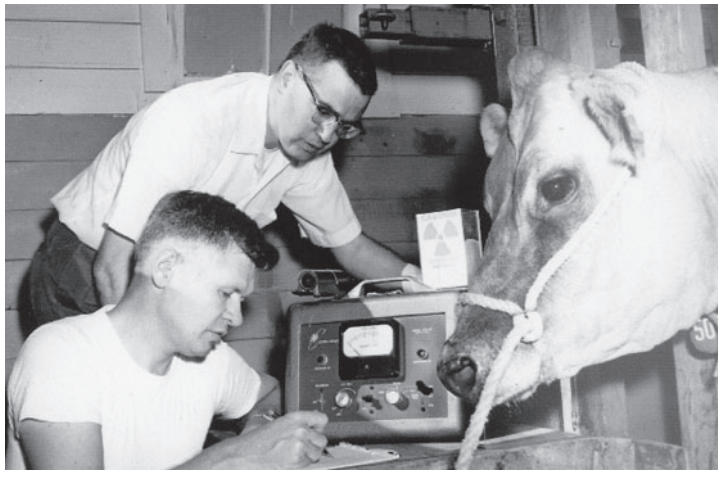

**Figure 2**

Jim Harper (OSU) and Russell Conrad (OARDC) with a radioactive cow at OARDC in Wooster, Ohio, 1966.

methionine or cystine. In addition to studying the time sequence for the labeling of individual milk proteins (Pereira et al. 1966, Harper & Gould 1967, Harper 1970b), the radioactive milks were used to study heat-induced changes during the heat treatment of milk, cream, and skim milk (Harper & Pereira 1970; Pereira et al. 1968; Pereira et al. 1970a,b) and the utilization of sulfur amino acids by starter cultures in cheese and fermented products (Harper 1965, 1970a; Harper & Gould 1967; Harper & Kristoffersen 1970; Harper & Wang 1980a,b, 1981; Harper et al. 1980b, 1979; Kristoffersen & Harper 1966).

With homogenized milk becoming increasingly popular, exposure to sunlight had become a major issue. The consensus of opinion was that the oxidation of methionine of casein to methional was responsible for sunlight off-flavor of milk. By adding $S^{35}$-labeled cystine to milk, exposing it to light, and trapping the volatile sulfur compounds released into a series of traps, it was possible to show that the methionine was degraded to mercaptans and sulfides (Harper & Brown 1964, Samuelsson & Harper 1961).

In 1960, I had a sabbatical at the Dairy Research Lab in Lund, Sweden and took radioactive compounds with me on the airplane with no problems. What wasn't used I brought back and declared on entry at New York. This was in the middle of the night, and checking was done with a Geiger counter. It went crazy, even at the far end of the airport. At 4 AM and with much consternation, they discovered their Geiger counter was broken. A new one showed no activity, and we were allowed to proceed with our radioactive materials.

## Accelerated Cheese Ripening

Kristoffersen et al. (1967) utilized slurries of Cheddar cheese to accelerate cheese ripening. As a result, ripening was accelerated at the rate of approximately one month per day (Harper & Kristoffersen 1970). A series of studies included protein degradation (Harper et al. 1971b), free fatty acid development (Harper et al. 1979), carbohydrate metabolism (Lin et al. 1979), esterases (Harper et al. 1980a), protein degradation (Harper et al. 1970a), and amino acid catabolism (Harper & Wang 1980a,b; Purvis et al. 1963). This research helped to form a basis for development of enzyme-modified cheeses.

Research almost always follows the availability of funding. In the 1960s and early 1970s, NIH and NSF funds were generally easy to obtain for food research. We had a number of government-funded projects. These included research on antibiotics, spore-forming organisms in milk (Harper 1960; Martin et al. 1962, 1966; Martin & Harper 1963, 1964, 1965a,b; Mikolajcik et al. 1963; Mikolajcik et al. 1965a,b), and immobilized enzymes (Okos & Harper 1974, Valeris & Harper 1973).

By the late 1970s, these funds were much more difficult to obtain for research related to food quality and processing. A project sent to the NSF would come back with the comment that it was too practical. The same project submitted to the USDA would come back as being too fundamental. My cheese and other flavor research was only funded by USDA Hatch funds by the middle half of the 1970s.

## Environmental Research

The dairy industry, including the cheese industry, had been undergoing strong consolidation since the introduction of bulk-tank milk handling, resulting in the emergence of whey as a serious environmental problem. With $BOD_5$ at 36,000 ppm, the waste strength of whey was 189 times that of human waste. Two significant things occurred in 1969. The Clean Water Act was passed, and I visited the first "loose" reverse osmosis pilot plant at a cottage cheese factory in upper New York State, which had been installed by the Haven's Company to reduce lactose and produce a 34% whey protein concentrate. Both of these were to change my life more than I could know at the time. John Blaisdell, a food engineer in the department, and myself got a grant from the United States Environmental Protection Agency (EPA) to study the "state of the art" of dairy wastes and their control and treatment. We wrote a 559-page report entitled "Dairy Food Plant Wastes and Waste Treatment Practice" that was published by the EPA in 1971 (Harper et al. 1971a) and became the foundation upon which the Effluent Limitation Guidelines and New Source Performance Standards for Dairy Product Processing was established in May of 1974. For the next 10 years significant attention was placed on environmental research with emphasis on processes (Carawan & Harper 1980), control (Harper & Carawan 1978), treatment (Harper & Chambers 1978), and management (Harper 1985).

## The Whey Dilemma

In 1970, we obtained a Haven's pilot plant with a true ultrafiltation membrane at no cost to evaluate the efficacy of the new technology (Harper 1980, Harper & Moody 1981). This was the beginning of my research to develop a "value-added" product from whey.

Discussion with others (both in the United States and in dairy exporting countries) interested and concerned about how to solve the dairy fluid waste problems, especially whey, led to John Woychik (USDA), Walter Dunkley (UC Davis), and myself making an application to the National Science Foundation to support a "Whey Products Workshop" with funding from the United States/New Zealand and United States/Australia Joint Research Funds set up during World War II. We were successful, and the first workshop was held in Columbus, Ohio, in 1975. There were attendees from universities, government research agencies, and industry from the United States, Canada, Ireland, Australia, and New Zealand. The discussions lasted for three days and concentrated on ultrafiltation as a means of making whey protein concentrates and on protein functionality. The New Zealanders and Australians were already involved in pilot plant studies at that time.

As a result of contacts made at the Whey Products Workshop, I was invited to spend a six-month sabbatical at the Commonwealth Scientific and Industrial Research Organization (CSIRO) in

1977. Much of this work was to continue and expand on the concept that the protein functionality alone could not predict the success of whey proteins in a food product application (Muller & Harper 1979, Harper 1980, Harper & Zadow 1984). Key people with whom I interacted included John Pearce, Greg Zadow, and Laurie Muller.

A second Whey Products Workshop, also funded by the National Science Foundation, was held at the New Zealand Dairy Research Institute in 1979 in Palmerston North, New Zealand, and attended by researchers from New Zealand, Australia, the United States, and Canada. The papers from this workshop were subsequently published in an issue of the *New Zealand Journal of Dairy Science and Technology*, 1979.

Mike Mathews (New Zealand), Laurie Muller (Australia), and I made the first attempt in 1979 to develop a "whey protein collaborative research program" made up with contributors from the Ohio State University, Pennsylvania State University, Clemson University, New Zealand Dairy Research Institute (NZDRI), and the Australian CSIRO Dairy Laboratory. This got off to a rather shaky start the first year, until we realized that we had to do a better job of communicating. Eventually, quarterly conference calls and biannual meetings at different locations resulted in meaningful collaboration and led directly, or indirectly, to more than 100 publications. The "Collab" effort lasted until approximately 1993 and has been credited to a large extent for the development of functional whey protein concentrates and isolates.

## THE NEW ZEALAND CONNECTION

One November night in 1980, it took me two hours to drive home in a snowstorm for what was normally a 30-minute commute. The phone was ringing as I entered the house and a well-known voice asked, "Jim, what would it take to get you to come to New Zealand for a year?" My answer to the NZDRI Assistant Director was, "Kevin, tonight not much." I asked for a year's leave the next day, and it was turned down with the comment, "Why do you want to do something stupid like that?" My reaction was to retire, and in April 1981, I started in the NZDRI Whey Products Section, working with a team of researchers that had an average of only six months of experience in whey products. What became readily apparent was that every kilogram of whey protein that had been produced was still in storage. The one year became two, then three, and finally after five years my wife decided it was time to go home and get to know the grandchildren. By the end of the five years, I was heading up the section and had more than twenty researchers. It was a real team effort bringing together protein chemists with chemical engineers, microbiologists, and food technologist to learn how to manipulate the process to meet the needs for different food products using model foods systems as a key to understanding processing-induced changes in functionality (**Figure 3**). During the five years, more than 100 reports were written, but there were only a few publications on model food systems (Harper 1984) that later became a book chapter (Harper 2009) and several on applications of whey protein concentrate (Ennis & Harper 1986, Huse et al. 1984, Mangino et al. 1987, Marshall & Harper 1988). After moving back to the United States, I continued to spend three months a year in New Zealand, working first at the NZDRI, then Fonterra, and for the past three years at the Riddet Institute at Massey University.

### Into the Wilderness

Upon returning to Galena, OH in 1986, I decided to do consulting work. This continued for the next six years and included working three months a year with the NZDRI in New Zealand, several months a year at New Zealand Milk Products in Santa Rosa, CA, several months a year with the Wisconsin Dairy Research Center, working closely with Norm Olson, some time with the

Wisconsin Milk Marketing Board, and time with other dairy and food industries. The consulting work lasted until 1992.

## ONCE A BUCKEYE, ALWAYS A BUCKEYE (1992 TO THE PRESENT)

During the major recession in the early 1990s, the Department of Food Science and Technology at Ohio State was having difficulty finding someone to teach some of their required courses.

In 1992, I received an invitation from the chair of the department to volunteer to teach two required courses—Introduction to Food Processing and Physical Properties of Food. After doing this for a bit more than a year, I was offered the new J.T. "Stubby" Parker Endowed Chair in Dairy Foods. Seventeen years later, I still hold the Chair position. The past 17 years have been both challenging and rewarding.

Initially, my research focused around functional properties of dairy foods. Model food systems, which had been such a major focus during the development of whey proteins in New Zealand, continued to play a significant part of the research. This included investigations of the functionality of milk proteins and their relationship to the characteristics of food products. (DeCastro & Harper 2001, 2002; DeCastro-Morel & Harper 2003; French & Harper 2001; French et al. 2002a,b; Kuo & Harper 2003a,b).

### Polyfunctional Thiols

Polyfunctional thiols, thiol compounds with 3–6 carbon units and a functional group such as a ketone or aldehyde, have been shown to be important at the parts per trillion/parts per billion (ppt/ppb) levels in a variety of foods (Vermeulen et al. 2005). The major challenge of working with this class of compounds is the lack of pure reference compounds. A method developed using a chemiluminescence detector was applied to aged Cheddar cheese and found a number of previously unknown polyfunctional compounds on the basis of matching with Kovat's indices (Kleinhenz et al. 2005, 2006, 2007). The only polyfunctional thiol that could be proven, based on GC-MS, was 4-mercapto-4 methyl-pentan-2-one.

### The Electronic Nose

In 1994, the first electronic nose was introduced at the annual Institute of Food Technologists (IFT) meeting. I was intrigued with the potential of this technology and investigated its application, especially to cheese, for the next 10 years. We obtained our first electronic nose in 1995 (**Figure 4**). Lack of full understanding of the mechanism of the first instrument resulted in erroneously, but diligently, measuring differences in the moisture content of cheese. Later by changing the type of sensors utilized we were able to differentiate the flavor of food products in a meaningful way. Although our work and that of others showed that we could differentiate on the basis of sensory differences (Harper 2001, Harper & Kleinhenz 1999, Jin & Harper 2003, Jou & Harper 1998), the early approaches were unable to provide a means to provide a chemical basis for differentiation. Shifting from organic polymer and metallic sensors to mass spectroscopy provided some elucidation of the functional groups involved in the differentiation (Jiamyangyuen & Harper 2004a,b). The use of chemical ionization, using a single soft ionizing reagent ion, was a step forward. However, compounds of the same molecular weight gave the same product ions, and thus many of the flavor compounds could not be fully differentiated from each other (Drake et al. 2003).

## The Swiss Cheese Consortium

The state of Ohio is first in the country in Swiss cheese production. In 2001, a group of Swiss cheese manufacturers, culture companies, and distributors formed a consortium and asked the Ohio State University to focus on the research needs of the Swiss cheese industry. This research has focused especially on the interactions of the starters and nonstarter lactic acid bacteria associated with Swiss cheese quality. In addition, new methods of analysis have been utilized to get a better understanding of the factors affecting Swiss cheese quality. It is quite clear that there are differences caused by different strains of starter cultures, the factory producing the cheese, and cheese-processing factors (Jenkins et al. 2002, Kocaoglu-Vurma et al. 2008).

In 2004, I was asked to present a paper at an American Chemical Society–sponsored symposium on my flavor research. I based it primarily on the methodology available in each of the decades since the 1940s. I gave a prepresentation to the department and mentioned that one of the new methods that might be useful in the current decade was that of Fourier transform infrared spectroscopy (FTIR). However, the current methodology is a far cry from what was available 40 years ago. Luis Rodriguez-Saona had just recently joined the department from the FDA and brought the newest FTIR technology with him. As it turned out, Nurcan Koca, a visiting researcher from Turkey working with me on cheese, came into my office two weeks after the meeting and asked, "What do you think of this?" "This" was the result of the use of FTIR to directly measure the lower molecular weight fatty acids in Swiss cheese (Koca et al. 2007). Thus started research that has been very useful in the differentiation of strains of starter cultures, the differentiation of both Swiss and Cheddar cheeses on the basis of factory, cultures, and sensory characteristics (Rodriguez-Saona et al. 2006, Kocaoglu-Vurma et al. 2009, Chen et al. 2009, Subramanian et al. 2009).

## The Newest Challenge

In January of 2008, I was asked to visit a company in Christchurch, New Zealand, to consider helping them to evaluate the potential of a fairly newly developed Selective Ion Flow Tube Mass Spectrometer for determination of the volatile organic compounds (VOCs) in the flavor of foods.

The method has the potential to identify and measure the VOCs in the head space of a sample on a volume basis at the ppb/ppt concentration without any sample preparation and in real time. The instrument used three separate reagent ions that could produce different reaction products with a given compound, thus markedly reducing the previous problems of conflicts. My concern was that although its sensitivity was its greatest potential benefit, it could also be its greatest limitation. To get quantitative values, it is necessary to run each compound in the method to identify the mass products formed and to obtain reaction rate constants for each product. Knowing that foods contain many hundreds of VOCs, it seems probable that the unidentified masses in the sample might limit its application. However, as it turns out, we have been able to differentiate significant factors with only 35–50 compounds in the method. Other research in progress involves the application of high-pressure processing to improve eye formation in Swiss cheese and a revisitation of factors affecting the efficacy of CIP (**Figure 5**), especially as related to biofilms.

# TEACHING

Over the years, teaching became a passion equal to or more important to me than research. Initially, my teaching was limited to a single course, and it was approximately six years before I became more fully involved. My involvement in teaching markedly increased when the Department of

Food Science and Nutrition was formed at the time of the retirement of Ira Gould. The shift from Dairy Technology to Food Science and Nutrition occurred over a weekend in July, 1971. The 12 Dairy Technology faculty and the three Institute of Nutrition faculty combined to form the new department. As the Chair of the Curriculum Committee at the time, I ended up writing most of the syllabi for the new and revised courses. Many new courses were added and many were modified, such that market milk became fluid food and ice cream became frozen food. New courses included food additives, technical problem solving, instrumental method of analysis, physical properties of food, food waste control, and management, among others. When it came time to teach the courses, there was great reluctance on the part of many of the faculty to become involved. I started out teaching eight different courses a year, although a number of advanced courses were taught on an every-other-year basis. Ten years later, I was still teaching six. Over the past 60-plus years, I have taught more that 15 different courses, and more than 2000 students have passed through my classes. In addition, I have advised more than 55 MS and 40 PhD students. Undergraduate courses taught have included senior seminar, market milk, refrigeration, dairy food plant layout and design, dairy chemistry, food chemistry, fluid foods, food waste management and treatment, technical problem solving, physical properties of food, introduction to food processing, product development, and food additives. Graduate level courses have included food proteins, food fermentation, instrumental method of analysis, and instrumental method of analysis for biochemists. (I had a joint appointment in biochemistry from 1971 to 1981.)

However, from the beginning there was a desire to teach more than facts and to challenge students thinking and problem-solving skills. This did not happen quickly or easily. Testing the ability of a student to apply knowledge gained in a class to a new situation was a great challenge. Many different approaches have been used to attempt to assess the ability of a student in respect to critical thinking.

My slow move to find a better way from personal experience started early in my teaching career when my father (then Head of the Department of Animal Science at Purdue University) was visiting the Dean at Ohio State and stopped by my office. He asked what I was doing, and I said that I was working on an examination. He asked if he could read it. The next thing I knew it landed in the waste basket, with the comment, "A student doesn't have to think to pass that exam." This was the beginning of trying to develop examinations that required more that the regurgitation of facts. What was desired was not only a measurement of knowledge gained, but the ability of the student to apply that knowledge to a new situation. Attempts to develop written examinations to challenge thinking were not overly successful. In fact, my first attempts resulted in an average of 25 points out of a 100, with the highest score being 52.

Early attempts at oral examinations were not much more successful than those for the written examination. However, my testing procedures evolved over time and have proved to be useful not only as a way to gauge student knowledge, but also as learning experiences for the students.

The first application of an oral final examination was for a course in Food Additives. The general approach was to develop situational questions that only the highest-achieving students could be expected to answer. If a student could not immediately answer the question, then the next question was based on the knowledge necessary to answer the original question. The questioning would continue in a stepwise manner until the simplest question relating to definitions was asked. Another approach was to start with simple questions and build to increasingly more difficult questions as the student demonstrated his/her ability.

It became evident that some students could get 100% of the questions asked, but could only answer 1 or 2 questions in a given time, whereas others might cover 20 questions but get only 80% correct. Also, some could only answer simple questions; whereas others could answer more difficult questions.

Evaluation was based on three elements:

- Level of difficulty of the question (each question had a point value from 1 to 10),
- Percentage of correct answers,
- Amount of material that could be covered in a give time (total points of questions/time).

A formula was developed giving weights to each element to achieve a numerical grade. This oral format has survived until today.

Both the students that passed through my classrooms and the graduate students in my laboratory have gone on to successful careers. Many joined universities both in this country and overseas, some became deans and assistant deans, some owned their own companies, and at least 10 became vice presidents of some major food companies, including Wrigley and General Mills.

## COLLABORATORS

I would be very remiss not to acknowledge my many collaborators, without whom I could never have accomplished what has been done over the past 60-plus years. These are in addition to all the graduate students who "worked in the trenches" and made much of the progress possible. They include: T. V. Armstrong (OSU), V. B. Alvarez (OSU), M. Boland (NZ DRI), G. Chism (OSU), P. D. Courtney, (OSU), J.F. Delwiche (OSU), M. A. Drake (NCSU), W. Dunkley (UC Davis), M. Earl (NZ), I. A. Gould (OSU), C.L. Hankinson (OSU), E. Hargrove (USDA), T. Kristoffersen (OSU), M. E. Mangino (OSU), L. Muller (CSIRO), N. Olson (UW), W. Price (UW), L. E. Rodriguez-Saona (OSU), K. Shahani (OSU), E.G. Samuelson (Sweden), D. A. Seiberling, (OSU and Seiberling Assoc.) W. L. Slatter (OSU), H. Wang (OSU), J. Woijchik (USDA), G. Zadow, (CSIRO), and many others.

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The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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**Figure 3**

Massey University Harper Protein Symposium 2009. Rex Humphrey, Shellagh Hewitt, Lee Huffman, Jim Harper. Part of Whey Product Development Team NZDRI, 1981–86.

**Figure 4**

Tony Jin and Jim Harper with an electronic nose, 1999.

**Figure 5**

CIP revisited. Jim Harper and Nurdan Kocaoglu-Vurma with a fully automated CIP testing system. Designed by Dale Seiberling, 2005. Currently in use to improve removal of biofilms from food equipment surfaces.