MAKING MILKING MODERN: AGRICULTURE SCIENCE AND THE AMERICAN DAIRY, 1890-1940

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In the late nineteenth century most dairy farmers went about their work in much same manner as had their predecessors centuries earlier. However, by 1940 most farmers practiced recognizably modern dairying techniques. Use of mechanical milking machines was widespread and growing, farmers compounded rations by combining feeds that blended precise proportions of proteins, carbohydrates, fats, and vitamins, and breeders, eager to maximize the influence of productive bloodlines, evaluated their animals with the use of scientific scorecards and employed intense breeding plans that relied on various forms of inbreeding in order to fix the desirable aspects of prized cattle.

Yet the majority of these changes were instigated not by the dairy farmers who actually performed the tasks but by agricultural scientists working in the laboratories of the nation's agricultural colleges and experiment stations. Agricultural science emerged in Germany in the 1840's; Americans pursuing advanced degrees in Europe brought these ideas to the United States War and received an official imprimatur with the passage of the

Hatch Act in 1892, which dedicated federal funds to the establishment and maintenance of agricultural experiment stations.

The focus of this study is the work performed by these scientists in shaping the development of American dairy farms between 1890 and 1940. Researchers not only made scientific advances, such as the discovery of vitamins, that led to new methods of feeding and breeding dairy cattle but also invented and evaluated technological advances such as the Babcock Milk-fat test and mechanical milking machines that would revolutionize American dairying.

This work contributes to our understanding of the emergence of the modern dairy farm by demonstrating that it was agricultural scientists, more so than farmers, who established the outlines of the modern dairy. They did so not only by adopting common techniques and methodologies that fostered communication and cooperation between and among researchers but by employing a number of rhetorical devices that broke down the barriers between laboratory and farm. While farmers enjoyed the benefits of scientific advances, they did so at the cost of their autonomy as scientists increasingly dictated what constituted modern dairying.

Key words: dairy, cattle, milk, agricultural science, experiment stations

DEDICATION

I would like to dedicate this work to my parents, Bruce and Gloria Rueber, without whose encouragement, support, prayers, and love of Jersey cattle none of this would have happened.

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While academics are important, friendships are too, and I have made some life-long friends during the course of my studies. Hence, my buddies at Iowa State must be mentioned, especially Paul, Megan, and Knut – many thanks for putting up with a greenhorn. I wish you all the best. And while I was loath to leave good friends in Iowa, I have made plenty more in Mississippi: to Scott (my fellow academic-carpetbagger), Nathan, and Erinn go especial thanks. I must also acknowledge Jung, a fellow-traveler, whose fearless spirit has been an inspiration in its own peculiar way.

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CHAPTER 1

INTRODUCTION

On April 23, 2009, National Public Radio aired a segment about an eight-year old Hereford cow. The animal, named L1 Dominette 01449, lived on a ranch in eastern Montana, and had been the subject of intense scrutiny for some six years. The news piece related that an international team of scientists had, using DNA provided by Dominette, completely mapped the genome sequence of a bovine for the first time. Researchers interviewed for the piece enthused about the importance of their accomplishment; Harris Lewin, head of the Institute for Genomic Biology at the University of Illinois at Urbana-Champaign, claimed the findings revealed "the essence of bovinity." Lewin believed the completion of the bovine genetic sequence offered radically new insights into and, ideally, control over, cattle breeding and feeding. Used in conjunction this knowledge would allow stock-raisers not only to breed cattle with genetically superior productive capacities but also to better feed these animals: beef cattle would produce more usable meat, and dairy cattle more milk, all while more efficiently converting feed into beef, milk, cheese, and other commercially valuable commodities.[1]

The print media also carried news of the discovery. Popular newspapers such as The New York Times reported the completion of the bovine genome sequence. So too

[1] Jon Hamilton: "Cow Achieves Fame Through Her DNA," NPR, April 23, 2009, transcript viewed at http://www.npr.org/templates/story/story.php?storyId=103382511 on September 13, 2009.

did more specialized journals, including Science, which devoted several articles to the various implications of this breakthrough. Like the NPR segment, these pieces waxed enthusiastic about the possibility that this new knowledge might revolutionize both the dairy and the beef industries.

The authors of the various news articles interviewed a variety of experts. Biologists, geneticists, technicians, and a smorgasbord of scientists all weighed in on the importance of the completion of the genome project. However, conspicuously absent was the voice of the presumed primary benefactor of this newfound knowledge: the farmer. For whatever reason, none of the authors deemed it important to discuss the findings with a dairy farmer or beef rancher. Moreover, none of the sources commented on fact that agricultural innovation should come not from the field or the barn but, rather, the laboratory. Indeed, the impression given the listener or reader was that agricultural progress naturally flowed from researcher to farmer.

Yet prior to the mid-nineteenth century such a situation would have been impossible, for the simple fact that agricultural scientists – or at least those who labeled themselves as such – simply did not exist. Agricultural science that was recognizable as such only emerged in the United States in the second-half of the nineteenth century as American students pursuing graduate degrees in Europe – and especially Germany – brought back ideas gleaned primarily from German chemist and pioneering agricultural scientists Justus Liebig and his students, a process outlined by Margaret W. Rossiter in *The Emergence of Agricultural Science*. Despite their advanced training, these scientists had little success implementing their ideas upon their return to the United States. As Rossiter demonstrates, the first generation of American agricultural scientists found it difficult to obtain patronage and influence as only a handful of universities maintained laboratories and state and federal politicians balked at the notion of establishing permanent experiment stations.[2]

In *Agricultural Science and the Quest for Legitimacy* Alan I Marcus examines how agricultural scientists came together in various ways to lobby for official recognition, a process that culminated with the passage of the Hatch Act in 1887. The Act provided federal funding for the establishment and maintenance of agricultural experiment stations in the various states and territories. At the same time, the imprimatur of the federal government raised the status of agricultural scientists. Marcus notes that the passage of the Hatch Act "sounded the scientific farmer's death knell…agriculturalists were businessmen, not scientific professionals."[3]

Though the scientists Rossiter and Marcus study – primarily the first generation of American agricultural researchers – paid little attention to the problems of the dairy, by the 1880's a cadre of dairy researchers had emerged, primarily in states - such as Wisconsin, New York, and Pennsylvania – that had a long history of dairying and, as such, a vested interest in the continued success of dairy farms. As interest in dairying spread so too did the number of researchers working in the field increase, and by the turn of century experiment stations from Mississippi to Oregon had published bulletins reporting their own findings.

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[2] Margaret W. Rossiter: *The Emergence of Agricultural Science: Justus Liebig and the Americans, 1840-1880*, Yale University Press, New Haven, CT, 1975, see esp. pp. 172-176.

[3] Alan I Marcus: *Agricultural Science and the Quest for Legitimacy*, Iowa State University Press, Ames, IA, 1985, quote from p. 219.

In just five decades American agricultural scientists emerged from obscurity – if not non-existence – to positions of prominence. If, as noted by historians as diverse as John L. Shover and Claire Strom, not all farmers recognized the authority, appreciated the efforts, or followed the advice of scientific experts, these scientists had, nonetheless, largely managed to set the terms for the debates that followed.[4]

Between 1880 and 1920 dairy scientists acted as arbiters – and, in some cases as prosecution – in a number of disputes. This period witnessed the passage of local, state, and federal legislation dealing with bovine tuberculosis, oleomargarine, milk produced by cattle fed brewery swill, and the pasteurization of milk. As noted by E. Melanie DuPuis, in *Nature's Perfect Food*, scientific authority played an important, and sometimes leading, role in each of these debates. [5]

Nor was the influence of dairy scientists limited to the courts and legislative halls. By the 1890's both of the nation's leading dairy periodicals – *Hoard's Dairyman* and *Kimball's Dairy Farmer*, each with a circulation that numbered in the hundreds of thousands – regularly published news of the latest scientific developments and often summarized the contents of recently published experiment station bulletins. The publishers and editors of the journals clearly aligned themselves `with the scientists and used their editorials both to promote the benefits offered by "scientific" dairying and to chastise – and hopefully win over – farmers who refused to adopt the "rational" farming

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[4] For example, see John L. Shover: *First Majority, Last Minority: The Transforming of Rural Life in America*, Northern Illinois University Press, De Kalb, IL, 1976 and Clair Strom: *Making Catfish Bait Out of Government Boys: The Fight Against Cattle Ticks and the Transformation of the Yeoman South*, University of Georgia Press, Athens, GA, 2009.

[5] E. Melanie DuPuis: *Nature's Perfect Food: How Milk Became America's Drink*, New York University Press, New York, 2002.

advocated by the journals. Furthermore, the articles themselves adopted a scientific tone; for example, articles on feeding routinely noted the balance of proteins, carbohydrates, and fats in a given ration, language that would be largely meaningless one unless one understood the scientific terminology employed.

The rise of agricultural scientists and the emergence of a new, recognizably "modern" dairy farm went hand-in-hand. The decades after 1880 that witnessed the increasing importance of diary researchers also saw the appearance of a number of technological innovations that would transform the American dairy: the silo, the mechanical milking machine, cream separators, and the Babcock milk-fat test all appeared and found wide acceptance during this period. Moreover, researchers did not ignore the new discoveries but instead tested, evaluated, and, eventually, opined about the usefulness and worth of the new technologies.

Hence dairy scientists played a crucial role in the creation of the modern dairy. What had once been the exclusive province of farmers now became something of a shared landscape, as scientists, farmers, and, increasingly, government officials together established the framework of the modern dairy industry. The role of farmers and government agents has been rather well documented; that of dairy researchers less so. Because they increasingly dictated not only the terms of the debate but set the agenda for the future an appreciation of their efforts is necessary in order to understand these developments. Nor has the influence of scientists dwindled over the last century. On the contrary, as the NPR article suggested, they remain key participant in the dairy industry, and their findings continue to point out the future direction of the industry.

However, while the goals of dairy researchers have remained unchanged – namely, determining how to better breed and feed animals in order to produce the most milk at the lowest price – the methods they employ have undergone a number of transformations. Just as the news pieces that discussed the completion of the bovine genome sequence overlooked the role played by farmers, so too did they fail to mention that the history of the "modern" dairy has been characterized by change, and that the latest findings are only the latest in a line of discoveries that promised to – and to a considerable extent, often did – revolutionize the dairy industry.

In this work I examine two methodologies employed by dairy researchers, one employed by the first generation of dairy scientists from roughly 1890 through 1920, the second primarily by their younger colleagues from approximately 1915 through 1940. The first methodology was characterized by a belief that understanding the physical world consisted of breaking the object of study into discrete components. Having done so, scientists could first quantify the various constituents and then, by isolating one variable, determine how changes to one component affected the whole

This methodology possessed a number of attractive characteristics. It offered a clear plan of research: scientists interested in the properties of cattle rations, for example, proceeded by breaking various foodstuffs into their constituent elements and then measured the amount of proteins, carbohydrates, and fats contained in the ration. This method also promoted cooperation: so long as scientists agreed on what was worth measuring, and how to go about making these quantifications, researchers could easily share in the work.

Most importantly, this methodology proved readily adaptable; scientists could, and did, employ the same basic scientific framework to investigate a wide variety of subjects, from feeding and breeding to the evaluation of milking-machines, the benefits or drawbacks of providing warm or cold water to animals, the merits of various milk-fat tests, and virtually every other facet of dairy science. Indeed, the scientist's greatest difficulty lay not in deciding what to study, nor in how to go about their investigation, but in agreeing just what should be quantified, and scientists early on settled on a number of lines of research that represented the bulk of their efforts.

However, just because scientists adopted a common methodology should not imply that they necessarily agreed on how that methodology should be employed. For example, as I demonstrate in chapter two, most scientists interested in the nutrition of dairy animals believed that the key to understanding feeding lay in discovering how to formulate the ideal ration for diary animals. Though these researchers went about their work in similar ways and found that their results agreed with those of their colleagues, considerable debate ensued about how to interpret and implement these findings. All agreed that foodstuffs could be broken down into three important components, a number of different factions emerged, but each advocated a different ratio of protein to carbohydrates and fats; some scientists advocated a "wide" ration, others a "narrow" ration, while a third camp suggested that different ratios might prove most beneficial to different groups of animals – that animals which produced large amounts of milk might require a different ration than their less productive sisters, for example.

In addition, a handful of researchers suggested that their colleagues were going about their business backwards. In chapter three I examine the work of Henry Prentiss

Armsby, an early and influential researcher into animal nutrition. After publishing numerous books and articles that helped establish the orthodox research lines adopted by most of his colleagues, Armsby eventually decided that to understand nutrition one should not study the ration but the animal. Having done more than anyone to establish one line of research Armsby changed direction in mid-career, and spent the rest of his professional life trying to convince his fellow scientists to follow his (new) lead. Clearly then, the adoption of a common methodology did not produce a consensus among researchers, in part because the very versatility of that methodology allowed scientists to follow a number of sometimes contradictory lines of investigation.

Moreover, by 1915 a handful of scientists began to question the usefulness of this methodology. The discovery of vitamins and new findings in the field of genetics suggested that factors did not operate in isolation but rather affected the whole in strange and – by the current methodology – unpredictable ways. This new generation of researchers suggested that studying discrete properties in isolation represented a dead end. To overcome these difficulties they had to devise a new methodology.

The hallmark of their new scientific methodology consisted of the implementation of advanced mathematical techniques, and most importantly the adoption of the tools of probability and statistics. Using these techniques allowed scientists to make sense of their findings. Most importantly, these techniques allowed researchers to measure and interpret factors, such as the appearance of an animal or the amount of a vitamin required by laboratory mice, that had thwarted the use of more straightforward quantitative techniques. However, the new methods were not without their drawbacks. Foremost among these was the loss of certainty about any individual observation; these techniques allowed scientist to investigate group behavior but largely precluded them from making definitive statements about any specific case. Genetic researchers, for example, could no longer assert that the cross of bull "x" with cow "y" would produce calf "z" that possessed certain characteristics. At best, they could predict with some accuracy that if the animals produced some impossibly large number of offspring a definite and predictable percentage would be better, worse, or roughly the same as their parents.

My examination of dairy scientists thus has two aims. First, I demonstrate the vital role that scientists played in the creation of the modern dairy. Second, I suggest that "science" is not the monolithic pursuit of truth but is, instead, a cultural construct that arises out of the necessities of a given time.

I show that while most dairy scientists accepted a common methodology, albeit it one that, as I explain, changed over time, the ends they sought were usually dictated by – or what researchers perceived as – the needs of dairy farmers. The case of Stephen Babcock, who developed the first practical and successful milk-fat test widely used on dairy farms, illustrates this point. As I relate in the first chapter, Babcock attacked this problem only at the urging of W.A. Henry, the Dean of the University of Wisconsin's School of Agriculture, who was himself responding to the calls of Wisconsin dairy farmers for a test that would put farmers and creamery owners on a more equitable footing in an effort to curb the abuses of the creamery system. While Babcock had not previously investigated this topic, the way he went about his research – contrary to the claims of some historians who portray Babcock as an eccentric or a savant – mirrored that of his contemporaries. Babcock was most notable not for his approach to science, but because of his success in applying a well-accepted methodology to a variety of questions.

In the second chapter I consider the first decades of dairy nutrition, a case that reveals some of the advantages and disadvantages of the approach employed by most dairy scientists. In studying nutrition almost all researchers followed a common methodology. Doing so allowed scientists at poorly-funded and equipped stations to make important contributions to their field. The adoption of common practices and language also allowed researchers to easily communicate their findings to their peers. In many ways this process mirrored the model expounded by advocates of government funding of agricultural schools and experiment stations. However, while scientists readily agreed about *how* they should go about their work, they had a much more difficult time reaching a consensus about *what* their findings actually meant in practice. As one group of researchers busily measured and catalogued the digestible nutrients in a wide variety of foodstuffs, another engaged in a debate about how to apply these findings. For example, while scientists quickly concurred about how to find the amount of protein contained in a particular variety of corn, they had a much more difficult time reaching agreement on how protein a dairy cow actually required. I thus demonstrate that the employment of a common methodology represented something of a two-edged sword: while the widespread acceptance of practices fostered cooperation and communication, it at the same time produced a cacophony of claims about the meaning of the findings. In short, the adoption of a common methodology raised almost as many questions as it answered.

Though most nutrition researchers followed accepted practices, a handful questioned the emphasis these scientists placed on rations. The dissenters maintained that *animals*, rather than animal *foods*, should form the basis of nutrition research. Henry Prentiss Armsby, who probably did more than anyone else to popularize the status quo in his earlier – and well-received – works, eventually concluded that scientists needed to better understand the physiological foundations of nutrition. To this end he raised funds and oversaw the construction and employment of a large respiration-calorimeter that he employed to study the processes by which cattle utilized their feed. His story, which forms the basis of chapter three, amounts of something of a cautionary tale. Like his contemporaries, Armsby practiced science by first isolating and then quantifying the various properties and processes he sought to understand; his was a shift in ends rather than means. However, Armsby, by all accounts a careful experimentalist, grew increasingly frustrated as his research with the calorimeter produced numerous anomalies that thwarted his expectations. A later generation of scientists would attribute these discrepancies to vitamins, but Armsby, though aware of the presence of these newly discovered substances, did not understand their importance and, instead, believed the problems stemmed from some oversight in the construction or operation of the calorimeter. Despite decades of careful work, scientists – including one of his successors – eventually declared Armsby's avenues of investigation a dead end. His example serves to illustrate the shortcomings of this scientific approach.

In spite of these faults, the research methodology employed by Armsby and his colleagues proved immensely popular, in part because the practice could be readily adapted to variety of purposes. In chapter four I examine how dairy scientists employed this methodology in their examination of a new technology: the mechanical milking machine. Researchers approached their evaluation of milking machines from a number of directions. Some compared the speed of machine milking with hand milking, others determined the cleanliness of the machines, and a third group attempted to ascertain the short- and long-term effects of machine milking on the production and health of animals. In performing these various tests scientists employed a common methodology, merely modifying the specifics to fit desired ends – and in the process demonstrating the adaptability of their methodology.

In chapter five I look at how scientists applied their methods to the question of selecting and breeding dairy animals. This process took two main forms. One group of researchers attempted to establish a scientific basis for judging cattle. They approached their task in a manner similar to that employed by animal nutritionists: they broke the animal's appearance and anatomy into a variety of discrete categories and then assigned a numerical value to each aspect of the animal: depending on the scale used, the face might be worth a maximum of five points, while the udder could be worth twenty. The aggregate of these scores indicated the relative merit of the animal in question. At the same time, a second group of dairy scientists attempted to apply Mendelian genetics – at that point only recently rediscovered – to dairy animals. These researchers, drawing heavily on the work of noted eugenicist Charles B. Davenport, tried to establish the scientific laws that governed breeding. Of the two parties the first – those attempting to establish a useful "score-card" for dairy cattle – proved more successful in popularizing their findings, though, like their colleagues studying animal nutrition, they too found agreement on methods easier to reach than consensus on how to apply the results they garnered. The second group, while producing mountains of mathematical data they hoped would place breeding on a scientific foundation, met with less success; it remained for a succeeding generation, inspired by the work of Sewall Wright, and, especially in the case of dairy cattle, Jay L. Lush, to more fully comprehend the implications of Mendelian genetics into practically useful animal breeding plans.

Together the first five chapters of my dissertation survey the main currents of dairy research in the United States from 1890 until roughly 1920. During this period scientists employed a common methodology regardless of the focus of their research. Despite the shortcomings noted above, this approach held a number of advantages. Perhaps most importantly, the methodology dictated how scientists went about their work. Scientists sometimes disagreed about what they should measure, but none doubted that isolation and quantification formed the keys to scientific progress. Adopting common procedures fostered communication between scientists; it also allowed researchers to present a (mostly) unified message to farmers.

Unfortunately, by 1920 developments in the fields of nutrition and genetics posed difficulties that resisted satisfactory analysis by traditional means. In response a second, generally younger, generation of scientists developed new tools and approaches to deal with these issues. Though nutritionists and geneticists faced different quandaries, they both found that the adoption of new mathematical methods offered them a way to make sense of their findings. In specific, researchers began to employ the techniques and procedures of statistics and probability. The use of these methods proved fruitful, and scientists developed a range of techniques that allowed them to make sense of findings that they had been unable to successfully tackle with more traditional approaches.

The discovery of various substances that played in an important role in maintaining the physical growth and health of animals – what eventually became known as vitamins – by a number of researchers, including Polish chemist Casimir Funk, the team of Osborn and Mendel of Yale, and of McCollum and Davis at the University of Wisconsin, in the second decade of the twentieth century forced researchers to adopt new methods primarily because these new substances resisted analysis by traditional techniques. I explore the implications of this process in chapter six. Because chemists could not determine the chemical composition of these elements – they did not discover the makeup of most vitamins until after World War II – they could only ascertain the presence of these substances indirectly by their effects on animals. This forced researchers to devise new methods of analysis. Scientists, reluctant to experiment on valuable dairy cattle, instead tested the effects of different rations on mice, and eventually led to the creation of a new measuring system: the "rat unit." By using statistical methods scientists extrapolated dietary guidelines that they hoped would ensure the health of cattle, a process that necessitated the revision of traditional feeding standards.

In the seventh chapter I look at how the discovery of vitamins caused some scientists to shift their focus to the milk produced by cattle. Until this point researchers paid relatively little attention to milk. Though it was well known that foods such as onions or silage could impart an undesirable taste to milk, most scientists believed that diet and other factors exerted little influence on the chemical properties of the liquid. The discovery of vitamins – and the realization that the production of milk high in vitamins might prove profitable for dairy farmers – caused scientists to reconsider the relation between the rations cattle consumed and the milk they produced. Again, because chemists did not understand the chemical composition of vitamins, they were forced to rely on indirect means of measuring the nutritional properties of milk.

Finally, in chapter eight I examine the adoption of statistical methods by researchers who studied animal genetics. Unlike their colleagues of the previous generation, who believed that they might eventually be able to mathematically predict the properties of the offspring of two animals, these younger researchers, led by Sewall Wright, realized that animals with a large number of genes – such as cattle – could produce literally billions of combinations. However, applying the laws of statistics allowed them to make quite accurate predictions about the characteristics of a large group of the offspring resulting from the mating of two animals. A handful of dairy scientists, led by Jay L. Lush, attempted to incorporate these findings into new selection and breeding plans that, while they could not offer absolute assurance, would present the best chance of permanently improving the overall characteristics of the group – be it herd or breed – under consideration. Again, the attention changed from the study of the individual to the group.

Despite the changes in both emphasis and methodology, the aim of dairy science remained the same: to determine how to breed and feed cattle that would produce the most milk at a lower cost. Viewed from this perspective the completion of the bovine genome sequence appears not as a unique event but as another in a series of changes that scientists hoped would create a better dairy.

An examination of the dairy farm's transformation is important precisely because it helps us to understand how this situation came about. The fact that we no longer evince surprise when discoveries about the future of the farm are proclaimed not by farmers but by researchers demonstrates not only how completely scientists have ensconced themselves in the driver's seat but, also, how we have come to accept their largely self-appointed position. In this work I demonstrate some of the ways a cadre of agricultural scientists – a group that hardly existed in this country prior to 1870 – came to appropriate a position of leadership in and of the dairy industry by the early twentieth century.

They did so in part by lobbying government officials for the creation of agricultural experiment stations that granted them, in addition to fiscal support, an official imprimatur. Under the auspices of the newly created experiment stations, agricultural science was, de facto, government science. Moreover, the mandates of the Hatch Act financed the bulletins – designed to broadcast the findings of these researchers to the farmers they ostensibly served – that allowed scientists to communicate with each other.

This was a crucial development, because scientists could only gain authority by toeing a common line. As I demonstrate, agricultural scientists, no matter where they worked or where they had received their training, employed common methods and practices. Doing so allowed them to easily differentiate their own methods from those of farmers who engaged in other, "non-scientific," practices.

That they did so is hardly remarkable; after all, as numerous historians have demonstrated, all sorts of groups – from doctors and lawyers to engineers and historians – founded (or reconceived) professional societies in the decades following the Civil War. By holding regular meetings, establishing professional journals, and requiring members to regularly disseminate their work these societies attempted to create – and then enforce

– conformity within their membership. In this regard agricultural scientists behaved like members of any other professional group.

However, historians have paid far less attention to the practices these organizations actually advocated, and how disparate members within the societies applied these conventions to their own work. In this work I demonstrate how individuals in one such group applied a common methodology to widely disparate investigations. And, as I demonstrate in the final three chapters, the methodology employed by researchers changed as accepted techniques failed to adequately explain newly discovered phenomena. Despite this change, however, "Science" as a concept rather than as a group of practices and beliefs held by scientists maintained the vaunted position it had assumed over the prior decades. The fact challenges orthodox understandings of the well-documented "rise of expertise" that took place during the Progressive era by demonstrating that Americans, by and large, accepted the increasing authority of Science writ large rather than a discrete and readily identifiable bundle of beliefs. Put another way, Americans readily acknowledge the authority of "science" – and of scientists – even as (or despite the fact) that what "science" was changed over time.

This work thus sheds light on one of the much-recognized yet little understood processes by which new players entered the playing field of society – in this case, how dairy scientists, employing common techniques and practices, quickly established themselves as leaders of the dairy industry. As such, it lends complexity to our understanding of the emergence of professional groups in the progressive era. Certainly the story of each cadre varies in its details; nonetheless, an appreciation of the methods used by dairy scientists furthers our historical comprehension of this time.

CHAPTER 2

SAVING STEPHEN MOULTON BABCOCK: ECCENTRIC CHARACTER, ORTHODOX MEANS

Stephen Moulton Babcock, a long-time researcher at the University of Wisconsin's agricultural experiment station, passed away on July 2nd, 1931, at the age of eighty-seven. His passing received an amount of attention rarely afforded agricultural chemists. News of his death appeared on the front-page of newspapers across Wisconsin and in several other Midwest states. His death amounted to more than a regional matter: the vaunted New York Times published a lengthy obituary of Babcock, complete with a photograph of the late professor.[6]

Between the various newspaper accounts it was possible to construct a rough biography of Babcock's life and career. Born in upstate New York in 1843, Babcock graduated from Tufts College in Massachusetts and started graduate work at the Rensselaer Polytechnic Institute before his father's death forced him to take over the family farm. While living at home he worked part-time in the chemical laboratory of nearby Cornell University. In 1877 Babcock, like many of his American contemporaries,

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[6] The Evening Independent of Massillon, OH. The Globe-Gazette of Mason City, IA. The Evening Telegraph of Alton, WI. The Capital Times of Madison, WI. The Daily News of Huntington, PA. The Wisconsin State Journal of Madison, WI. The Evening Herald of Decatur, IL. The Daily Freeman of Kingston, NY. The Tribune of Bismarck, ND. The Evening Tribune of Albert Lea, MN. The Daily Tribune of Wisconsin Rapids, WI. The New York Times, July 3, 1931, p. 31.

traveled to Germany to continue his education, and received his Ph.D. from the University of Gottingen in 1879. After serving briefly as Instructor of Chemistry at Cornell he accepted a chemistry post at the New York State Agricultural Experiment Station in Geneva, NY, where he worked until 1888. That year Babcock became chief chemist at the Wisconsin Experiment Station in Madison, WI, a position he held until his retirement.

Babcock's obituaries universally praised his generosity, and many mentioned his somewhat eccentric character. They noted that Babcock had amassed a modest fortune – valued at approximately $130,000, no small sum during the Great Depression – and, after establishing limited trusts for a handful of less fortunate relations, had bequeathed the remainder of the estate to the University of Wisconsin. Newspapers reported that Babcock remained an enthusiastic supporter of the University's football and baseball teams until his death, that he rarely wore an overcoat even in the coldest Wisconsin winters, and that, while he enjoyed motoring in his Franklin automobile, he considered the telephone a nuisance and refused to install one in his home.[7]

The newspaper obituaries commented on Babcock's lengthy list of scientific achievements. Babcock had developed a viscosimeter that allowed scientists to detect the presence of adulterants in fluids, as well as a gravimetric method of analyzing milk that was adopted by the American Association of Official Agricultural Chemists. Babcock also devised a method for determining the quantity of fat globules in milk, investigated the cold-curing of cheese, performed pioneering investigations in animal nutrition,

[7] Ibid.

studied the chemical processes involved in the production of silage, and made a number of other notable contributions to the field of agricultural chemistry.

But above all, Babcock's obituaries noted that Babcock had been the inventor of the test that bore his name: the Babcock Milk-Fat test. Despite his long list of impressive achievements, it was the invention of the milk-fat test that ensured Babcock's fame and justified the attention given his death. The papers claimed that Babcock's test transformed the American dairying industry, and that its invention marked the advent of "modern" dairying. Newspapers also praised Babcock's largesse: while he could have patented the device and made millions of dollars in licensing fees, Babcock instead gave the device free to the world, further lowering the price of an already inexpensive test and truly making it available to the masses of dairymen of the United States and the world.

Babcock's test did, in fact, revolutionize the dairy industry. At the market level it evened the playing field between farmers and the creameries that bought their produce. In the 1890's cream, which could be turned into valuable – and before the widespread use of mechanical refrigeration, easily transportable – butter, rather than milk, was the nation's most economically important dairy product. Because of this, creameries paid for milk on the basis of the amount of cream contained in each milk barrel delivered by farmers. Creameries typically employed a graduated dipping spoon, often referred to as a "shotgun-spoon" (after its resemblance to a shotgun shell), that they would dip into the milk barrel. A clear glass window on the side of the spoon allowed them to determine the amount of cream – which, being lighter than milk, rose to the top of the milk barrel - contained in the milk, and the creameries paid the farmers accordingly.[8]

Unfortunately, such a crude method meant that the results of such tests were rough estimates at best. As a result, farmers rarely received full value for the cream they supplied. As might be suspected, unscrupulous farmers and creameries both engaged in dishonest practices to maximize their profits. Some farmers added adulterants to their milk, either by diluting it with water, or adding other ingredients that appeared to increase the amount of fat contained in the milk. For their part, creameries would agitate the milk, increase its temperature, or use doctored measuring devices to minimize or obfuscate the fat content of the milk – and hence, the amount they paid the dairy farmer. The absence of a cheap, easily employed, and reasonably accurate test for the fat content of milk did little to discourage these abuses.[9]

Babcock's test fit the bill. His test - which employed a minimum of equipment, utilized readily available and inexpensive sulphuric acid as its only chemical component, and required very little skill - made it possible for both farmers and creameries to quickly and cheaply determine the amount of fat contained in milk. Farmers could now calculate the amount of fat that they sold, and creameries could easily detect adulterated milk. In the event of a dispute, the sample could be quickly retested. Put simply, the Babcock test marked a turning point in the history of commercial dairying. Ralph Selitzer, author of the most comprehensive history of dairying in the United States, stated the importance in

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[8] See Eric E. Lampard, *The Rise of the Dairy Industry in Wisconsin* (Madison: State Historical Society of Wisconsin, 1963), 197-202.

[9] See chapter III (pp. 57-98) of Ralph Selitzer's *The Dairy Industry in America* for an account of the advent and rise of the "Creamery" movement in the United States.

no uncertain terms: "Upon this test…the 20th century structure of United States dairy economy was built."[10]

The use of the Babcock test established an equitable footing between dairy farmer and creamery, and for this alone would have merited the praise Babcock received both in life and in death. As importantly, the practicality of the test – its low cost and ease of use – meant that farmers could also readily employ it on the farm. For example, by testing individual cows dairymen could determine which animals produced the most profit and, as importantly, which actually produced a loss. Dairymen could almost immediately increase the profitability of their herds by culling unproductive animals. Eliminating unprofitable animals also had long-term benefits. Removing low-producing animals from the herd improved the breeding stock, increasing the probability that future generations would prove even more productive. Finally, the Babcock test allowed dairymen to tinker with the effects of various foodstuffs. By experimenting with different rations farmers could more easily determine which fodder produced the greatest amount of valuable butterfat at the lowest cost.[11]

The net effect of all the changes wrought by the Babcock test amounted to nothing less than a revolution in American dairying; on his death the Constitution-Tribune declared the Babcock test "the basis of building up dairy herds and grading milk throughout the world," while the Wisconsin Rapids Daily Tribune referred to him as "the

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[10] Ralph Selitzer, *The Dairy Industry in America* (New York: Magazines for Industry, 1976), 84.

[11] John T. Schlebecker, *A History of American Dairying* (Chicago: Rand McNally and Co., 1967), 31.

dairyman's greatest benefactor." The fundamental importance of his test to American dairying ensured Babcock's fame even during his lifetime and explains why his death engendered so much press coverage.[12]

Newspapers lauded Babcock, and historians have been hardly less kind. Babcock's remains one of the few scientific names familiar to most serious students of American agricultural history. In part because of his groundbreaking work, and, perhaps, because of the University of Wisconsin's long-established history of science program, Babcock has figured prominently in the handful of dairy histories published since his death, and he numbers one of the few American agricultural scientists to have merited the publication of a monograph in their honor.

In 1943 the Wisconsin Alumni Research Foundation published *Stephen Moulton Babcock: Man of Science*. The volume's subtitle, "A Memorial to Him in Observance of the Centenary of His Birth," spells out the aim of the publication. The book included a number of remembrances written by scientists who worked with Babcock, a chronology of his career, and a list of the honors bestowed upon him during his life. *Man of Science* paints an almost saintly portrait of Babcock's life, and – unlike other biographies which focus exclusively on the development of the milk-fat test - does pay some mention to the breadth and scope of his research interests. Like most accounts of Babcock, the book emphasizes the importance of the discovery of his eponymous milk-fat test. Unfortunately, despite its title, the book attempts to portray Babcock the exemplar of

[12] Constitution-Tribune: "Man Who Invented Butterfat Test Is Found Dead Today," Thursday, July 2, 1931, p. . Wisconsin Rapids Daily Tribune: "Discoverer Of Butterfat Test Victim Of Heat," July 2, 1931, p. 1.

"Yankee ingenuity" rather than as a successful, mainstream scientist, describing him as a loner in the laboratory who rarely collaborated with other researchers, refused to work with graduate students, and eschewed the services of the University's technicians, instead preferring to "whittle out a piece of apparatus with his jackknife than have the University mechanician build the apparatus in the shops." Though the slim volume remains the only publication dedicated solely to Babcock, and does contain some important information about Babcock's work outside the development of the milk-fat test, its hagiographic portrayal of Babcock occludes the important work that he performed. Furthermore, the volume's tendency to emphasize Babcock's intuition – one section is entitled "Born Doubter" – de-privileges his stature as a well-trained and well-respected scientist who, far from acting as some sort of rogue genius, operated well within the accepted scientific parameters of his day.[13]

Aaron J. Ihde's short essay "Stephen Moulton Babcock – Benevolent Skeptic" also represents Babcock as an intuitive savant; moreover, it describes him as a sort of forbearer to the counterculture of the 1960's whose "good-natured resistance toward current paradigms…were to have profound implications…" While the latter observation might be attributed to the spirit of the time – Ihde first presented his work in 1969 – the former, unfortunately, parrots the position outlined in *Man of Science*. Though Ihde chronicles a handful of Babcock's breakthroughs, he seems more interested in demonstrating that Babcock achieved professional success despite his unorthodox behavior. Ultimately declaring that "Babcock can hardly be looked upon as a scientist of

[13] The Wisconsin Alumni Research Foundation: *Stephen Moulton Babcock: Man of Science*, Madison, WI, 1943.

the first rate," Ihde's essay does little to further our understanding of Babcock's work nor does it adequately explain the importance of his research.[14]

Babcock's reputation as a serious and respected scientist has been better served by two somewhat more recent histories of the American dairy industry. Both John T. Schlebecker's slim but valuable *A History of American Dairying* and Ralph Selitzer's much more comprehensive *The Dairy Industry in America* describe the impact of the Babcock test on American dairying. According to Schlebecker the Babcock test minimized the abuses of the creamery system, afforded farmers the opportunity to determine the profitability of individual animals, and gave farmers the ability to experiment with feeds. As a result, "the development of modern dairy farming stems primarily from the invention and use of the Babcock butterfat test.[15] While Schlebecker emphasized the wide range of applications of the Babcock test, Ralph Selitzer focused on the development of the test and its role in reforming the creamery system of dairying. He too made explicit the importance of the Babcock test: "Upon this test…the 20th century structure of United States dairy economy was built."[16]

Eric E. Lampard's *The Rise of the Dairy Industry in Wisconsin* presents the fairest and most historically useful overview of Babcock's work on butterfat testing. Lampard (perhaps understandably) acknowledges the role played by Babcock's colleagues at the

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[14] Aaron J. Ihde: "Stephen Moulton Babcock – Benevolent Skeptic," in Duane H.D. Roller, ed.: *Perspectives in the History of Science and Technology*, University of Oklahoma Press, Norman, 1971, quotes from pp. 281-282. In addition, Ihde shows little understanding for the need of a butterfat test:

[15] Schlebecker, 32.

[16] Selitzer, 84.

University of Wisconsin, though he, too, fails to mention the number of other American scientists working concurrently with Babcock to discover a practical means of testing the fat content of milk. Lampard neatly demonstrates how the development of such a test became something of a priority for agricultural scientists associated with the University of Wisconsin. In addition, Lampard emphasizes the economic importance of the test for Wisconsin dairy farmers and notes how the adoption of the test helped curb the abuses of the creamery system. Lampard pays rather less attention to the benefits the test offered on the farm, and how by using the test farmers could determine the productivity of individual animals as well as experiment with different rations. Despite this minor quibble, Lampard does a fine job of understanding and emphasizing the importance of Babcock's work.[17]

Unfortunately, these books – including, in some important ways, Lampard's - tend to obscure the historical significance of Babcock's innovation. They do so in two important ways. First, they suggest that Babcock was some sort of visionary genius whose invention appeared out of the blue and revolutionized the dairy industry. Babcock was, in truth, only one of several contemporary scientists who hoped to develop a simple, inexpensive means to test milk for butter fat. In the years prior to Babcock's announcement of his invention a handful of agricultural scientists, including several working for various state experiment stations, had devised fat tests and published their findings in experiment station bulletins. In fact, Babcock was not even the first scientist working at the Wisconsin experiment station to devise such a procedure: one of his

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[17] Lampard, Ibid.

cohorts at the station devised a test two years before Babcock, and the station bulletin reports that Babcock himself had helped to test this device. That others sought the same goal as Babcock dissolves the fiction that Babcock somehow stumbled not only on a successful test, but did so in an unprecedented or unlooked-for manner.[18]

Second, a survey of Babcock's life's work shows that throughout his career Babcock routinely devised tests designed to quickly, cheaply, and accurately measure some aspect, ingredient, or quality of milk. Therefore, that he showed interest in devising a milk-fat test becomes a logical goal for Babcock's research rather than an outstanding anomaly. Ultimately, the success and importance of Babcock's milk-fat test has overshadowed the efforts not only of Babcock but also those of his contemporaries.

Moreover, the emphasis on the success of Babcock's test, and the attention that historians have paid to this one innovation, obscures an important historical issue. Specifically, the emphasis that both Babcock and his contemporaries placed on developing simple, inexpensive, and accurate tests allows a window into their world. It shows, first of all, that these scientists believed that milk – like all compounds, for that matter – was comprised of discrete substances. Just as contemporary nutritional scientists believed that all foodstuffs could be broken down into fats, nitrogenous and non-nitrogenous matters, and ash, so too did dairy scientists hold that milk consisted of water, sugar, fat, and a small quantity of other, at that time only partially understood, substances. Many scientists believed that the identification and measurement of these substances comprised their primary scientific task.

[18] See F.G. Short, "A New Method for Determining Fat in Milk, *Wisconsin Agricultural Experiment Station Bulletin No. 16* (1889).

Furthermore, their understanding of the physical world shaped their research methods and goals. Having accepted that the world was ultimately composed of discrete, identifiable, and measurable elements – albeit often assembled in as yet unknown ways - science became a matter of devising methods to first isolate and then to quantify the various constituent components of the substance under investigation.[19] By doing so these scientists hoped to determine the laws that dictated the behavior of such substances, and they believed that doing so would allow them to calculate the most efficient means by which to produce desired ends. For example, dairy scientists hoped that by finding the rules that governed rations, milk, and the physical processes by which cattle transformed the former into the latter they could establish American dairy farming on a more profitable basis.

Finally, the fact that these scientists placed such a high premium on devising tests that could be employed by farmers in the milk room of the dairy barn, rather than by scientists in a laboratory, showed that they believed in the value of applied science. Nearly all of the milk-fat tests devised by agricultural chemists were designed for use in the real world, not the ideal and artificial world of the laboratory. The various research bulletins make it clear that they possessed accurate laboratory methods for detecting and measuring the various substances for which they devised field tests. More bluntly, they explicitly stated that they hoped to find simple methods that would allow farmers and other dairy workers to perform these tests in the real world.

[19] Alan I Marcus has written about the same process. See his *Agricultural Science and the Quest for Legitimacy* (Iowa State Press, Ames, 1985), pp. 63-64.

That they did so challenges the notion that a gulf existed between scientists and farmers. The idea that these groups regarded each other with mistrust if not outright hostility – that farmers regarded scientists as pointy-headed intellectuals whose work bore little practical use outside the laboratory, while scientists viewed farmers as backward hicks – has had long currency in agricultural history. Perhaps most clearly expounded by John L. Shover in his seminal *First Majority – Last Minority*, historians have begun to reconsider the idea in recent years: Alan I Marcus and Kathy J. Cooke have both shown that, at the very least, some scientists actively sought to provide farmers with practically useful results and modified their research aims towards these ends.[20]

In one sense a study of Babcock serves to confirm their findings. Like the scientists studied by Marcus and Cooke, Babcock actively sought to aid the American farmer. However, much more so than most of his contemporaries, Babcock's work demonstrated his commitment to providing farmers not only with the results of tests, but with actual methods that farmers could perform themselves. Furthermore, an examination of the perfection of a practical milk-fat test reveals that Babcock was not alone; that a number of Babcock's colleagues also devised tests intended for use on the farm suggests their commitment to the idea that farmers should not only accept the fruits of scientists' work, but that they could – and, more importantly, should – act as scientists themselves.

[20] For example, see Alan I Marcus, *Agricultural Science and the Quest for Legitimacy* (Ames, IA: Iowa State Press, 1985); Kathy J. Cooke: "From Science to Practice, or Practice to Science," *ISIS* 88, no. 1 (1997): 62-86; John L. Shover, *First Majority – Last Minority* (De Kalb, IL: Northern Illinois University Press, 1976).

Hence an examination of Babcock, the development of the milk-fat test, and a survey of the other tests he devised serves a number of historically useful purposes. First, it dispels the notion that Babcock was a either a "one-trick pony" or an eccentric loner not only by showing his lifelong commitment to developing practical tests designed to be employed by farmers but also demonstrating that Babcock was only one of a number of scientists actively working – at least in the case of the milk-fat tests – along similar lines. Second, that Babcock developed a test for farmers and – crucially – that farmers adopted the test in large numbers suggests not only that the gulf between scientists and farmers proposed by some historians was not necessarily intractable but that, at least in some cases, both sides made real efforts to understand each other. Finally, that Babcock and his colleagues, by suggesting that farmers undertake scientific testing for themselves, blurred the line between science and practice by proposing that science – or, at the very least, scientific testing – could be performed not only by scientists in the laboratory but by farmers in the milk-barn.

Moreover, in performing these tests dairy farmers and creamery workers adopted the scientific methodology employed by Babcock and his contemporaries in the laboratory. Just as researchers went about their work by identifying, isolating, and quantifying discrete properties, dairy workers using the Babcock test followed the same procedure. In short, Babcock viewed this methodology as equally applicable to both the laboratory and the "real" world. So too did farmers: the widespread adoption of Babcock's test suggests that dairymen appreciated the benefits it offered and demonstrates their willingness to employ scientific methods - or at least those that proved economically beneficial.

Therefore an understanding of Babcock's work helps us to understand and appreciate how Babcock and his contemporaries went about their work. A survey of his career demonstrates that he did not alter his methodology in devising his famous milk-fat test. Instead, he employed the same techniques he employed throughout his career. In devising his test he did not change his science, but rather substituted technologies that minimized the technical skill required to perform the test and eliminated the need for expensive scientific instruments. Placed within this context, Babcock's career represents not the anomaly suggested by some writers but a model that exemplifies the spirit of the time.

In 1883 Babcock, then a chemist at the New York State experiment station, introduced a new milk-fat test in the pages of the Station's Annual Report. The procedure, usually referred to as "Babcock's Method of Gravimetric Analysis," quickly became the standard laboratory technique for testing the amount of fat contained in a sample of milk or cream. It eventually became the yardstick by which other fat tests – including Babcock's later, more famous, test – were measured. Despite its accuracy, it was, in the end, a laboratory technique: to perform the test necessitated a fair amount of experience with laboratory techniques, specialized – and expensive – scientific instruments, and access to a variety of chemicals.

Though very successful in the laboratory, the requirements of Babcock's gravimetric test rendered it impractical for employment in the barn or the creameries. To this end a number of chemists endeavored to devise a more practical technique that would allow farmers and creamery operators to easily and inexpensively perform their own tests. Their aim was not to replace Babcock's gravimetric method in the laboratory, nor did they expect the new tests to measure fat as accurately. Instead, they hoped to perfect a method that would allow farmers and creameries to produce usefully accurate results with a minimum of effort or expense.

Most of the bulletins and articles that introduced these tests emphasized the benefits that farmers and creameries would enjoy. They stressed that under the creamery system prevalent in most of the country's dairying regions farmers received payment based on the amount of fat contained in milk rather than the amount of milk they delivered to the creamery. Animals that produced milk low in fat represented a loss no matter how much milk they might produce. Hence, some authors stressed the importance of the test as a means of identifying and culling unprofitable cattle. Charles L. Parson of the New Hampshire experiment station made this clear: "one may ask, on what basis shall one cow be kept and another rejected…? The question has been many times answered. Quantity was formerly the basis, but now the amount of butter-fat which the milk contains must have equal consideration."[21] William Frear and George L. Holter of the Pennsylvania state station echoed these concerns, linking the need for milk-fat testing on the farm with the flourishing creamery movement: "With the rise of the creamery system and its close competition, has come the demand for stringent economy in management…In view of the more urgent demand of the butter- and cheese-maker for milk of a high percentage of butter fat, and in consequence of the resultant discrimination in price, the farmer can no longer disregard the quality of his herd product and look

[21] Charles L. Parsons: "A New Volumetric Method for the Estimation of Fat in Milk, Skimmed Milk, Butter-Milk, and Cream," *The Journal of Analytic Chemistry*, Vol. III, 1888, p. 273.

solely to its quantity."[22] F. G. Short, Babcock's colleague at the University of Wisconsin, also stressed the need for a simple, easily employed test: "there are loud calls for it from dairymen and breeders of dairy stock." "The practical dairyman," Short continued, "desires to keep only such animals as will yield a profit," while "The breeder of dairy stock is no better off…In order to weed out the herd and breed…intelligently something like the exact knowledge of the butter production of each individual must be known."[23]

In addition to providing farmers with a method of identifying unproductive animals, scientists believed that milk-fat testing could curb the abuses – to, in Babcock's words, "avoid the evils" - of the creamery system.[24] Before the development of a simple fat test creameries rarely if ever tested the quality of milk provided by individual dairymen. What tests were performed usually took place only after the milk had been pooled from all contributing farmers and was used not to reimburse them but to determine the quality of butter or cheese produced by the creamery. Many people, farmers and scientists alike, viewed this system as inequitable to those dairymen who provided milk rich in fat. Scientists at the University of Illinois experiment station recognized this fact: "the pound of milk brought to a creamery by its patrons is not the

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[22] Wm. Frear and Geo. L. Holter: "Simple Methods of Determining Milk Fat," *The Pennsylvania State College Agricultural Experiment Station Bulletin No. 12*, July 1890, p.2.

[23] F.G. Short: "A New Method for Determining Fat in Milk," *University of Wisconsin Agricultural Experiment Station Bulletin No. 16*, Madison, July 1888, p. 3.

[24] S.M. Babcock: "A New Method for the Estimation of Fat in Milk, Especially Adapted to Creameries and Cheese Factories," *University of Wisconsin Agricultural Experiment Station Bulletin No. 24*, Madison, July 1890, p. 3.

most accurate basis upon which to pay for the milk, since the butter fat, which alone is of value to the creamery, is not always proportionate to the quantity of milk."[25]

Some researchers believed that creameries would also benefit from the development of a practical milk-fat test. Making the case for the adoption of his method, Charles Parsons claimed, "Every butter-maker, in order to compete with others, must soon know the amount of fat in the original milk, the amount he has obtained in his cream, and the amount lost in the skimmed milk and buttermilk."[26] G.H. Failyer and J.T. Willard of the Kansas Experiment station reiterated this belief: "For the use of dairymen, creameries, etc., it has seemed very desirable to have some simple method of testing the relative quality of milk."[27] So too did scientists at the Vermont Agricultural Experiment Station: "For some years there has been an urgent call for some method by which creameries could pay their patrons for the actual butter contained in the milk furnished instead of by the pound of milk…"[28]

Babcock's intentions for his new test mirrored those of his colleagues. Like them, he hoped his method would answer the needs of both farmers and creameries. He wrote that, despite the expense and difficulty of existing methods, "a few of them are being used to a considerable extent by careful breeders of dairy tock to determine the quality of

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[25] "Investigations of 'Milk Tests,'" *University of Illinois Agricultural Experiment Station Bulletin No. 10*, August 1880, p. 329.

[26] Parsons, 273-274.

[27] G.H. Failyer and J.T. Willard, "A New Method of Milk Analysis for the Use of Dairymen, and a Comparison of Its Results with Those Obtained by the Churn," *The Journal of Analytic Chemistry* 3 (1889): 294.

[28] "Testing Milk at Creameries," *Vermont State Agricultural Experiment Station Bulletin No. 16*, 2.

their cows, and by creameries for adjusting the price of milk between their patrons."[29] Far from being the lone voice crying in the milk-barn for the development of a practically useful milk-fat test, Babcock instead joined a chorus of colleagues who actively sought the invention of such a procedure.

Furthermore, Babcock recognized – and hoped to surmount – the same difficulties recognized by the other chemists working along similar lines. Lamenting the lack of a practical test, he acknowledged "The chief obstacle to this much desired end, at present, is the time required and the expense involved for apparatus and chemicals where a large number of tests must be made form day to day."[30] Again, Babcock echoed his colleagues in calling for an inexpensive, reasonably accurate test that would require little skill, special equipment or materials. Researchers at the Pennsylvania Station noted ""It is impossible to adopt for the use of creameries and farmers the accurate methods employed in the chemical laboratory," and lamented that earlier tests of milk did "not furnish satisfactory data upon which to base the valuation with reference to its content of butter fat."[31] Chemists at the Cornell University concurred, calling for a milk-test "that…can be carried out without special acquaintance with chemical manipulation."[32] Failyer and Willard, of the Kansas Experiment Station, agreed, stating that the successful milk-fat test "must be so simple, so far as manipulations are concerned, as to be easily performed

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[29] S.M. Babcock, "A New Method…," 3.

[30] Ibid.

[31] Frear and Holter, 5.

[32] "A Description of Cochran's Method for the Determination of Fat in milk, for the Use of Dairymen," *Cornell University Agricultural Experiment Station Bulletin No. 27* (1890): 19.

by one who has not had training in a chemical laboratory."[33] Moreover, they maintained that "The appliances must be comparatively inexpensive," an attitude shared by Parsons, who sought "a simple and cheap method…which would give good results in the hands of any dairyman."[34]

Between 1888 and the introduction of Babcock's "new method" in 1890 no fewer than six American scientists introduced milk-fat tests. Each believed his method successfully addressed the need for a practical test that could be employed on farms and in creameries. Importantly, all shared a similar approach. Recognizing that a successful test would minimize the demands – specifically, the need for special skills or costly, delicate apparatus – placed on the user, all of these methods relied on volumetric, rather than gravimetric, testing. Making volume rather than weight the means of measurement eliminated the need for expensive scales. Instead, each of these tests employed some form of graduated glass beakers or vials, which allowed untrained farmers and creamery employees to (so the developers of these methods hoped) easily and accurately determine the amount of milk-fat in a given sample. The replacement of scales with graduated glassware not only reduced the need for costly equipment but also minimized the skill required to make an accurate reading. Instead of weighing a sample on a delicate scale the user simply read the amount of fat by referencing the graduated lines on the side of the glass cylinder.

Besides the use of specialized glassware, all of these methods called for a similar combination of materials and techniques. All employed one or more chemicals, required

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[33] Failyer and Willard, 294.

[34] Parson, 275.

the sample to be heated for a period of time, and called for the agitation of the sample. Finally, some of the techniques – including Babcock's – called for the use of a hand-powered rotary device that would spin the sample, a process that stratified the various chemical constituents by density due to centrifugal force. All of the methods employed a comparable methodology: one measured a specified amount of milk into the glassware, added one or more chemicals, and agitated the mixture. The sample was then heated, after which, in some cases, more chemicals were added. The mixture was then allowed to cool and, if necessary, spun in the rotary apparatus. Once the sample settled the tester could then (in theory) easily read the amount of it contained.

F.G. Short, Babcock's assistant at the University of Wisconsin, published the results of his research in July 1888 making it – at least according to the USDA's index of publications issued by federally-sponsored experiment stations - the first milk-fat test devised by an American agricultural scientist to appear in print. He endeavored to produce a "quick, accurate, and inexpensive method for determining the total fat in milk, simple enough to be used by persons of ordinary education…" Short's test required the use of a number of chemicals – caustic soda, caustic potash, sulphuric acid, and acetic acid – a scale, a small amount of glassware, a copper water bath, and a wash bottle. To perform the test one first measured a small amount of milk into a glass tube, added a quantity of alkali solution (the soda and potash), agitated the mixture, and heated the sample by immersing it in boiling water for two (!) hours. At this point the specimen was removed and allowed to cool slightly, at which time one added an acid mixture. The sample was then heated in boiling water for another hour before being allowed to cool. One then measured the amount of fat – which had congealed in the glass tube – and plugged the measurement into a mathematical formula (consisting of some five variables) which, upon solving, indicated the amount of fat in the milk sample. [35]

Short's method showed promise: two students, neither of whom "had any training in laboratory work," employed Short's test to produce results that closely agreed with results obtained by Dr. Babcock using the much more complicated procedure of gravimetric analysis. Unfortunately, Short's method ultimately failed to achieve wide use. While it is impossible to determine why dairymen failed to adopt his test, the main shortcoming, besides the number of corrosive chemicals called for, seems to have been the time required for the chemical agents to perform their processes; each test required the mixture of milk and chemicals to stand in heated water for at least two hours – no mean feat before the widespread use of water heaters.[36]

Charles L. Parsons of the New Hampshire Station also proposed a "simple and cheap method…which would give good results in the hands of any dairyman." His system required three pipettes of different sizes, a "slender bottle," a flask, a drying oven and kerosene stove, gasoline, caustic soda, alcohol, and "a little strong acetic acid." To perform his test required the user to mix the milk to be sampled with the caustic soda, alcohol, and gasoline; this solution was then agitated "five or six times, at about equal intervals, in the next half hour." Once the sample had separated, the user then carefully poured out the "upper liquid," filled a pipette with the "upper solution" – which was then put in a flask – and "evaporated the gasoline." The tester added "two drops" of acetic

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[35] F.G Short, A New Method for Determining Fat in Milk, *University of Wisconsin Agricultural Experiment Station Bulletin No. 16* (1888): 3.

[36] Ibid.

acid to the fat mixture that remained in the tube and this compound was put into an oven and dried "at 245º to 255º F. for one hour and a half." Finally, the contents of the flask were allowed to drip ("for ten minutes") into the measuring tube which permitted – after the mixture had cooled "until the first appearance of the fat solidifying" – the user to read off the number on the side of the graduated tube, at which point the tester could compare the number with the table included in the article to "ascertain the per cent. of fat" contained in the sample.[37]

Parson's method met with some success: in 1890 (shortly before the introduction of Babcock's test) the New York Experiment Station in Geneva, "after a careful consideration of the different methods recommended for the testing of milk," chose to promote Parson's method as "being simpler in manipulation, requiring no special skill, and giving results which compare very favorably with the gravimetric method." To test the method they provided written instruction for the test to a number of station employees "wholly unfamiliar with laboratory methods," and found the results "very gratifying," despite the fact that "the operators were unskilled in chemical work, not familiar with chemical manipulation and working from very briefly written instructions."[38]

The New York Station's advocacy of Parson's method speaks as much to shortcomings of other tests as it does to the (relative) advantages of Parson's system. First, even before submitting the test to trials the station's scientists tinkered with Parson's method by amending the amount of chemical reagents used. Second, though

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[37] Parsons, 273-277.

[38] "A Method for the Determination of Fat in Milk and Cream," *New York Agricultural Experiment Station Bulletin No. 19* (1890): 199-201.

unskilled operators performed the tests, they presumably employed the station's facilities: in other words, they operated under ideal, rather than "real-world," conditions. Finally, despite the apparent ease of obtaining accurate results, the station felt the need to offer "practical instruction to those who may choose to avail themselves of this plan." Believing that "one may, in a single day, become so familiar with this simple method of analysis, as to be able to intelligently conduct the work," the director of the station offered to set aside a day "when such practical instruction will be given at the Station," or, "if a sufficient number may agree to assemble at any given point in the State, the necessary instructions will be given to them."[39]

The director's offer touches on two central concerns. The need for personal instruction calls into question the supposed simplicity of Parson's test, and implies why Babcock's test succeeded in manner that Parson's did not. More importantly, the issue suggests that, at least in this case, distinctions between "scientist" and "farmer" were sometimes purposely blurred. The offer to train dairymen could be interpreted as an attempt to initiate farmers into the mysteries of the laboratory and, by implication, that farmers would do well to emulate scientists or, at the very least, adopt some of their methods.

G.H. Failyer and J.T. Willard of the Kansas Experiment Station introduced a third test in 1888. Familiar with Short's method, they praised the fact that it did "not depend on a knowledge of the principles involved…if the very full instructions…are carefully followed," but noted "one drawback" to Short's test: "the fact that…fully five hours will be required to complete the analysis." To this end, they proposed their own method.

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[39] Ibid.

Like the other tests theirs required a fair amount of equipment: a number of different tubes, a water bath and holding rack, a gas stove and a Bunsen burner, two special pipettes, a bellows, concentrated hydrochloric acid, and gasoline. The primary advantage of their plan was that the solution needed to be heated "briefly" and, after some further manipulation (a method that was "hard to describe," but "easy of execution") was boiled "for a few minutes" rather than the hours required by Short's method. If the user properly followed the instructions – which were considerably more complex and involved a greater number of operations than Short's method – the results garnered compared favorably with both Short's method and gravimetric analysis. Despite the relatively quick results obtained by Failyer and Willard's method it does not seem to have made much of an impact on the dairying community, and performance trials of the various milk-fat tests performed by Experiment Station's mention it only in passing.[40]

It should be noted that scientists employed by experiment stations were not alone in their search for a practical milk-fat test. C.B. Cochran, Inspector of Foods for the Pennsylvania State Board of Agriculture, introduced a patented testing method in the pages of the *Journal of Analytic Chemistry* in 1889. Cochran's test in most respects resembled earlier methods, and required similar apparatus: a boiling tub, some pipettes and a beaker, sulphuric acid, glacial acetic acid, and ethyl ether. Cochran's innovation was a special flask "provided with a side tube" which, "for the sake of convenience," Cochran dubbed the "fat indicator." Like other milk-fat tests, Cochran's method required the user to combine the milk sample with the specified chemicals, agitate and boil the mixture. At this point the sample was carefully poured into the "fat indicator" and

[40] Failyer and Willard, 294-301.

reheated. When it cooled the operator could read the amount of fat contained in the sample off markings on the "fat column" of the indicator. The primary advantage of Cochran's system, as claimed by Cochran himself, was that the amount of fat could be "more accurately read." Unfortunately, like earlier methods, Cochran's required a lengthy amount of time to complete: "two to three hours" to perform sixty tests, and then only if one employed "the largest and most improved forms of apparatus."[41]

Following on the heels of Cochran's test, the Iowa Agricultural Experiment Station introduced the Iowa Station Milk Test in February 1890. Devised by Prof. G.E. Patrick, the "principle, or plan upon which it works," was, according to Prof. Patrick, "entirely new, and yet very simple." To perform the test, it was necessary only to "dissolve the casein and albumen in the milk, by means of chemicals and heat, and to allow the melted fat to rise and collect in a narrow tube." In fact, in equipment, chemicals, and process the method resembled that of other tests, and given that Prof. Patrick intimated his familiarity with "another method of testing milk which I myself know to be an excellent one," one can find very little in Patrick's method that was "entirely new," save his employment of oil of vitriol (more commonly known as sulfuric acid) as one of the chemical components.[42]

Despite their shortcomings, the last two tests – those of Cochran and Patrick – received the highest praise from the University of Illinois Experiment Station, which in August 1890 – a month after the publication of Babcock's test, but presumably after the

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[41] C.B. Cochran, "A New Process for Determining the Percentage of Fat in Milk, Cream, or Skim-Milk," *The Journal of Analytic Chemistry* 3 (1889): 381-383.

[42] G.E. Patrick, "The 'Iowa Station Milk Test,'" *Iowa Agricultural Experiment Station Bulletin No. 8* (1890): 295-313.

Illinois Bulletin had gone to print – rated the five milk-fat tests hitherto introduced. The authors of the Illinois study lauded the Cochran and Patrick tests for "the rapidity and ease with which the details can be comprehended and a sample of milk analyzed by almost any careful person." Furthermore, they found the Cochran and Patrick tests required considerably less time to perform: approximately twenty to thirty minutes, compared with several hours for the other tests, though it is unclear how much time would be saved by these methods if one performed several tests at once. Though all of the milk-fat tests examined performed as intended – that is, produced usefully accurate results while requiring little special skill or knowledge – the success of the Babcock test soon rendered them superfluous.[43]

The Babcock test was introduced in the University of Wisconsin's Agricultural Experiment Station Bulletin No. 24 in July 1890. Acknowledging that "during the past few years a number of methods have been proposed by which the estimation of fat in milk may be accomplished without the delicate appliances of a chemical laboratory, and by persons unskilled in chemical manipulations," Babcock deemed them largely unsatisfactory: "either too complicated or too expensive to meet the needs of the practical dairyman." As the tests did provide "substantially correct" results, Babcock believed that "little improvement can be expected except in simplicity and economy of both time and money," and that his test offered "some progress in both of these directions.[44]

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[43] "Investigations of 'Milk Tests,'" *University of Illinois Agricultural Experiment Station Bulletin No. 10* (1880): 332-227.

[44] Babcock, 3.

Babcock's test utilized relatively few components: graduated test tubes, a pipette for measuring milk, a cylinder for measuring acid, the means to boil "two or three" quarts of water, and sulphuric acid. Unlike most other tests, however, Babcock's required a "centrifugal machine" which would spin one or more tubes containing the milk sample (commercial machines would eventually appear that could handle dozens of samples at once.) It remains unclear how Babcock hit on the use of such a device, though Babcock would have been familiar with the use of centrifuges in the laboratory; Ralph Selitzer, in *The Dairy Industry in America*, implies that Babcock was influenced by the DeLaval cream separator, which was introduced five years earlier and worked on a similar principle. In addition, it must be noted that H.F. Beimling devised a similar apparatus and demonstrated its use at the annual meeting of the Vermont Dairymen's Association in January 1890, some six month before Babcock described his device in print.[45] As no questions of priority appeared, it seems safe to assume that each man worked without knowledge of the other. In any case, it demonstrates that Babcock was not alone in his search for a useful milk-fat test.[46]

Babcock was singularly successful in designing a test that required a minimum of skills and materials: the only special piece of equipment required being the centrifugal machine, and Babcock included plans should readers desire to construct their own device. To perform the test one measured a sample of milk into the test-tube and added 17.5 cc of sulphuric acid, though Babcock reassured readers that "the acid need not be measured

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[45] "A New Milk Test," *Vermont State Agricultural Experiment Station, Bulletin No. 21* (1890).

[46] Selitzer, 84-85.

with great accuracy." The tubes were placed in the machine and whirled for "six to seven minutes," at which point they were filled to the neck with hot water, and served to "calibrate" the test tube. The tube or tubes were then whirled again for approximately one minute. This process pushed the fat to one end of the glassware and allowed the user to easily read the amount of fat contained in the sample from graduated lines on the test tube.[47]

Babcock's test met with immediate success; at least seven agricultural stations published bulletins that examined the Babcock test, and the University of Wisconsin was so inundated with requests for information about the process that they twice reprinted Babcock's bulletin. Researchers at the Idaho station praised the test as "simple, accurate, and easily mastered by anyone who will but give the matter careful study and attention."[48] Members of the Connecticut station echoed these sentiments, extolling the "Babcock Method as the most desirable being as rapid and as accurate as any and surpassing all others in simplicity."[49] W.J. Spillman of the Washington station perhaps waxed most enthusiastically, claiming not only that Babcock's test "has been almost universally adopted," but also that the method "has made Dr. Babcock's name a household word in every dairy district in this country."[50]

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[47] Ibid.

[48] J.H. Frandson, "Babcock Test for Butter Fat," *University of Idaho Agricultural Experiment Station Bulletin No. 63* (1908): 3.

[49] "The Babcock Method of Determining Fat in Milk and Cream for the Use of Creameries," *Connecticut Agricultural Experiment Station Bulletin No. 106* (1891): 2.

[50] W.J. Spillman, "The Babcock Milk Test," *Washington Experiment Station Bulletin No. 18* (1895): 3.

The success of Babcock's test seems to have stemmed from two primary factors. Most obviously, the wide adoption and almost universal praise afforded it suggests that Babcock best answered the call for a simple, inexpensive, and usefully accurate milk-fat test that could be employed with a minimum of training or expertise. So too does the fact that five different stations proposed various modifications – altering the amount of acid used to make it more suitable for measuring cream, or suggesting the use of slightly different beakers, for example - to the Babcock test yet for a half-century no station published the details of a method designed to supplant Babcock's procedure; Babcock's test for most practical purposes became the de facto standard in the United States. Perhaps as importantly, Babcock refused to patent the devise, instead offering it freely to dairymen, and though it is impossible to measure the importance of this act, it seems likely to have played some part in the widespread adoption – and "universal fame" accorded its inventor – of the Babcock test.[51]

More than five decades after its introduction in 1890 dairymen and creamery operators still relied on Babcock's method to test not only whole milk but, with various modifications to the basic technique, homogenized milk, buttermilk, and cream. A 1947 report published in the *Journal of Dairy Science* reported that 36 of the 48 states in the Union required the use of Babcock's test for fat testing, and 11 of the remaining 12

[51] For examples of proposed modifications to Babcock's test see J.H. Frandson, "Babcock Test for Butter Fat," *University of Idaho Agricultural Experiment Station Bulletin No. 63* (1908); J.M. Bartlett, "A Modification of the Babcock Method and Apparatus for Testing Milk and Cream," *Maine State College Agricultural Experiment Station Bulletin No. 31* (1896); W.J. Spillman, "Correction of Babcock Test for Cream," *Washington State Experiment Station Bulletin No. 32*; and P.H. Tracy and O.R. Overman, "A Modification of the Babcock Test for the Determination of Fat in Buttermilk," *University of Illinois Agricultural Experiment Station Bulletin No. 248* (1924).

recognized Babcock's test as one of several acceptable alternatives. Only Alabama, which did not specify the use of any specific test – though they did, in fact, require milk to be tested – did not explicitly recognize the validity of Babcock's technique.[52]

Nor was Babcock's method considered an artifact of the past. As late as 1945 dairy scientists still proposed various methods to make the test more accurate, more widely applicable on a range of dairy products, or easier to perform. A review of the literature demonstrated that the bulk of these experiments involved tinkering with the amount of sulphuric acid employed as a reagent and/or the temperature to which the sample was heated before taking the measurement. The most common complaint raised about Babcock's technique stemmed from criticism that it did not provide consistent results when applied to homogenized milk. Researchers at Michigan State University surveyed a dozen of the proposed modifications in 1945 and found that the problem stemmed from the fact that the reagent often failed to react with the entirely of the milk sample. Testing a wide variety of proposed solutions, the Michigan State team recommended adding the sulphuric acid in three doses rather than all at once; they also found that increasing the amount of time the sample spent on the centrifuge as well as the speed at which it was tested helped to ensure that the reagent mixed completely with the sample of homogenized milk. With these modifications the researchers found the Babcock test consistently produced reliable readings.[53]

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[52] B. Heinemann et al., "A Study of Procedures Used in Conducting the Babcock Test in Various States, *The Journal of Dairy Science* (1947): 963.

[53] G.M. Trout and P.S. Lucas, "A Comparison of Various Modifications of the Babcock Test for the Testing of Homogenized Milk," *The Journal of Dairy Science* (1945): 901-920.

The fact that several of Babcock's contemporaries, including his own colleagues at the Wisconsin experiment station, all attempted to devise a practical milk-fat test demonstrate that Babcock's goals fell well within the contemporary mainstream and contradict, or at least modify, the portrait painted by historians who suggest that Babcock's development of a successful test came entirely out of the blue. Moreover, the fact that Babcock was only one of several researchers seeking the same end does nothing to diminish the importance of his accomplishment. If anything, it rescues Babcock from the myth of "Yankee ingenuity" by accurately portraying him as the well-trained academic scientist that he was rather than as an intuitive savant operating outside the mainstream.

That Babcock – who throughout his career usually (quite successfully) followed his own interests – set aside his own research to investigate milk-fat tests also serves to demonstrate the importance that Babcock and his contemporaries placed on finding useful, practical tests that could be used outside the laboratory and would allow dairy farmers to increase the profitability of their herds. In addition, it also illustrates the way that Babcock and his fellow researchers understood the physical world and the way that they went about investigating it. They believed that they could break down the substances under investigation into discrete components, and that measuring these components would lead to truths which, ideally, would lead to not only better understanding of the, but to a fundamentally better, world.

However, demonstrating that Babcock was not alone in seeking to develop a milk-fat test tells only half the story. It suggests that the milk-fat test was somehow exceptional. In fact, Babcock devised tests to measure specific properties of physical substances throughout his career. Furthermore, several of these tests were designed not for use in the laboratory but for practical use on farms and in creameries. From his earliest days working for the New York experiment station in Geneva through his retirement from the Wisconsin station some three decades later, Babcock regularly devised practical tests.

In 1883 Babcock announced the invention of a new device that would allow researchers to determine the viscosity of liquids. Though viscometers (sometimes called viscosimeters) had long been employed by laboratory scientists, Babcock, who was then engaged in studying the effect of the viscosity of milk on its creaming and churning properties, found that the solids suspended in milk quickly plugged the jets of the viscometers then generally employed. Babcock experimented with a number of mechanical viscosimeters but, dissatisfied with the results he obtained using them, devised his own device.[54]

As described in the *Journal of Analytic and Applied Chemistry*, Babcock's apparatus consisted of "a disc of metal supported in its axis by an elastic wire, the torsion of which causes the disc to oscillate when a motion of rotation is imparted to it. At the lower of this axis is rigidly attached a hollow cylinder…which is immersed into the liquid to be tested." To perform the test, one lowered the hollow cylinder into a tank containing the substance to be measured. The metal disc was then rotated 360 degrees and released. Using a protractor and magnifying glass attached to the apparatus, one then measured the arc of the disc's rotations. A simple formula allowed the operator to graph the

[54] S.M. Babcock, "A New Viscometer," *The Journal of Analytic and Applied Chemistry* 1 (1887).

diminishing arcs, which provided a visual representation of the liquid's viscosity. As each liquid slowed the rotating cylinder a different rate, one could them compare the viscosity of various substances. Those in which the cylinder slowed more rapidly therefore displayed a higher viscosity, while "thinner" liquids would slow the rotating cylinder more gradually.[55]

Babcock's article included an illustration of his apparatus, and showed a simple, easily constructed device that required only one "scientific" component: the protractor used to measure the arc of rotation. Having obtained the necessary measurements, the operator employed a simple mathematical technique, which could be performed using the common slide rules then in wide use, to calculate the viscosimetric reading for the liquid under scrutiny. Like his milk-fat test, Babcock's viscometer was a model of simplicity, requiring little in the way of equipment and very little training or skill on the part of the operator. More importantly, the device produced usefully accurate results.[56]

Babcock claimed that his device possessed enough accuracy to detect not only adulterants added milk but even to contaminants contained in sugar that was then dissolved in water. Babcock designed the machine because he believed that "the churning and creaming qualities of milk were largely dependent upon its viscosity."[57] *Man of Science*, on the other hand, suggests that Babcock employed the device to detect adulterants in milk and that, furthermore, the device was "used commercially."[58]

[55] Ibid., 155.

[56] Ibid., 156-158.

[57] Ibid., 151.

In either case, Babcock's viscometer and his milk-fat test share some important similarities. Both required little skill to employ, required no expensive or difficult to obtain components, and both produced usefully accurate results. Moreover, Babcock's development of his viscometer some seven years before he introduced the milk-fat test demonstrates that the milk-fat test was not produced as a fluke or on a whim but instead illustrate Babcock's commitment to practical science that could be employed in the "real" world. It also illustrates Babcock's approach to science: to "do" science was to first determine which aspect of a substance one wanted to measure, and then devise a test that allowed the quantification of that aspect.

Babcock revisited the viscosimeter more than a decade later. His experiments on pasteurized cream showed that pasteurization reduced the consistency of cream. Babcock and his associate H.L. Russell devised a means of restoring the consistency by adding a solution of lime in sugar called viscogen. However, before adding viscogen one had first to determine the relative viscosity of the different milks and creams. Because "the methods usually employed for measuring viscosity are as a rule too complicated to admit of practical application," Babcock devised "a simple viscometer for the determination of consistency of cream."[59]

Described in Wisconsin Experiment Station Bulletin no. 54, Babcock's new viscometer was a model of simplicity. It consisted solely of piece of glass – "preferably plate or picture glass" – onto which the user placed drops of the creams being sampled in

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[58] Russell, 30.

[59] S.M. Babcock and H.L. Russell, "The Restoration of the Consistency of Pasteurized Cream," *Wisconsin Agricultural Experiment Station Bulletin No. 54*, (1896), 3.

a row “near one edge of the glass plate.” The tester than simply inclined the plate “at an angle sufficient to cause all of the cream droops to flow slowly down the plate. Creams having the heavier body move more slowly…” Comparing the lengths of the streaks allowed the operator to determine the relative viscosities of the creams under investigation.[60]

To be sure, the test possessed some limitations. As Babcock noted, the test was “purely a relative measure” that “cannot be used for the determination of consistency referred to any particular standard.” Despite its simplicity, Babcock’s test fulfilled its intended purpose in an elegantly straightforward manner. Like the other tests he designed, this simple viscometer reveals Babcock’s conviction that scientists devise methods that allowed the application of laboratory methods in the “real” world. Too, the test confirms Babcock’s conception of science: that “doing” science consisted of devising methods to measure the various aspects of the substance under investigation and further, that this method held practical, or applied, as well as theoretical implications.[61]

Wisconsin Experiment Station Bulletin no. 31, issued in 1892, consisted of two sections. The first reprinted, in slightly revised form, the station’s bulletin no. 24, which introduced the Babcock milk-fat test to the world. The second section contained Babcock’s instructions on the use of a lactometer to detect adulterations in milk. For Babcock, the use of the lactometer would complete the task begun with his invention of the milk-fat test. Just as the test quantified the milk-fat contained in milk, the lactometer allowed dairymen on both farm and factory to calculate the amount of non-fat solids

[60] Ibid. pp. 3-4.

[61] Ibid. p. 3.

present in a sample of milk. Doing so had important implications, not only to prevent abuses against farmers, but "to maintain a fair quality of milk and insure the public against frauds." The most common scam was to offer for sale milk which had been "skimmed" until its fat – and, in Babcock's eyes, nutritional - level approached zero; also popular with unscrupulous milk suppliers was the adulteration of milk with water or other substances. Babcock continued: "To detect adulterations it is necessary to determine both the fat and the solids not fat. If either of these is below the legal standard, the milk must be considered adulterated even if it has not been tampered with after being milked."[62]

To prevent these abuses, intentional or not, Babcock recommended the use of a lactometer in conjunction with his fat test. The lactometer was a simple apparatus, consisting of "a narrow stem to which is attached an elongated bulb weighted at the bottom so as to float in an upright position in milk, with the stem partially submerged. The depth to which the lactometer sinks depends upon the specific gravity of the liquid in which it is placed." Babcock included instruction for employing the lactometer, including conversion formulas for milk at various temperatures, and tables to help determine the presence of adulterated milk.[63]

Though Babcock did not invent the lactometer, his advocacy of its use again shows his conception of the place and uses of science. Specifically, it reveals that Babcock viewed scientific laboratory methods as perfectly applicable – and beneficial –

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[62] S.M. Babcock: "Notes on the Use of the Babcock Test and the Lactometer," Wisconsin Agricultural Experiment Station Bulletin No. 31, Madison, WI, 1892, pp. 17-18.

[63] Ibid. p. 18.

in the “real” world. Applying laboratory techniques in the real world promised a better future for everyone: farmers would receive fair payment for their products, creameries would receive better materials, and, most importantly, the consumer could rest assured that he or she had purchased nutritious dairy products.

Too, it once again demonstrates that Babcock viewed the world as consisting of discrete components that could be quantified to make meaningful statements about the substance under investigation. Furthermore, this need not take place within the laboratory. In fact, by performing these tests themselves farmers, creameries, and public health inspectors, if not the public itself, could guarantee the quality of the merchandise under consideration.

Hence an understanding and appreciation of Babcock’s life’s work serves a number of historically useful purposes. It reveals that Babcock took seriously the mandate of the Hatch act that the agricultural experiment stations produce “practical” knowledge. To be sure, Babcock, like his contemporary agricultural scientists, was an accomplished and prolific researcher in the laboratory. Indeed, the publicity he gained as a result of the success of his milk-fat test has tended to overshadow his successes as a “theoretical” scientist. This is unfortunate, as Babcock made important contributions to the understanding of the constituent components of milk, on the role of different bacteria in the production of cheese, in the function of “metabolic” water in the respiration of plant and animal life, and several other fields including, as will be discussed in following chapters, animal nutrition.

However, as the examples presented in this overview suggest, Babcock also took seriously the task to applying theoretical science to the “real” world. They reveal that

Babcock viewed science as having practical value outside of the laboratory. True, scientists could use his milk-fat test to quickly measure samples in the lab, but Babcock plainly saw these techniques as having value for dairymen, creameries, and the public. Dairymen could not only use the test to avoid being cheated by unscrupulous creameries but could employ the test to weed out low producing cattle, to increase the profitably of the herd, and to experiment with different foodstuffs. In short, the use of Babcock test helped the farmer to maximize the profitability of his farm. Nor did only the farmer benefit from this plan: so did the creamery, which could now rely on a more consistent product, as well as the consumer, who was more assured of receiving a valuable product.

Perhaps as importantly, these tests show that Babcock not only hoped to introduce scientific *techniques* to the public, but *science*, as he and his colleagues understood it, itself. That is, while he certainly devised his tests to require a minimum of skill, to produce accurate results, and to need little in the way of expensive or difficult to obtain equipment, he did not "dumb-down" the science involved. Those who employed his tests measured the same quantities for the same reasons as did Babcock and other scientists. Babcock simply packaged the procedure in such a way that they, first, were easy to perform and, second, produced results that were of benefit to those performing the exams.

In this way Babcock promoted his conception of science. Like his contemporaries, he believed the world consisted of discrete substances that he could measure, and that these measurements allowed him to make meaningful predictions about their behavior. Just as Babcock adhered to this worldview in the laboratory, so did he attempt to facilitate the public's employment – and, presumably, their understanding – of science. The real triumph of the Babcock test, therefore, lay as much in the fact that it promoted Babcock's conception and understanding of science as it did in answering the call for a practical milk-fat test.

CHAPTER 3

DAIRY SCIENTISTS SEARCH FOR DIRECTION

The example of Stephen Babcock as described in the first chapter clearly demonstrates the dominance of the prevailing scientific orthodoxy. Regardless of his personal eccentricities, throughout his career Babcock practiced science in the same manner of his contemporaries, if perhaps more successfully than most. Babcock's colleagues might have gossiped about his behavior, but none complained about the manner in which Babcock practiced his craft. His professional writings confirm that Babcock was a careful experimenter who charily took pains to clearly explicate his processes and verify his results. Several sources suggest that Babcock's associates at the University of Wisconsin urged him to unveil his milk-fat test before it had been proven to his satisfaction, and Babcock published his famous bulletin only when he was certain it would perform as described in all cases.[64]

Quantification and isolation comprised the hallmarks of science as it was practiced by Babcock and his contemporaries. Scientists believed they could reduce the physical world to a number of discrete constituents. Having done so, they continued by measuring the various elements. Once they had established a baseline for the behavior, ration, property, etc. in question, they would then isolate one element to determine how

[64] See John T. Schlebecker, *A History of American Dairying* (Chicago: Rand McNally, 1967), 31 and Ralph Selitzer, *The Dairy Industry in America* (New York: Magazines for Industries, Inc., 1976), 84.

changes in that aspect affected the whole. By repeating the process they trusted they could gain an accurate knowledge of the whole. As Alan I Marcus describes the process as practiced by plant scientists, “researchers took a limited number of plants and excluded all but one variable. That process was repeated but each time a different variable remained free. From these few tests, investigators ‘proved’ conclusively that plants did not get their nitrogen from the atmosphere.” Though dairy scientists explored different subjects, they employed an identical methodology.[65]

The advantage of this approach to science stemmed from its adaptability. With appropriate modifications researchers could – and did – apply the methodology to a bewildering array of investigations. Regardless of the focus of their inquiries dairy researchers employed a similar modus operandi: whether testing cattle rations, evaluating milking machines, or resolving whether the temperature of water provided to cattle influenced the amount of milk they produced, scientists went about their business in similar manners. Doing so made their work understandable to their colleagues and allowed researchers to more easily communicate their findings to their fellows.

Moreover, by practicing science in accepted ways researchers helped establish and reinforce the scientific methodology advocated by proponents of the newly formed research stations, a system that, according to Marcus, “was based on the division of each agricultural question into its constituent parts, the provision of an appropriate specialist to perform each investigative operation, and the coordination of relevant staff on each experiment.” The system of agricultural experiment stations founded and/or funded by the Hatch Act itself encouraged scientists to practice their craft in a certain manner.

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[65] Alan I Marcus, *Agricultural Science and the Quest for Legitimacy…*, 63-64.

Whatever the other benefits of scientific orthodoxy, following its dictates also reinforced the claims of science advocates in their debate with supporters of more traditional agriculture.[66]

However, for scientists to agree about how to go about their work proved rather easier than did reaching a consensus about what their findings meant. The example of researchers studying the nutrition of dairy cattle serves to illustrate this point. Though nutrition researchers almost universally agreed about the goals of their work, and, for the most part, followed a similar research methodology, they differed in their interpretation of the results of their investigations. For example, two scientists could analyze identical samples of corn, perform the same tests, and reach similar conclusions about the chemical composition of the ration. Yet, depending on their understanding of the role played by the different components one scientist might wholeheartedly recommend – and another just as strenuously caution against – feeding the ration to dairy cattle. The example of nutrition scientists thus affords an opportunity to examine some of the limitations of the science practiced by these researchers.[67]

Nutrition scientists practiced their trade in the manner discussed above: by isolation and quantification. Researchers began by deciding what foodstuff they wished to study. Having done so, they would determine the amount of protein (or "nitrogenous matter"), carbohydrates (or "non-nitrogenous matter"), and fat (or "ether extract") contained in the ration. Scientists then fed a group of cattle a diet comprised exclusively of the fodder under investigation. Comparing the chemical composition of the ration

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[66] Ibid. 102.

[67] For typical cases, see any of the numerous feeding manuals referenced in this chapter.

with that of the dung produced by the cattle allowed scientists to establish the amount of digestible nutrients the foodstuff contained. In a nutshell, dairy scientists isolated one variable – the ration itself – and then quantified the amount of nutrients that the animal assimilated from that ration.

This approach proved admirably suited to the research methodology advocated by proponents of agricultural research stations. Scientists at the various stations could each make a valuable contribution. Those who lacked the skill, desire, or elaborate laboratory apparatus to otherwise participate in the study of dairy nutrition could still perform tests on locally available foodstuffs. Feeding trials also involved numerous members of the experiment station staff: scientists devised and oversaw the experiment, assistants fed the cattle and collected dung samples, and technicians performed the actual chemical analyses necessary. In this manner, the experiment station represented in microcosm the ideal world pictured by the advocates of agricultural research described by Marcus in *Agricultural Science and the Quest for Legitimacy*.[68]

Animal nutrition also provided an ideal platform for the dissemination of scientific agriculture to "ordinary" farmers who, after all, had to feed their animals regardless of their inclination to practice "scientific" agriculture. Between 1880 and 1920 well over a dozen different feeding manuals appeared on the market. Though sales records do not exist, the fact that several of these volumes went through multiple printings and editions suggests that these works enjoyed a wide circulation. These texts, explicitly written for a popular audience, aimed to promote the advances made by agricultural scientists and promote the benefits that would accrue farmers who adopted

[68] Marcus.

the methods they advocated, and purported to represent the latest findings of the scientific community. The prefaces of these works – roughly half of which were written by current or former faculty at agricultural experiment stations or colleges - demonstrate that the writers framed their argument in terms that would not alienate skeptical traditional agriculturists. The authors of the numerous texts on animal feeding almost universally took pains in the introductions of their works to assuage the concerns of farmers who either distrusted the claims made by agricultural researchers or feared that acting on the advice of scientists might compromise their autonomy as agricultural experts in their own right.

To this end, writers employed a number of rhetorical devices to convince farmers of the usefulness of their work. For example, in the preface to his *Manual of Cattle Feeding* Henry Prentiss Armsby began by noting that scientists had made prodigious advances in their understanding of animal nutrition in the last two decades. However, rather than castigating farmers for ignoring this work he instead pointed the finger of blame at scientists, whose works "are largely inaccessible to the majority of American feeders" and agricultural publications, which "deprived" from reports of these findings "much of their good effect by their necessarily fragmentary character." To rectify these problems Armsby hoped to present the findings of nutrition scientists "in a connected and systematic form to American farmers…an attempt which, so far as the author is aware, has not before been made."[69]

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[69] Henry Prentiss Armsby, *Manual of Cattle Feeding, Second Edition* (New York: John Wiley and Sons, 1898), iii.

W.A. Henry, whose *Feeds and Feeding* appeared in 1898 and remained in print for over sixty years, going through some twenty editions, took a different tack in appealing to farmers in the first edition of this work. Henry, Dean of the College of Agriculture and Director of the Agricultural Experiment Station at the University of Wisconsin, explicitly acknowledged the expertise of farmers and stockmen, noting "That stock feeding is an art and not a science, and that experience and judgment must rule in its successful conduct." Despite these observations, Henry shrewdly turned the tables on farmers by noting that "facts and truths are same whether their repository is a book or the human mind. Held by the latter, all perish with the possessor; in the keeping of the former, the whole world may be benefited." Conflating knowledge gleaned from books with that won from experience allowed Henry to assert the merits of science without offending farmers: "Abstract knowledge cannot take the place of experience, though it will prove of the highest value when both are rightly combined."[70]

Both authors closed their introductory remarks by asserting that both agricultural scientists and farmers sought the same ends: the more economical – and hence profitable – production of beef and milk. Describing the advances made by nutrition scientists, Armsby noted that "The ultimate object of this branch of applied science is, of course, to enable us to feed better and more economically."[71] Henry echoed this sentiment: "The stockman who in addition to experience possesses some knowledge of the composition of the nutrients of feeding stuffs…is certainly better equipped for wisely and economically administering feed to the animals under his care." Linking science with the farmer's

[70] W.A. Henry, *Feeds and Feeding* (Madison, WI: self-published, 1898), iii-iv.

[71] Armsby, iv;

pocketbook allowed these authors to assert the merits of scientific feeding without explicitly claiming its primacy over traditional methods.[72]

Though scientists almost universally agreed not only about the methodology they employed but about the goals of their investigations, they found it more difficult to reach consensus about the meaning of their findings. The most common disputes centered on establishing the "best" nutritive-ratio for cattle rations. Establishing the nutritive-ratio of individual foodstuffs proved a rather straightforward task, as did combining various feeds to meet any desired nutritional standard. Instead, debate centered on exactly what ratio would provide the most beneficial results. Some scientists advocated a "narrow" ratio such as 1:3 (one part protein to three parts combined carbohydrates and fats) that contained a high percentage of protein while others advocated much "wider" ratios of 1:6 or even higher.

Researchers also disagreed about whether the amount of milk and/or milk-fat an animal produced should factor into the ration the cow received. One camp maintained that dairy cattle should be fed based solely on the animal's weight, while another asserted that the animal's production should factor into the formulation of its rations. A smaller group of scientists believed that the length of lactation should play a role; they held that animals that had recently freshened should receive a different ration than an animal that had been producing milk for several months.

Clearly, then, scientists had a much easier time agreeing on how to go about their work than they did deciding what the results they produced meant in practice. Throughout the period scientists debated the merits of different feeding standards, and

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[72] Henry, iv.

though by approximately 1920 most researchers advocated one of a handful of feeding plans the discovery of other essential components in foodstuffs – what eventually became known as vitamins – added further complexities and sources of disagreement.

An appreciation of this process is essential to understanding the emergence of the recognizably modern dairy farm. Throughout the period examined in this work dairy scientists steadily increased their influence. Though not all dairy farmers followed the lead of researchers, the number practicing "scientific" farming techniques grew. The formation of dairy herd improvement associations, in which farmers banded together to hire a professional to test their cattle on a regular basis, and the steady increase in the number of farms enrolled in such programs suggests that more and more farmers appreciated the benefits offered by science. So too does the publication of dozens of dairy manuals, several of which went through several printings, that purported to distill the latest finding of researchers for practical use on the farm.

Perhaps the clearest example of the growing pervasiveness of scientific farming could be seen in the pages of the leading dairy journals. Nearly every issue of *Hoard's Dairymen* and *Kimball's Dairy Farmer* reported news of the latest discoveries and often summarized the findings of recently issued research bulletins published by the various experiment stations and agricultural colleges. Moreover, the articles carried in these periodicals regularly employed scientific language, and usually assumed the reader possessed at least a rudimentary understanding of scientific concepts. The adoption of scientific terminology suggests that scientists had successfully set the tone for future debates. Farmers might well reject the findings of professional researchers, but, increasingly, they had to meet scientists on the scientists' own terms.

Hence the example of dairy nutrition serves to illustrate how agricultural scientists – a group that had not even existed let alone influenced policy and practice a century earlier – came to play an integral, and eventually a leading, role by the early twentieth century. An understanding of this process accentuates our understanding of how these researchers and other experts emerged from obscurity during the Progressive Era.

Of course, the science of nutrition – and nutrition scientists - did not emerge from thin air. American scientists based their nutrition research on the theories of Justus Liebig and his German disciples. Of these, the Berlin-born Emil Theodor Wolff proved the most influential among dairy nutritionists. Wolff, trained as a mineral chemist, was influenced by Liebig's writings, and in the 1840's shifted his focus to agricultural chemistry. In 1851 he founded the "Versuchsstation" – the world's first agricultural experiment station – in the German city of Moeckern. His work attracted the attention of several American students and scientists studying in Germany, including Samuel W. Johnson, who later taught at Yale's Sheffield Laboratory, and Evan Pugh, who eventually became president of Pennsylvania State University. These men, in turn, introduced Wolff's work to American students and scholars.[73]

Wolff's most important contribution to the science of nutrition was the concept of the "nutritive ratio," a figure that related the amount of digestible nitrogenous matter (protein) to the amount of digestible non-nitrogenous matter (carbohydrates and fats) in the ration. The first figure in the ratio, always "1," represented protein, the second number the other digestible constituents of the ration. Thus, a feed that contained high

[73] See Rossiter, 127-148.

amounts of protein relative to the other digestible components might have a nutritive ratio of 1:3, while a foodstuff with little protein could have a nutritive ratio 1:10 or even higher. The employment of such a ratio made it easy to compare the relative merits of various foodstuffs, and American nutritionists were quick to adopt the concept.

Despite its success, the widespread adoption of the nutritive ratio would eventually prove something of a mixed blessing. While its use allowed scientists and non-scientists alike to quickly and easily understand the makeup of various rations, it also acted as something of an intellectual strait-jacket. By relying on only three factors – proteins, carbohydrates, and fats – the use of the nutritive ratio discouraged analysis of other constituents of feeds that might – and as it was later found, do – play an important and even vital role in nutrition. As will be discussed in a later chapter, the scientists who discovered and identified what would later become known as vitamins faced not only the challenge of convincing both their scientific colleagues and farmers of the importance of their work, but in devising means to meaningfully measure and relate the amount of vitamins contained in various rations.

Unfortunately, the widespread adoption of the nutritive ratio did little to spread consensus about animal nutrition. Rather, it fostered debate. Almost all scientists agreed about the importance of ratio, and they almost universally concurred about how to perform the chemical tests required to compute the ratio. Discussion centered, instead, on what the nutritive ratio actually meant, and what ratio was most beneficial. In effect, the adoption of the nutritive ratio raised almost as many questions as it answered, and led to disagreements as scientists debated about the merits of different ratios to herd health and production.

Henry Prentiss Armsby proved the most successful booster of Wolff's theories in the United States. In 1880 Armsby, then a chemist at the Connecticut Agricultural Experiment Station, published the first edition of the *Manual of Cattle Feeding*. Armsby set out to translate Wolff's influential "Landwirthschaftliche Fütterüngslehre" – literally "Agricultural Feeding Theory" – into English. However, Armsby "soon found…that considerable additions and changes were required to suit it to American readers." Rather than provide a faithful translation, Armsby combined the theories of Wolff with the findings of other German and American researchers to produce a substantially new work. The book proved popular, and went through at least six editions between its appearance in 1880 and 1898, when Armsby began work on *The Principles of Animal Nutrition*, a work that signaled – at least for Armsby – a dramatic change of focus that will be discussed in detail later in this work.[74]

In his *Manual of Cattle Feeding* Armsby carefully detailed how scientists computed the nutritive ratio, which he described as "the ratio of the digestible protein to the digestible non-nitrogenous nutrients." He enumerated at some length the advantages of the use of the nutritive ratio. Foremost among these, the nutritive ratio "presents the results of careful experiment and observation in a concise form, and one admitting of practical application." The "practical" utility of the ratio stemmed from the fact that its use allowed farmers to concoct equivalent rations from whatever foodstuffs they could purchase most economically. Any two rations that possessed the same nutritive ratio and amount of digestible matter would – according to this theory - provide the animal with an

[74] Henry Prentiss Armsby, *Manual of Cattle Feeding, Second Edition* (New York: John Wiley and Sons, 1900), v.

identical amount of nutrients: "When his cows are thus fed, though they may not consume the same kind or weight of fodder…they will resorb into their systems the same amounts of protein, fat, and carbhydrates [sic], and will therefore be equally well nourished." The only difficulty in this method lay in supplying animals with roughly the same volume of food, a problem easily surmountable by the use "of bulky fodder, containing much indigestible matter and serving to make up the necessary volume."[75]

Armsby also included an inventory of limitations inherent with the use of the nutritive ratio for compounding rations. First, he acknowledged that the accuracy of feeding charts that listed the ratio of foodstuffs "must depend on the extent and accuracy of the observations on which they are based." Those rations which had been well tested were "worthy of much confidence" but others, "based on but few observations…are confessedly only tentative." Armsby recognized that different situations demanded different rations: "it is plain that a single feeding standard cannot possibly take account of all the varying conditions that arise in practice." He cautioned that feeding standards could indicate the foods "*in general* best adapted to the end in view," and warned that the "*unintelligent* use of feeding standards is quite as likely to result in failure as in success."[76]

Despite these disadvantages, Armsby ultimately pointed to the benefits of employing feeding standards that would produce rations that provided the desired nutritive ratio: "The convenience of possessing such a standard is obvious…with the feeding standard he [the farmer] has simply to calculate, by the aid of a table…what

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[75] Ibid. 51, 367-369.

[76] Ibid., 369-370. Italics in original.

quantities of the materials at his disposal will give the amounts of the various constituents and the bulk which the standard calls for." Armsby continued "The aid which such a method of calculation gives in comparing the experience of different observers is not easily overestimated; it reduces the heterogeneous observations to a comparable form, and to one which shows exactly in what direction the ration is defective, if it is so at all."[77]

Thus, Armsby suggested that the use of the nutritive-ratio, and of feeding standards based on a specific ratio, allowed farmers in the barn to imitate scientists in the laboratory. Correctly employing feeding standards eliminated personal bias by reducing human interference to a minimum. Also, performing the calculations repeatedly – and, importantly, in a similar manner – fostered the development of consensus. The use of nutritive ratio not only allowed farmers to act as scientists, but put all the players on a equal footing by allowing anyone versed in the meaning of the nutritive-ratio to easily compare various foodstuffs. While on the one hand Armsby urged farmers to adopt the findings and the methods of scientists, on the other he assured them that the use of the nutritive-ratio allowed them to easily compare finding and results. In a nutshell, Armsby suggests that farmers could join the scientific fold but, in addition, could play an active role in the process. Again, Armsby use of inclusive language demonstrates one of the methods whereby scientists hoped to gain the trust of farmers.

Yet even in his *Manual*, one of the earliest and most popular works to popularize the use of the nutritive-ratio, Armsby demonstrated the disagreements that would characterize nutrition science. In the introduction to the work Armsby revealed that what

[77] Ibid., 378.

had started out as a translation of Wolff's work became something different. What Armsby labeled "one of the most marked changes" between Wolff's work and his own was the "substitution…of Kühn's [a noted German animal nutritionist] tables of the composition and digestibility of feeding-stuffs for those of Wolff." Moreover, Armsby did "not accept all of Kühn's opinions," but "felt justified in making the substitution names, though aware that Kühn's views, on some points, are warmly opposed by Wolff." Clearly then, disagreements about the meaning of nutrition research emerged early in the process, and were fostered not only by authors who proposed nutritional schemes but by well-trained professionals like Armsby, who seemingly selected what he considered the most plausible schemes from various sources.[78]

Despite these disagreements American dairy researchers quickly and enthusiastically embraced this vision of nutrition research. Between 1890 and 1920 American agricultural experiment stations issued hundreds of bulletins that detailed the results of those stations investigations into the nutrition of dairy cattle. While a large percentage of those bulletins were published by states – like New York, Pennsylvania, and Wisconsin – that, because of climate, soil, natural crops, and proximity to lucrative markets had long specialized in the production of milk and cheese, virtually every other state and territory issued at least one, and in some cases dozens, of publications. State experiment stations from Florida and Mississippi in the Southeast to Oregon and Washington in the Northwest issued bulletins describing the findings of their own experiments with dairy nutrition.[79]

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[78] Ibid., v.

The vast majority of these publications elicited little controversy. Rather than question the methodology employed by their peers, the authors of these bulletins instead tended to use them to present the findings of their queries into the value of native food crops. In making these inquiries researchers followed orthodox and well-accepted practices: they selected a fodder to test; performed a chemical analysis to establish the composition of the ratio; fed a group of cattle exclusively on that ration; and analyzed the dung produced by these animals to determine the digestibility of the foodstuff under investigation. Doing so allowed them to establish the nutritive-ratio of locally grown – and hence usually more readily and inexpensively available – crops.

Following this plan allowed the researchers involved to participate in the research system advocated by proponents of experiment stations. Performing these tests required little in the way special, expensive equipment yet allowed the scientists to make valuable contributions to the advancement of scientific farming. At the same time, by studying and promoting locally grown crops scientists could more easily justify their work to the state taxpayers that contributed to the support of the stations. There were, then, a number of both scientific and practical reasons for researchers to embrace a common methodology in studying dairy nutrition.

Thus researchers at the Ohio Station examined the benefits of substituting locally grown sugar beets for corn silage; they found that on a diet of silage "the cows not only gave more milk on the average, but also showed a greater average increase in live

[79] Missouri, for example, while not usually considered a dairy state, published a large number of bulletins in the first decades of the twentieth century.

weight."[80] About the same time, scientists at Michigan's station performed tests on a somewhat surprising variety of crops, including sorghum, kaffir corn (a variety of sorghum that does not produce enough saccharine to be valuable as a source of molasses), vetch (a variety of pea that grew widely in Michigan), mangolds (a type of turnip), carrots, sugar beets, rutabagas, and potatoes. They found that when judiciously combined with other crops all these foodstuffs could be readily incorporated into dairy rations, though the researchers warned that "Turnips and rutabagas need be used in the dairy with extreme caution, because of their liability to impart an unpleasant taint both to the milk and to the products made from it."[81]

Workers at experiment stations located in the southeast also experimented with locally grown foodstuffs. Researchers at Mississippi's Experiment Station near Starkville studied the affects of the substitution of locally grown cottonseed meal for the more widely used wheat bran; a pair of trials led the staff of the station to declare "the superior value, pound for pound, of cottonseed meal over the wheat bran." In addition, they demonstrated that cowpea hay, widely found in Mississippi, made an acceptable

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[80] Chas E. Thorne, J. Fremont Hickman and F.J. Falkenbach, "Experiments in Feeding for Milk," *Ohio Agricultural Experiment Station Bulletin No. 50* (1893): 72. The various state agricultural colleges and experiment stations produced a voluminous literature dealing with the feeding of locally grown products; those listed are typical but represent merely a small sample. For other typical examples, see "Feeding Dairy Cows," *Iowa State Experiment Station Bulletin No. 32* (1896); "Methods of Dairy Feeding," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 56* (1901); "Cattle Feeding in Colorado," *Colorado Agricultural Experiment Station Bulletin No. 34* (1896); "Experiments in Feeding for Milk," *Ohio Agricultural Experiment Station Bulletin No. 50* (1894).

[81] Clinton D. Smith, "Feeding Dairy Cows," *Michigan State Agricultural College Experiment Station Bulletin No. 149* (1897): 105.

replacement for Johnsongrass Hay.[82] The Arkansas station also tested a variety of crops widely grown in the south, including a number of by-products such as peanut vines and cotton-seed hulls. They found that cotton seed hulls, though containing intrinsic nutritional value, could serve a useful role by adding bulk to rich rations: "they seem well adapted as a dilutant for…highly concentrated food material and are probably better suited for the purpose than almost any other substance which could be obtained – certainly better than any which could be obtained in the vicinity where used." The Arkansas station also advocated the feeding of cotton seed meal and cowpeas, both of which provided large amounts of valuable protein in relation to their weight.[83]

In preparing his 1908 book *The Farm Dairy* H.B. Gurler, President of the National Dairy Show Association, "collected rations from widely separated parts of agricultural America." In response to his queries researchers from a dozen state agricultural colleges and experiment stations provided sample rations that they recommended for use by local farmers. The answers provided by station workers reveal that they endeavored to construct rations from locally grown sources as much as possible. Scientists in the south often advocated the use of cotton seed meal: both the rations submitted by the Kentucky Station, all six of those offered by Georgia State College, and all ten provided by the Mississippi Agricultural College included cotton seed meal as a component of the ration. Responding to Gurler's inquiries, John Michels, Professor of Animal Husbandry at the North Carolina College of Agriculture, felt it important to

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[82] J.S. Moore, "Feeding Dairy Cows," *Mississippi Agricultural Experiment Station Bulletin No. 70* (1901): 7.

[83] "Stock Feeding," *Arkansas Agricultural Experiment Station Bulletin No. 30* (1894): 153.

iterate that he held “a position in regard to the feeding of cottonseed hulls which will interest all southern dairymen.” Michels maintained “it would not be desirable to feed any of this material to cows for any length of time, as…the continuous feeding of cotton seed hulls will soon make a poor producer out of a good one” – a contention, it should be noted, not advanced elsewhere.[84]

Southern researchers were not alone in favoring locally grown rations. Scientists from the Midwest – from the Universities of Wisconsin, Illinois, Minnesota, Nebraska, and Iowa State College - almost universally proscribed the liberal use of corn, which, according to respondents from the University of Illinois’s Department of Dairy Husbandry, “is the best fat producing feed for dairy cows.” H.W. Wing of the New York State College of Agriculture, on the other hand, noted that “Dried distillers’ grains are extensively used…as a protein food,” and the ration provided by another source from the Northeast, the University of Vermont, also included dried brewers’ grain.[85]

Several authors commented that they advocated different rations depending on the resources of the farmer. The response from the Department of Dairy Husbandry at the University of Illinois, for example, noted that “we send you a few rations, with the attempt to suit them to farmers who have neither silos nor alfalfa.” H.G. Van Pelt, Assistant Professor at Iowa State College, also qualified his response, merely mentioning that “silos are scarce.”[86]

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[84] H.B. Gurler, *The Farm Dairy* (Chicago: The Breeder’s Gazette, 1908), 68-80.

[85] Ibid.

[86] Ibid.

Clearly, then, the methodology that scientists employed allowed them to tailor their research to suit different audiences. Analyzing readily available, locally grown feeds satisfied both dairy farmers desirous to purchase the most inexpensive fodder and the grain farmers who produced these crops. Moreover, by employing a common methodology, scientists – even those in the most humble and ill-funded experiment stations – could make valuable contributions that would be recognized by their peers.

But scientists seemed to desire more than merely convincing dairy farmers to mix their rations in certain ways. In addition to publishing the results of their trials with various foodstuffs, many state colleges and experiment stations issued bulletins that attempted to explain the science foundation that underlay their work. To this end at least nine published lengthy bulletins between 1888 and 1900 that detailed exactly how nutrition scientists analyzed feeds and used these results to compound rations.

These bulletins explained, in greater or lesser detail, how to feed "rationally." Almost all began by outlining the composition of feeds; they explained that foodstuffs were made up of proteins, carbohydrates, fats, minerals, and water, and explained how animals employed each of these constituents for different physiological purposes. The authors usually then described how scientists determined the digestibility of fodder, and used this information to determine the nutritive-ratio of the various feeds. All of the bulletins explicated how farmers could use feeding tables that listed the nutritive-ratios of feeds to compound rations that fit any desired feeding standard. None of the authors intended these bulletins to replace text-books; instead, they aimed to describe scientific feeding and practices "In such a way that they may be understood and practiced by the farmers of this State who have neither the time nor necessary opportunities for a more extended study of the subject."[87]

These publications not only described *how* farmers should feed, but also as propaganda describing *why* it would be in the farmers' best interests to employ the methods outlined in these publications. Like the feeding manuals of Armsby and Henry described above, the authors of these bulletins often employed linguistic ploys presumably designed to assuage the fears of farmers. Several writers began by claiming that the bulletins they produced emerged not because scientists felt the need to describe how they went about their work but in response to the cries of farmers who asked for more information about scientific feeding. Thus, J.L. Hills of the Vermont agricultural experiment station noted that "No general statement as to the laws of nutrition and the results of experience has been made in the publications of this station since 1887. There is so much call for information that it has been thought wise to attempt in some measure to meet the demand by a popular presentation of present knowledge." Carl August Wulff of the Indiana station echoed these sentiments: "The many inquiries that have recently been sent to the station on this subject, have shown that a bulletin touching the question would be of great value to the farmers of this state," as did F.W. Woll of the Wisconsin

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[87] "Stock Feeding," *Arkansas Agricultural Experiment Station Bulletin No. 30* (1894): 137. See also "The Science and Practice of Stock Feeding," *New Hampshire Experiment Station Bulletin No. 4* (1888); "Principles and Practice of Stock Feeding," *Vermont Experiment Station Bulletin No. 81* (1900); "Feeding Dairy Cows," *Michigan State Agricultural Experiment Station Bulletin No. 149* (1897); "Rational Feeding," *Indiana Agricultural Experiment Station Bulletin No. 21* (1889); "Rations for Dairy Cows," *University of Wisconsin Agricultural Experiment Station Bulletin No. 33* (1892); "Stock Feeding," *Delaware College Agricultural Experiment Station Bulletin No. 7* (1889); "Stock Feeding Suggestions," *Maine Agricultural Experiment Station Bulletin No. 39* (1897).

station: “We are constantly receiving letters from farmers asking for advice in regard to the proper kinds of feeds for milch cows and how to combine them so as to obtain first-class results.”[88]

Some authors took pains to assure farmers that scientists did not wish to usurp the authority of the dairy farmer but, instead, that rational stock feeding amounted to collaboration between farmers and scientists. Writers, seemingly worried that farmers would automatically reject “book learning,” took pains to demonstrate that science should complement, rather than replace, experience; researchers from New Hampshire, for example, noted “Science can never take the place of practical knowledge, but it can point out the methods which lead to success. True science and *good* practice never conflict; if theory and practice lead to opposite conclusions, either the science or the practice is wrong.” Though the bulk of the bulletin suggests that the author doubted how many farmers actually employed “good” practices, the use of such language indicates the author’s awareness that not all readers were pre-dispossessed to follow the suggestions of experts from the research station.[89]

Writers employed a number of ploys to convince farmers of the scientists’ good-will, but most commonly they simply appealed to the farmer’s pocketbook. The authors of these works stressed that adopting rational feeding methods would increase the farmer’s income and/or decrease his costs. According to the author of the New

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[88] J.L. Hills: “Principles and Practice of Stock Feeding, *Vermont Agricultural Experiment Station Bulletin No. 81* (1900): 5. Carl August Wulff: “Rational Feeding,” *Indiana Agricultural Experiment Station Bulletin No. 21* (1889): 3. F.W. Woll: “Rations for Dairy Cows,” *Wisconsin Agricultural Experiment Station Bulletin No. 33* (189): 20.

[89] “The Science and Practice of Stock Feeding,” *New Hampshire Agricultural Experiment Station Bulletin No. 4* (1888): 4-5, italics in original.

Hampshire bulletin, "Here is where a study of the science of stock feeding may aid us in the practical work. I have very little doubt but that better results might be obtained, at less cost for food, if the rations fed were better proportioned. A saving of even five per cent would amount to $323,175 in the aggregate [for the farmers of the state], and I believe much more than this may be saved." Bulletins from Vermont, Purdue, Maine, and Arkansas and others all demonstrated how, by using feeding standards according to the methods outlined in the bulletin, farmers could increase their profits not only by producing more milk but by substituting foodstuffs that would produce equal or even superior results at a lower cost.[90]

Though scientists almost universally agreed about the mechanics of feeding – about the important constituents of feed, how to analyze the digestible quantities of each in various foodstuffs, and how to calculate the nutritive ratio of fodders and use this information to build a balanced ration – they failed to adopt a universal feeding standard. Several schools of thought emerged about how best to feed cattle. The most basic contention among scientists involved the "ideal" ratio of protein to carbohydrates and fats in a ration; in other words, researchers argued about which nutritive-ratio was "best" for the maintenance of animals and the production of milk and cheese. Some contended that feeding a ration with a "narrow" ratio that contained relatively large amount protein relative to the other components of the ration would maximize profits; others maintained that dairy cattle could not assimilate the amount of protein provided in such rations, and therefore the excess protein – usually the most expensive component of rations – was

[90] Ibid., 4; see articles listed in footnote 20: virtually all include a cost-benefit analysis of the advantages of scientific or rational feeding.

wasted. Instead, they advocated that farmers employ a ration with a "wider" ratio that contained relatively less protein and more – and less expensive – carbohydrates and fats.

Other debates emerged when some scientists suggested that rations should be tailored to individual animals. While most researchers agreed early on that animals should be fed by weight – that bigger animals should receive more feed – they held divergent opinions on whether rations should take into account the production of the individual cow. One group of scientists advocated feeding standards that factored in the amount of milk produced by the animal; while another felt this step unnecessary complicated the computation of rations. A third group believed that feeding standards should be based not only on the quantity of milk produced but on its quality – i.e. the amount of butterfat contained in the milk: animals that produced richer milk required more feed. Finally, some nutrition experts – including, eventually, Henry Prentiss Armsby who, though the author of one of the first and most influential texts championing the value of feeding standards and the use of the nutritive ratio – rejected the notion that proteins, fats, and carbohydrates formed the basis of animal nutrition and suggested alternate methods based on entirely different principles.

The proliferation of feeding standards is exemplified in Larson and Putney's 1917 treatise *Dairy Cattle Feeding and Management*. In the chapter dedicated to "Feeding Standards" the authors listed nine distinct feeding standards. The use of feeding standards dates to roughly 1860 when the German chemist Grouven posited that cattle should be fed on a scientific basis based on the nutrients contained in their fodder rather than by the weight of feed. Emil von Wolff agreed that carbohydrates, fats, and proteins formed the foundation of nutrition, but argued that rations should be constructed based on the digestible, rather than the gross, amount of nutrients in the foodstuff. Wolff's feeding standard became the first widely used rational feeding plan, and feeding standards of many later researchers amounted to modifications of Wolff's work. The German chemist C. Lehmann, for example, based his widely used feeding standard on Wolff's work, but unlike Wolff he factored the amount of milk produced by a cow into his feeding scheme.[91]

T.L. Haecker of the Minnesota experiment station took Lehmann's plan one step further by introducing a feeding plan that considered not only the milk production of the animal but also the fat-content of the milk she produced. Savage, who worked at New York's Cornell station, agreed with Haecker's contention that both milk and fat production should be factored into feeding, but believed the nutritive-ratio that Haecker recommended was too "wide" – that it did not contain enough carbohydrates and fats in relation to the amount of protein in the fodder. Savage offered a similar plan that increased the amount of protein in the feed. Woll and Humphrey of the University of Wisconsin's station, on the other hand, offered yet another system based on Wolff's feeding standard. They argued that feeding should be based on the weight of the animal and the amount of milk-fat she produced but did not believe the gross amount of milk produced by an individual animal needed to be factored into the feeding calculations.[92]

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[91] Carl W. Larson and Fred S. Putney, *Dairy Cattle Feeding and Management* (New York: John Wiley and Sons, 1917), 35-51. For further information on the plans of Wolff and Lehmann see also "A Study of Feeding Standards for Milk Production," *New York (Cornell) Experiment Station Bulletin No. 323*.

[92] For a useful overview, see Larson and Pitney. For Haecker, see "Feeding Dairy Cows," *Minnesota Experiment Station Bulletin No. 130*. For Savage, see "A Study of Feeding Standards for Milk Production," *New York (Cornell) Experiment Station Bulletin*

W.A. Henry, Dean of the College of Agriculture and Director of the Experiment Station at Wisconsin, published the first edition of his *Feeds and Feeding* in 1898. The volume would remain in print for decades and see numerous revisions. The feeding plan he recommended was based on the Wolff-Lehmann plan, which used the weight of the animal and the amount of milk she produced to compute the desired ration. However, Henry tinkered with the Wolff-Lehmann standard, and, according to Larson and Putney, Henry's scheme "modified the Wolff-Lehman standard by adopting certain parts of Haecker's, Savage's, Kellner's, Armsby's, Pott's, Bull's, and Emmet's standards. By use of these different standards Henry and [his assistant and later co-author] Morrison had made an approximation which they believe to be more nearly accurate than any other standards published up to this time." Though Henry believed his feeding standard to be the best available, his rather catholic employment of bits and pieces of different standards reveals the widely divergent beliefs of dairy scientists.[93]

Adding to the confusion, a handful of scientists questioned whether analyzing foodstuffs and building rations based on the amount of digestible proteins, carbohydrates, and fats they contained represented the best – or even the correct – way to study animal nutrition. Stephen Babcock, for example, expressed doubts about the usefulness of feed analysis along orthodox lines. While still a chemist at New York's Geneva experiment station (he left Geneva to join the faculty of the University of Wisconsin in 1888) the

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No. 323. For Woll and Humphrey, see *University of Wisconsin Experiment Station Research Bulletin No. 13*.

[93] Larson and Putney, p. 40. See also W.A. Henry, *Feeds and Feeding, First Edition* (Madison, WI: published by the author, 1898). As far as I can determine Henry did not explicitly list all of the influences claimed by Larson and Putney in any of the editions of his work, but this does not diminish the usefulness of their observation.

supervisor of the station, E.L. Sturtevant, assigned Babcock the task of analyzing various fodders. In the words of Babcock's biographer, "He found that if left out of consideration the mineral ingredients in the feed the chemical analysis of the intake corresponded closely with that of the wastes…Taking the two analyses to Sturtevant…Babcock asked the director which would make the better ration. Sturtevant could see no essential difference…To make matters worse, Babcock then suggested that he could make up from soft coal and similar materials a feed that would show the same chemical composition." Later, at the University of Wisconsin, Babcock had the opportunity to perform some trials with animals from the school's dairy herd. Babcock fed the test animals rations comprised of only one element – corn, for example; the animals soon deteriorated, and one died, ending the experiment. Babcock hypothesized that simple chemical analysis of feeds did not account for all variables, and, decades later, the scientists working on the substances that eventually became known as vitamins cited Babcock as a predecessor.[94]

The most strenuous objections to the practice of basing animal nutrition on the chemical analyses of foodstuffs came from Henry Prentiss Armsby. Though Armsby had championed the methodology of nutritional science employed by most of his peers, by the closing years of the nineteenth century Armsby began to express serious doubts about the wisdom of studying feeds. As the next chapter will detail, Armsby eventually decided that nutrition scientists would make little progress until they understood how

[94] Harry L. Russell, *Stephen Moulton Babcock: Man of Science* (Madison, WI: Wisconsin Alumni Research Foundation, 1943), 5-6.

animals made use of the food. In essence, Armsby insisted that *animals*, rather than *rations*, must form the basis of animal nutrition.

Despite the objections of Babcock, Armsby, and a handful of others, most researchers seemed quite content with the status quo. As outlined above, there were a number of reasons for them to practice science along recognized lines: not only did the analysis of foodstuffs fit the model advocated by proponents of agricultural colleges, but the system allowed experts at even the most modestly equipped and funded stations to make valuable contributions. Moreover, "rational" feeding offered financial benefits not only to dairy farmers who could confidently employ locally grown – and presumably, less expensive, if only because they did not have to be transported – foodstuffs but also to the farmers that produced these crops.

But the very versatility of the method – and its widespread acceptance – was a double-edged sword. Just as it allowed researchers to rather easily take part in the process of science, so too it encouraged many to draw their own conclusions and offer their own interpretations and theories. The proliferation of bulletins and texts that advocated scientific feeding attests to the widespread acceptance of the method; so too, in a different sense, do the wide array of feeding plans researchers concocted based on their findings.

Most scientists agreed on both the methodology and the goals of nutritional research. Doubtless the advocates of the multitudinous feeding plans believed their own scheme offered the most promise for dairy farmers, but the example of animal nutrition serves as a potent reminder of mixed-blessings of science. Though science served – and continues to serve – as a useful roadmap, its usefulness was never fully realized because scientists could not agree on a common destination.

CHAPTER 4

HENRY PRENTISS ARMSBY AND A NEW APPROACH TO FEEDING

As discussed in the previous chapter, a handful of scientists questioned the methods employed by most of their colleagues in the study of animal nutrition. Certainly they shared the same goal - an understanding of both the physiology of animals and the chemical makeup of the rations they consumed. Both groups of scientists hoped that their research would allow farmers to produce more milk and realize higher profits while at the same time providing healthy, high-quality to consumers at a lower price. The most strident objections to current trends came from Henry Prentiss Armsby, who, as described in the last chapter, played a central role in popularizing the methods he eventually came to attack. An examination of this process demonstrates not only the methodology employed by a scientist who no longer accepted the status quo, but exemplifies the limits of that methodology.

A year before his death in 1921, Henry Prentiss Armsby, among the most prolific and most influential animal nutritionists of his generation, published an article in the *Yale Review*. In it he combined the accrued knowledge of his almost five decades as an agricultural scientist with his recent experience as a member of the Inter-Allied Scientific Food Commission, a government body charged with surveying “the nutritional status of war-torn peoples" in Europe.[95] He wrote: "The experiences of the great war have forced us to realize as never before that the maintenance of the food supply is the basal problem of civilization…A starving world cannot be made safe for democracy." In Armsby's mind, the "maintenance of an abundant food supply" formed the basis of a "rational programme of national…preparedness."[96]

Noting that both the press and the Congress had deemed this "not only an urgent but an extremely complex problem," Armsby reminded his readers that though "its economic and political aspects are those which chiefly occupy the public mind…it must not be forgotten that nutrition is fundamentally a physiological question." A lifelong champion of agricultural science, Armsby suggested that nutrition scientists might play a vital, even pivotal, role in meeting these new challenges: "Much attention has been given by scientists…to the physiology of nutrition, and notable progress has been made in the discovery and application of its basal laws." Armsby innocently concluded the introduction to his paper by recognizing that "In these investigations, studies in the seemingly remote field of animal calorimetry have played a most significant part."[97]

In making this assertion Armsby acted somewhat disingenuously. Though he mentioned himself only obliquely, including "the writer and his associates" among a roster of nutritional scientists engaged in agricultural research, in truth Armsby had been a recognized leader in calorimetric research – which involved calculating how animals

[95] Robert Cowan: "Henry Prentiss Armsby, 1853-1921: A Brief Biography," Journal of Animal Science, 1988, p. 1841.
[96] Henry Prentiss Armsby: "The Modern Science of Food Values," Yale Review, 1920, p. 330.

[97] Ibid.

employed the food they consumed, usually by isolating the test subject in some sort of apparatus and measuring the amount of heat they produced - for almost twenty years. Indeed, he had overseen the construction and operation of one of the two calorimeters in the entire United States, and his apparatus was the only one designed and used primarily for animal research. What seemed an innocent acknowledgement of the work done by his scientific colleagues was, in fact, a tacit claim that Armsby – or at the very least, his methods – could help solve the most important issue of the time.

In the rest of his article Armsby displayed a zealot's faith in the possibilities of calorimetric research. By determining exactly how different animals assimilated and employed various foodstuffs, such investigations could ameliorate, if not eliminate, the food shortage that Armsby perceived as a potential threat to world peace. Recognizing that animals could utilize "waste" products – corn stalks, peanut hulls, wheat straw, etc. – that humans could not digest, Armsby hoped to use his analytic techniques to maximize the production of food fit for humans: people would directly consume those vegetable products they could efficiently digest; the rest would be fed to animals who could, via the production of meat, milk and eggs, render it fit for human consumption.

Though the construction and employment of the respiration-calorimeter Armsby oversaw while a member of the Pennsylvania agricultural experiment station played a central role in his research, more important were his reasons for constructing the apparatus. Despite his professional and popular success – by the 1890's Armsby had not only achieved a prominent place among agricultural scientists but sold thousands of copies of his feeding manual – Armsby, already near the pinnacle of his profession, decided to adopt a decidedly unorthodox research path. Though widely respected for his work in animal nutrition, Armsby abandoned the orthodox methodology that he had, in part, popularized. Though virtually all his peers continued – as had Armsby for two decades – to study nutrition from the side of the ration, Armsby chose to turn his attention from foodstuffs to the animals that consumed them. This decision ultimately led to his decision to construct the calorimeter and shaped the research he undertook for the rest of his professional life.

However, in doing so he maintained the same scientific methodology that he – and his contemporaries – believed held the key understanding the physical world. Namely, Armsby believed that measurement and quantification would, if performed scrupulously, allow scientists to discover underlying truths that would, in turn, permit researchers to not only understand their world but, as importantly, determine the formulas that predicted and governed behavior. Though he decided to abandon one path – the study of nutrients – for another – the analysis of how animals actually employed those materials – he never lost his faith in the methodology he employed.

Armsby's career serves as perhaps the ultimate example of a scientist unwilling, or, more likely, unable to comprehend that quantitative analysis might not be able to provide answers to all possible questions. This assertion should not be construed as a slight to Armsby's intellect or precision; he meticulously designed his experiments, considered all conceivable sources of error, and, as demonstrated in his published research bulletins, never shied away from reporting outcomes that varied from those predicted or anticipated. Nor should this suggest that Armsby blindly clung to a specific scientific goal; he could – and did – change directions in mid-career. Armsby was no blind fanatic but an intelligent, curious scientist who possessed an uncommon, and enviable, ability to shift focus when one avenue of investigation proved unviable.

Even in his decades-long explorations of calorimetric analysis Armsby maintained his high standards. He possessed a prodigious and exacting talent for laboratory experimentation, and demanded such precision from his colleagues. He did not write off unexpected results but instead devised new techniques and controls that might help to explain anomalies. As time went on, his procedures became increasingly elaborate as he attempted to account for every possible variable.

Ultimately, Armsby's findings proved inconclusive. Despite generating reams of data, his findings raised more questions than they answered. That Armsby maintained the correctness of his approach until his death despite these failures indicates his faith in the techniques he employed. Unfortunately, he failed to consider that discoveries made by some of his younger colleagues – results of which Armsby was well aware - would result in the adoption of a new methodology and the radical modification of, if not the abandonment, of the techniques in which Armsby placed so much confidence.

Armsby's education established him in squarely in the mainstream of American scientists who came of age in the decades following the Civil War. He studied at the Sheffield Scientific School at Yale and earned his B.Phil. in 1874. Like many of his peers, Armsby studied in Germany and spent a year in Leipzig where he became acquainted with German agricultural scientific methods, and returned to New Haven to serve as chemist at the Connecticut Agricultural Experiment Station. While serving in that capacity he completed his Ph.D. at Yale in 1879.

His experience in Germany proved especially prescient. Though many of his contemporaries also studied in Europe, Armsby's writings throughout his career indicate that he, more so than the bulk of his colleagues working in Agricultural Experiment Stations, kept a close eye on developments in Europe. The fact that he translated and published at least five articles in German scientific publications between 1878 and 1905 suggests that he did not consider science a nationalistic enterprise.[98] Armsby first came to national prominence in 1880 with the publication of his *Manual of Cattle-Feeding*, which in the author's words "was begun as a translation of Wolff's '*Landwirthschaftliche Fütterüngslehre*'" but, because of Armsby's lengthy considerations of American advances, became a substantively different document.[99]

The *Manual of Cattle-Feeding* established Armsby in the forefront of American agricultural nutritionists. At the time, the *Manual*'s importance stemmed from the fact that it popularized the notion that the correct formulation of animal rations lay in combining foodstuffs that balanced proteins and non-proteins in definite proportions. Indeed, the proliferation of works claiming to have precisely calculated the optimum nutritive-ratio, as Armsby called it, suggests that it became something of an Eldorado for animal scientists. More significantly for this work, the *Manual* served as an exposition of Armsby's approach to science.

Though he never explicitly outlined his scientific methodology, Armsby's *Manual* clearly demonstrated his modus operandi. Armsby's scientific strategy consisted of four

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[98] Francis G. Benedict: Biographical Memoir of Henry Prentiss Armsby, National Academy of Sciences, 1938, pp. 280-284.

[99] Henry P. Armsby: Manual of Cattle-Feeding, John Wiley and Sons, New York, 1882, p v.

elements: identification, quantification, classification, and formulation: one identified a scientific (often, though not necessarily, a chemical) element, one developed a method of measuring that element, one compared the quantification of that element to others, and, finally, one developed a formula that would combine two or more elements that would explain or predict a desired outcome. His "Rules for the Calculation of Rations" exemplifies this approach:

> "1. The composition of the fodders used is either ascertained by analysis or estimated form the table of the composition of feeding-stuffs…6. By multiplying the percentage of each ingredient of the fodders by its digestion coefficient, the percentage of digestible matters in each feeding-stuff is obtained…7. From the data thus obtained we calculate, first, the quantities of digestible protein, carbhydrates [sic], and fat in the amounts of fodder available, and second, what additional of bye-fodder must be made to the ration up to the feeding standard…9. If it is desired to test the correspondence of the calculated amount of digestible protein with that really present, the latter may also be calculated by Stohmann's formula…"[100]

For more than a decade after the publication of the *Manual* Armsby conducted nutritional research along the orthodox lines that he, perhaps more than any American agricultural scientist, had helped establish. He investigated the digestibility of different varieties of bran, performed experiments with maize and the feeding of ensilage, and continued his analyses of feeding standards and nutritive ratios. In addition, like most of his colleagues working at State Agricultural Experiment Stations, he undertook a number of studies not directly related to his specialty: he wrote a critique of the Bunsen lamp, evaluated the Cooley system of creaming milk, and explored how the employment of particular manures might increase the production of specific crops. These investigations

[100] Ibid. pp. 473-475.

reveal that Armsby, whether by choice or duty, did not limit his purview to a limited field; in the scope of his professional duties Armsby proved to be as catholic as his colleague at Wisconsin, Stephen Moulton Babcock. More importantly for this consideration, however, these studies show that Armsby consistently employed the same approach regardless of the target of his experimentation. In each case, Armsby applied the quantitative methods outlined above, and the results suggest that this method proved perfectly satisfactory in providing answers to his scientific inquiries.[101]

By the 1890's Armsby changed tack; he began to question the usefulness of the chemical analysis of feeding stuffs and undertook a new path of investigation. Doing so marked a shift of scientific emphasis that would shape the course of the rest of his career. Armsby believed that scientists – including himself – had paid too much attention to animal rations and not enough to the animals themselves. Furthermore, most studies that examined how cattle utilized their fodder looked at how the animals employed food for production of milk or meat at the expense of learning how much food animals required for simple maintenance of their body. In the 1898 bulletin that reported the results of his initial foray into such considerations, he wrote: "A certain degree of justification for this practice [the study of fodder's effect of production] exists, it is true." However, "it is plainly inadmissible to attempt to establish general laws by comparison of the food with one of its effects, viz., production, while ignoring entirety its other effect, viz., maintenance."[102]

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[101] For a bibliography of Armsby's work, see Francis G. Benedict: Biographical Memoir of Henry Prentiss Armsby published in the National Academy of Sciences Biographical Memoirs, Vol. XIX, 1938, pp. 280-284

Armsby did not claim to be the first to consider the importance of this approach; he prefaced the account of his research with a survey of the work done by others. Most importantly, he discussed the results obtained by the German scientist G. Kühn and his associates at the Moeckern Experiment Station. They employed a respiration device – a form of calorimeter – that enabled them to measure the energy generated by the bovine subjects of their research. A comparison of the energy put into the system – i.e. the ration fed the animal – and the energy output – measured in terms of heat – allowed the determination of net energy expenditure. Armsby believed that this approach "constitutes by far the most complete investigations upon this subject yet reported."[103]

Unfortunately, as Armsby noted, "The Station does not possess a respiration apparatus, and hence has not the means for scientifically accurate determinations of the gain or loss of fat by the animals under experiment."[104] Undaunted, Armsby devised and carried out a series of experiments that attempted to emulate Kühn's research without the need for a respiration apparatus, and performed a series of six lengthy trials between 1892 and 1897. In these trials, Armsby carefully recorded the weight of the animals tested as well as weighing and performing chemical analyses of the food consumed and the urine and feces produced. These experiments demonstrated that individual animals utilized their fodder in different ways. When fed the same ration, some animals lost weight, some gained weight, and some maintained a consistent weight; likewise, some

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[102] Henry Prentiss Armsby: The Maintenance Ration of Cattle," Pennsylvania State College Agricultural Experiment Station Bulletin No. 42, State College, PA, May 1898, p. 7.
[103] Ibid., 21-25.

[104] Ibid., 25.

cattle absorbed large amounts of nitrogen and/or proteids, while others did not, and these variations did not appear to necessarily correspond with weight gains or loss. In short, Armsby collected vast amounts of quantitative data which did not appear conducive to the establishment of firm, scientific formulas.[105]

Armsby acknowledged these shortcomings: "It is apparent from the figures…that there was considerable apparent fluctuation in the digestibility of the ration." He attempted to account for these discrepancies by a careful examination for possible errors. To this end, he considered miscalculations in weighing, in the determination of the air-dry matter of feces, in the air-dry matter of hay, in the determination of nitrogen, and of the co-efficients used in making all these measurements. Though Armsby admitted that "these computations are by no means of rigid mathematical accuracy," he hoped that they might provide "a fairly approximate idea of the probable limits of experimental error." Armsby recognized that errors existed, but believed they did not seriously compromise his findings. It is perhaps telling that Armsby did not include a conclusion that neatly summarized his findings; instead, he included several dozen pages of complex tables that reported his experimental results without making any real claims about their importance.[106] Perhaps aware that the inconclusiveness of these experiments did not facilitate the adoption of the new research goals that he recommended, the usually

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[105] See Henry Prentiss Armsby: "The Aim and Methods of Investigations in Stock Feeding," *Agricultural Science* 7 (1893): 481; "The Computation of Rations for Farm Animals," *Report of the Pennsylvania Experiment Station* (1896): 18; "The Maintenance Ration of Cattle," *Pennsylvania Experiment Station Bulletin No. 42* (1898); "Rational Stock Feeding," *Report of the Pennsylvania Experiment Station* (1894): 36.

[106] Ibid., 135-140.

prolific Armsby published very little over the next five years: four articles, three of them co-written, that reported the results of experiments on sugar beets and timothy hay.

In 1903 Armsby introduced the first edition of *The Principles of Animal Nutrition*. In his preface Armsby made it clear that he understood that his early *Manual* was out of date: "The past two decades have…witnessed great activity in the study of the various problems of animal nutrition." Armsby signaled that animal science had itself changed; not only had the "various problems" been examined, but they "they are especially distinguished by the new point of view from which these problems have come to be regarded."[107] Armsby referred to this new point of view as "The Statistics of Nutrition," which itself consisted of "two distinct although closely related parts, viz.: 1. The income and expenditure of matter. 2. The income and expenditure of energy."[108]

The introduction to *Principles* serves as an exposition of Armsby's beliefs about both his conception of animals and the role they play in the natural world, but also his scientific approach to understanding these phenomena. He began "The body of an animal…consists of an aggregate of a great variety of substance," and of "so-called 'organic' compounds." The "manifestation of life" consists literally of "the conversion of complex into simpler compounds," a process that releases the energy "which is the essential end and object of the whole process and which, if not synonymous with life itself, is the objective manifestation of life." These organic compounds eventually come from the sun, and therefore "we may look upon the animal as a mechanism for

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[107] Henry Prentiss Armsby, *The Principles of Animal Nutrition* (New York: John Wiley and Sons, 1903), iii.

[108] Ibid., 3.

transforming the stored up energy of the sun's rays, contained in its tissues, into the active or 'kinetic' forms and heat and motion."[109]

Armsby believed that "The animal body…consists of a certain amount of matter…which represents a certain store or capital of potential energy." In addition, the ratio of energy and matter "is in a constant state of flux." Discovering the rules that governed this state of flux became the consuming goal of Armsby's scientific efforts for the rest of his career; as noted in the address which opened this piece, he believed that such knowledge could eliminate hunger. For example, if a person wanted to maintain a certain weight, he or she would simply balance matter consumed with energy produced; on a national scale, governments could use these findings to set national production goals. Likewise, a farmer who wished to fatten cattle for market would feed food in excess of that which the animal required for maintenance; in this case, the animal would convert the excess energy into body tissue that could eventually feed humans. These considerations became increasingly important to Armsby when he turned his attentions from farm animals to the world food supply.[110]

Moreover, Armsby made it clear that these were mechanical processes and, as such, could be quantified and formulized – the scientific approach that he followed throughout his life. The term he employed – "The Statistics of Nutrition" – clearly indicated that he ultimately viewed the world in terms of discrete elements that could (eventually) be quantified, classified, and measured; Armsby assigned himself the task of helping to discover exactly how to precisely measure these elements. Though historically

[109] Ibid.

[110] Ibid.

important because it provides a clear picture of Armsby's methods and approach, the introduction would not have surprised contemporary readers; Armsby's scientific approach practices mirrored that of his colleagues. Armsby's assertions in the introduction to his work were typical rather than exceptional, and are primarily useful because they neatly summarize contemporary scientific orthodoxy.

However, in the second part of his book, Armsby made two truly important, and perhaps even audacious, announcements. He asserted that "Since kinetic energy in the animal is derived from chemical processes…we may regard the study of the transformations of energy in the organism as constituting a branch of thermo-chemistry." As such, animals were subject to the scientific laws that governed other processes. Specifically, Armsby believed that he could apply the law of the conservation of energy to his study animals. Simply put, the law of the conservation of energy states that energy, like matter, is indestructible, though it can, and often does, change forms. Armsby had convinced himself that the application of this law to animals would eventually allow the total understanding of animal physiology and nutrition: "The truth of this law, as applied to chemical processes, has been fully demonstrated…That the same law applies to the processes taking place in the body of the animal is exceedingly probable, *a priori*, and has been demonstrated experimentally."[111] That Armsby pegged his whole approach, and indeed, his whole career, not to a surety, but merely to the "exceedingly probable" clearly demonstrates Armsby's faith in the ability of science to provide answers, and his willingness to accept such promises on faith. Armsby used this opportunity to announce that he possessed the means to make investigations in this new vein; he revealed – for the

[111] Ibid., 228-229.

first time in a work aimed at a popular rather than scientific audience – that he had overseen the construction of a state-of-the-art respiration-calorimeter at the Pennsylvania Experiment Station, where he had served as director since 1887.

A calorimeter functions by measuring the heat produced by a chemical reaction; their use to study the heat generated animals dates back to at least the late eighteenth century and the efforts of the noted French chemist Lavoisier, who employed an ice-calorimeter, whereby the heat generated could be measured by the amount of water melted. Unfortunately such devices, while useful for measuring simple chemical reactions, proved less than ideal for testing living subjects. Water-calorimeters proved problematic because of the difficulty of maintaining a uniform water temperature; in essence, the water near the surface acted as an insulator for the "deeper" water, making reliable readings difficult. Air calorimeters, which measured heat by noting the pressure change in a system of fixed volume, or the volume change in a system with fixed pressure, enjoyed a vogue in the second half of the 19th century.

Armsby's decision to employ a respiration meter – which measured the amount of carbon dioxide exhaled by its (animal) subjects as well as the head produced and energy expended by its test subject - precluded the use of an air-calorimeter. Instead, he employed a new system of water-calorimetry that measured temperature changes in flowing rather than stationary water. This solved the problems noted above, and proved very accurate. The major advantage of this system – besides its high level of accuracy – lay in the fact that, given the resources, one could build a calorimeter of any size. Armsby's calorimeter – the largest in the world – was large enough for cattle investigations, and Armsby noted that "still larger ones are in the process of construction," though, it should be noted, it is unclear whether these devices were ever actually constructed.[112]

Armsby modeled his apparatus after that constructed by W.O. Atwater and E.B. Rosa at Wesleyan University in Middletown, Connecticut. They employed their respiration-calorimeter to study human subjects, and published a series of reports detailing their finding beginning in 1897. In most respects, save size, Armsby's device resembled that at Wesleyan.[113]

Armsby's calorimeter was a massive instrument, measuring some six feet wide, ten feet long, and eight feet tall; as an additional layer of insulation, it sat on supports two feet above the ground. The sides, floor, and ceiling contained three separate walls, each of which consisted of heavy wood lined with copper, while a two inch air-space between each of these plies provided insulation. Three separate doors insured the integrity of the experimental space created within the apparatus. A pair of aspirators supplied a constant supply of fresh air and trapped all out-going gases; analysis of the respired air could determine how much oxygen had been converted into carbon dioxide, methane, and other gases – an important specific of Armsby's research plan. Likewise, a grid of pipes filled with a constant flow of water at fixed temperature surrounded the inner chamber of the device and allowed researchers to measure temperature changes. Armsby's apparatus proved incredibly sensitive; according to one source, "a window blind had to be

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[112] Ibid.

[113] For information on Atwater and Rosa's calorimeter, see Atwater and Rosa, USDA Office of Experiment Station Bulletin No. 63 and W.O. Atwater and F.G. Benedict, "Experiments on the Metabolism of Matter and Energy in the Human Body, *USDA Office of Experiment Stations Bulletin No.* 69 (1899).

employed to avoid the heat surge stimulated in the two male student subjects caused by the arrival of a co-ed assistant with the breakfast tray."[114]

Armsby first published the results of his experiments employing the new calorimeter in 1905. Noting that "Few questions recur more frequently than those concerning the relative values of different feeding stuffs," he claimed to have determined the "exact nutritive effect" of the tested fodders. Armsby began by noting that "even a comparatively short time ago" researchers would have gladly accepted a computation of the digestible nutrients of a foodstuff based on a chemical analysis of the excreta of animals fed exclusively on that ration. However, more recent research, including his own, found discrepancies with these figures, and Armsby claimed that "their erroneous character is clearly shown by the results about to be described."[115]

Armsby proceeded to explain his conception of nutrition. In Armsby's explication, orthodox understanding of nutrition suggested that food played two roles: it served as a source of tissue, and it furnished energy. Armsby hoped that the determination of the amount of potential energy contained in food – the "fuel value" of a ration – might function as a "more accurate measure of its relative feeding value than…its 'digestible nutrients'" Ultimately, Armsby's research with his respiration-

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[114] Quote from Robert L. Cowan, "Henry Prentiss Armsby, 1853-1921: A Brief Biography," *Journal of Animal Science* 66 (1988): 1839. For information on Armsby's calorimeter, see Armsby: "The Respiration Calorimeter," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 104* (1905), and "The Respiration Calorimeter at the Pennsylvania Experiment Station," *U.S. Experiment Station Record No. 15* (1904): 1037-1051.

[115] Henry Prentiss Armsby, "Relative values of Feeding Stuffs," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 71*, (1905): 4-5.

calorimeter convinced him animals could not take full advantage of the "fuel-value" of their fodder.[116]

He determined that animals – in his case, cattle – could only assimilate a certain percentage of the calculated fuel-value of a given foodstuff – approximately 60-80% depending on the fodder – for "maintenance" use such maintaining bone, muscle mass, and the various organs of the animal. The rest of the fuel-value was consumed in the "muscular exertion to grasp, chew…swallow…and move it [the food] through the alimentary canal," in the "quite extensive fermentations and putrefactions" of the digestive process, and in the "chemical change…necessary to convert the digested materials into forms suited to nourish the cells of the body." Armsby suggested that a new term, "production-value," be used to denote the amount of energy available for use after the ration had been assimilated.[117]

Armsby believed that his investigations - though "suggestive but not final" because they rested upon "one or two determinations only upon a single animal" - presented a serious challenge to traditional methods of compounding rations. According to these accepted practices, 174 pounds of hay had the nutritive equivalent of 100 pounds of corn meal; Armsby's research found that for maintenance, 211 pounds and for fattening, 273 pounds of hay "are required to equal 100 pounds of corn meal…differences too large to be accidental and too important to be ignored."[118]

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[116] Ibid.

[117] Ibid., 9-10.

[118] Ibid.,13.

Armsby concluded by suggesting that the only "safe basis for a comparison of the values of feeding stuffs is the actual experiment upon the animal." As mentioned above, this constituted a serious challenge to the orthodox aims of nutritional science, which, at least as practiced in the United States, examined animals only indirectly by comparing the chemical makeup of foodstuffs consumed with the animal waste produced. Armsby's insistence on the importance of studying the animal itself marked a different approach to animal nutrition. However, even more important was his commitment to his the quantitative method. Armsby never suggested that the scientific methods he and his colleagues employed might be wrong; instead, it was a matter of what they chose to examine.[119]

The following year Armsby read a paper before the Baton Rouge Convention of the Association of American Agricultural Colleges and Experiment Stations. Entitled "Problems of Animal Nutrition," the paper was a far-ranging indictment of agricultural science. Though the paper addressed a wide range of topics and merits attention as a whole, for present purposes only issues related to this paper will be discussed.

Armsby began with a blanket reprimand of the "experiment stations of the United States" for having "failed to recognize the importance" of the economic feeding of animals. He then quickly surveyed nutritional research published by the state experiment stations. He summarily dismissed over two-thirds of these publications: he noted that one could "accumulate a great deal of data" from these works, but thought it "very doubtful whether the results reached would be worth the labor." The bulk of the research

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[119] Ibid.

contained useful material, but those results could be deduced by "a good practical feeder."[120]

The problem lay in the fact that such researches, while demonstrating many useful facts, "served only to a very subordinate degree to reveal principles." He continued "As scientific men we should know…that permanent progress in agriculture is possible only through the establishment of principles. One principle well founded is worth a thousand facts…" Armsby condemned the practice of comparing food values as out-of-date, and noted that most European researchers had long since abandoned such investigations.[121]

Armsby insisted that American agricultural scientists needed to focus their attention on animals rather than animal fodder, specifically along the lines he had established with his use of the respiration-calorimeter. More importantly, they needed to discovery and establish "natural principles and laws" rather than compile facts. "By this path alone can we hope to attain a clear and definite *quantitative* conception of the process of nutrition." Nor did scientists need "formidable equipment" or the "heavy artillery" of the calorimeter: "The sling may still prove a formidable weapon, if wielded by the hand of a David, and the smooth pebble from the brook may still do its wonted execution. It is largely a method of aim"[122]

This paper neatly encapsulated Armsby's scientific worldview. Three elements stand out. First, Armsby continued his emphasis that scientists study animals; the analysis of fodder alone did measure its true value. Second, he insisted that the purpose

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[120] Ibid.

[121] Ibid.

[122] Ibid, pp. 516-517, italics mine.

of science was the discovery of principles. Note that that while he discouraged the mere *accumulation* of facts, he did not condemn them outright. Indeed, facts formed an integral part of Armsby's own scientific approach. However, this paper suggests that he considered facts subordinate to principles: one gathered facts to test a hypothesis; one did not gather facts for their own sake. Finally, Armsby once again suggested that the path he had championed proved most promising and vital.

Armsby's 1907 Pennsylvania Station Bulletin, "Feed as a Source of Energy" is noteworthy because it amends and clarifies his operational principles. First, he introduced a modified conception the uses of energy. Under Armsby's updated scheme, animals utilized their rations in the performance of three functions: the maintenance of their body and tissue, external work "such as pulling or carrying a load," and the production of human food. Second, he clarifies the three ways that animals eliminate the un-utilized energy of their fodder: in feces, in urine, and in the production of "marsh gas, or methane." Finally, Armsby modifies his earlier conclusions by noting that the full extent of the "production-value" of the feed – the energy available from the digestible portion of the feed after subtracting the energy required to consume and digest the food – was not simply converted into the "flesh and fat" desired by stockmen, but that, as with the digestion process, the animal consumed a certain amount of this "excess" energy in converting the surplus energy into body tissue.[123]

In 1911 Armsby, then president of the American Society of Animal Nutrition, used his presidential address at the Society's annual meeting to emphasize the importance

[123] Henry Prentiss Armsby, "Feed as a Source of Energy," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 84* (1907): 4-5.

of his work. After lauding the work done by standing committees to facilitate advances in nutrition, Armsby lamented the fact that despite the great amount of work that had been performed scientists still remained ignorant of many important factors. Perhaps not surprisingly, Armsby singled out his own scientific bailiwick – that of animal digestion and metabolism – as an area that deserved more consideration. He began by noting "The treatment of this subject in the text books on stock feeding is apt to produce the impression that it is a comparatively simple and well understood process." He attacked this view, noting that most of the experiments on digestion had been performed not on farm animals but on dogs and human subjects, and the "results…transferred rather uncritically to herbivorous animals."[124]

He continued by noting the growing acceptance of energy values as the basis of forming rations, but asserted that 'the basis for such a comparison is inadequate." Furthermore, researchers – including himself – "do not know the energy content of our feeds and especially do not know how much of the total energy…is capable of transformation in the body." Moreover, "of the innumerable problems in animal metabolism one scarcely dares begin to speak." While Armsby politely refrained from explicitly noting that he had been concerned with such questions for over a decade, he clearly used his address as an opportunity to suggest that his approach to research might prove more beneficial.[125]

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[124] Henry Prentiss Armsby, "The President's Annual Address: Some Unsolved Problems," *The Journal of Animal Science* (1912): 5-6.

[125] Ibid., 8-9.

Between 1911 and 1912 Armsby also issued three lengthy bulletins through the Bureau of Animal Industry of the USDA. Among them they outlined the various avenues of Armsby's research. The first to appear, "The Nutritive Value of the Nonprotein of Feeding Stuffs," found Armsby questioning the distinction drawn by most nutritionists between the proteins and non-proteins found in foodstuffs. Iterating his conviction that "the animal undoubtedly has the power to build up body proteins out of the comparatively simple cleavage products resulting from the digestion of food proteins," Armsby suggested that animals – or perhaps the microorganisms that inhabited the digestive track – had the ability to synthesize their rations into whatever the body demanded.[126]

After briefly surveying previous work on the subject, most of which had been performed on carnivores, Armsby marshaled his own findings. He concluded that bacteria within the ruminant's digestive system could convert nonproteins into proteins which the animal could utilize. However, he warned that while animals could employ this protein for the maintenance of their bodies and tissue, "the nonproteins are much inferior to the proteins in nutritive value for productive feeding." Again, Armsby referenced his belief that animals utilized their rations in two different ways: maintenance and production. Unfortunately, "if…the nonprotein is to be regarded as of full value for maintenance bus as practically valueless for production, an undesirable complication is introduced into the computation of rations." Armsby then cited some of his own research

[126] Henry Prentiss Armsby, "The Nutritive Value of he Nonprotein of Feeding Stuffs," *USDA Bureau of Animal Industry Bulletin No. 139* (1911): 5.

that suggested an alternative method of computing rations that took this factor into account.[127]

If true, Armsby maintained that this proposition would require a complete re-evaluation of animal nutrition; it would be "futile to seek to establish any definite ratio between protein and nonprotein as to their value to the organism, because both of them…are in this respect more or less variable and indefinite conceptions." Because "the failure to recognize this fact is responsible for not a little of the existing confusion of thought on" the problems of nutrition, Armsby hoped that his own research could shed light on an important subject.[128]

Armsby attacked the problems of animal nutrition from a different angle in his next publication, "The Influence of Type and of Age upon the Utilization of Feed by Cattle." Since it was "common knowledge that marked differences exist between individual animals as regards the returns which they yield for the feed consumed," Armsby hoped to shed some light on this subject. He began by rebuking the orthodox view of the subject: that some animals produced more milk or meat from a given ration because they were better able to "digest" their feed. Armsby questioned this point of view. Citing "numerous experiments" that "failed to show any marked difference in the…digestibility of same feeding stuffs by animals of different types," Armsby instead suggested that his colleagues draw a difference between "digestion" and "assimilation." Again, Armsby used the opportunity to emphasize his conviction that nutritionists erred when they studied rations without examining animals. "Digestibility," for Armsby, was a

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127 Ibid., 46-49.

128 Ibid., 6.

characteristic of foodstuffs; "assimilation," on the other hand, referred to the animals ability to make use of a particular ration. Armsby believed that his colleagues had spent far too much time studying the former, and far too little considering the latter.[129]

Armsby hoped to "test this question" of assimilation by studying "two animals of markedly dissimilar type and determining their digestive power for the same feeding stuffs…" while at the same time looking at "the portion of energy" that they employed "for maintenance or for productive purposes." Armsby selected a pair of mismatched steers: a "purebred Aberdeen-Angus of typical beef form," and a "scrub" (or mixed-breed) steer "containing considerable Jersey blood and possessing the dairy rather than the beef form." The animals were maintained under identical conditions and fed the same rations for over 30 months during which time they were, at intervals, placed inside Armsby's calorimeter in order to determine the amount of energy they produced.[130]

Armsby's findings challenged traditional understanding of how different animals utilized their feeds. He discovered, after extensive testing, that the two disparate animals metabolized virtually the same amount of food they ingested. The steers also produced approximately the same quantity of energy per unit of food eaten. However, there the similarities ended: the beef steer put on much more weight than the scrub, and put it on more quickly. Armsby posited a number of explanations for this phenomenon. He determined that the beef animal demanded less energy for the maintenance of its body tissue; the scrub steer required 18.7 percent more feed just to maintain its body.

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[129] Henry Prentiss Armsby and J. August Fries, "The Influence of Type and of Age upon the Utilization of Feed by Cattle," *USDA Bureau of Animal Industry Bul. No. 128* (1911): 11.

[130] Ibid., 12-13.

Moreover, the beef breed demonstrated the ability to consume more feed than the Jersey cross; because of this the beef could convert more of its energy into meat than its partner.[131]

Armsby believed that these findings called conventional wisdom into question. He found that, contrary to popular belief, that beef cattle did not digest their fodder more efficiently than non-beef breed. Instead, they required less to maintain their bodies and could thus employ more of the ration for adding meat. In doing so, Armsby offered further evidence for two of his main contentions: that the "digestibility" of various feeds remained almost constant between animals, and that determining how much food an animal required for maintenance would play a large part in better understanding how to best utilize foodstuffs.

Armsby brought these findings together in the third bulletin he issued through the Bureau of Animal Industry, 1912's "The Maintenance Ration of Farm Animals." He began by defining exactly what he meant by "maintenance." Though "sometimes used popularly…to signify the total amount of feed required, for example by a horse in order to perform his daily work," Armsby considered it "important to grasp the idea that…the maintenance requirement means the minimum required simply to sustain life." Just as he had when discussing the differences between "digestion" and "assimilation," Armsby once again took pains to carefully define his terms, an important consideration that confirmed his belief in, and reliance on, the methodology of science as he understood it. In order to quantify various characteristics one had to first carefully decide what one actually wanted to measure. Like his contemporaries, Armsby worked in a very

[131] Ibid. 16-17.

methodical, and predictable, manner. His patent disagreements with his contemporaries stemmed not from the way his colleagues went about their work, but what they chose to study.[132]

Despite the fact that "it might seem…that not much importance attaches to a study of the maintenance requirement," Armsby considered the precise determination of the maintenance requirement of vital significance, not only in order to recognize the difference between different animals and how they respond to different conditions and feeds, but, as importantly, "to understand the principles governing the production of meat, milk, or work."[133]

Armsby employed most of lengthy bulletin – over a hundred dense pages – to iterate his understanding of animal nutrition and how it should best be investigated. Central to his vision was his belief that in order to understand nutrition one should begin not by studying foods but by looking at the animals themselves. This required the consideration of a number of factors overlooked or ignored, in Armsby's opinion, by most researchers. Computing the basal metabolism – the "maintenance" requirement of each animal – only represented the first step of Armsby's approach. In order to truly understand the nutritional needs of an animal, one had to consider the animals age, the amount of work it performed, whether the animal primarily stood or, instead laid down. Equally important was finding the animal's body temperature and determining how it was affected by external temperature and the type and amount of feed consumed. Armsby

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[132] Henry Prentiss Armsby, "The Maintenance Rations of Farm Animals," *USDA Bureau of Animal Industry Bul. No. 143* (1912): 7.

[133] Ibid., 7-8.

considered it crucial to determine how, why, and under what conditions animals – or the micro-organisms they harbored – could synthesize protein from the nonprotein constituents of various rations. In short, Armsby maintained that the only "in the phenomena of maintenance" could "the fundamental processes of nutrition…be studied uncomplicated by the demands of growth, fattening, or reproduction."[134]

By 1913 Armsby had confirmed, to his own satisfaction, that the law of the conservation of energy did in fact, as he had suggested a decade earlier in the *Principles*, apply to animals. He used Pennsylvania State Experiment Station Bulletin No. 126 to trumpet his conclusions. In presenting his findings, Armsby employed an extended metaphor that likened animals to machines, and the comparisons Armsby draws are noteworthy:

> "The living animal constitutes what the engineer calls a prime motor, that is, it generates power for its own operation and is able to produce a surplus which may be used to do work of one sort or another. In particular there is a very close resemblance between the animal body and the various forms of internal combustion motors…Both are mechanisms designed to utilize the chemical energy stored up in fuel of one sort of another of the performance of work and the requirements of both are similar…When fuel is burned in the cylinder of the engine the energy which it contains is transformed, part appearing as work but usually considerably more taking the form of heat…Such an engine is spoken of as a transformer of energy because it changes one form of energy into another. The case of the animal body is precisely similar…The food, then, may be regarded in the light of fuel for the bodily mechanism, and may be studied from this point of view."[135]

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[134] Ibid., 8.

[135] Henry Prentiss Armsby: "Food as Body Fuel," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 126* (1913): 60-61.

However, Armsby balked before declaring that animals and machines operated by the same physical and chemical laws. The processes of the animal, unlike those of the machine, took place rather slowly and at relatively low temperatures. Moreover, animals are "*living*…and we are not justified in assuming…that energy is transformed…according to the same laws as in lifeless matter." Armsby then marshaled the results of various calorimetric analyses that confirmed his hypothesis: the results of experiments on a variety of mammals produced results very close – within a fraction of one percent – of those predicted according to the laws of physics. Armsby concluded that "These results may be taken as demonstrating that the conversion of the energy of the feeding stuff of our farm animals…is governed by the same general laws which apply...to the transformations of energy in lifeless matter."[136]

Ultimately, Armsby's confidence in his findings would blind him to the possibility that the traditional nutritional analysis he practiced – i.e. that foods consisted of measurable quantities of proteins, carbohydrates, and fats, which could be measured and their value as animal fodder ascertained – might not fully account for the utilization of foodstuffs by animals. He confidently asserted that "All that it [the animal] gives out it gets from its food and all that is supplied is sooner or later recovered in some form…we may be confident that any food energy that does not reappear in the form of heat or work has not been lost but has been stored up in the body." Unfortunately, Armsby failed to appreciate that contemporary investigations – that the evidence suggests

[136] Ibid., 61, 65.

he was well aware of – would eventually become so important as to render Armsby's contributions, vitally important in his own mind, largely irrelevant.[137]

In 1917 Armsby published *The Nutrition of Farm Animals*. "Intended for the student rather than directly for the farmer," the book served as a recapitulation of Armsby's life-work in nutrition. In form, *Nutrition* followed his other works: Armsby constructed his science from the ground up, so to speak. He began by describing the various elemental components of nature, built them into plants and animals, and then described how animals assimilated and converted animal matter into heat, work, tissue, and the various products desired by humans – eggs, milk, and meat.[138]

The book contained no scientific bombshells; it did not introduce – nor was it intended to – new theories. Instead, it served as a summary of Armsby's views on nutrition. As such, Armsby devoted large portions of the book to a careful explanation of how he believed animals assimilated foods and why he considered his own approach to animal nutrition superior to others. He included a description of his calorimeter and described the experiments he had performed with that apparatus.

Perhaps most noteworthy was that it marked the first time that Armsby acknowledged in print the recent "discovery" – though they had, in fact, always existed – of "an important but as yet rather ill-defined group of food constituents called by some investigators vitamins and by other growth substances."[139] Armsby demonstrated his

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[137] Ibid., 66.

[138] Henry Prentiss Armsby, *The Nutrition of Farm Animals* (New York: The Macmillan Co., 1917), vi.

[139] Ibid., 41.

awareness of the work of those who had first investigated these substances – Funk, Hart and McCollum, and Osborne and Mendel – but seems at this point to have considered them as constituents of growth rather than elements maintenance or production. He refrained, however, from making any personal assessment of the importance of vitamins, merely noting that "The subject is one which is hardly ripe for discussion, but it opens up an interesting field for investigation."[140]

The following year, in 1918, Armsby and his associates at the Institute of Animal Nutrition at the Pennsylvania State College, J. August Fries and Winfred Waite Braman, published a short article in the Proceedings of the National Academy of Sciences. Entitled "The Basal Katabolism of Cattle and Other Species," the short paper reported the latest findings of Armsby's research. Armsby had, by this point, been investigating the metabolism of cattle with his calorimeter for almost two decades. One of his primary aims had been establishing the "katabolism" of cattle – which he defined as the amount of energy employed by a fasting animal. Armsby evidently believed that finding this number would establish a baseline for his research. Unfortunately, he found that "the basal katabolism of…ruminants, unlike that of man or carnivore, cannot well be measured in the fasting state on account of the relatively large amount of feed always present in the alimentary canal." Despite this fact, Armsby postulated that the katabolism could "be determined indirectly…by measuring the total metabolism upon two different

[140] Ibid.

amounts of the same ration and…computing the level to which the metabolism would be reduced were all feed withdrawn."[141]

Having estimated the "daily basal katabolism per square meter of body surface" of a pair of steers, and finding that each animal produced similar readings, Armsby concluded his paper by comparing the results he obtained with those of other researchers who studied the "katabolism" of men, women, hogs, and horses. Listing the values in a table, Armsby demonstrated that the numbers – save for those of hogs, which while within 10 percent of the other results represented a significant outlier and, Armsby believed, was "due to the imperfect data available for computing the body surface of this species" – fell within a very narrow range. He concluded that the "rather striking degree of uniformity" among the results "tended to confirm the conclusions" the German scientist E. Voit. In short, between these announcements and his increasingly strident claims about the importance of nutritional science it seems likely that Armsby believed that he and others working along similar lines had discovered a fundamental scientific law.[142]

The last years of Armsby's life found him occupied with a number of subjects only indirectly related to his work on nutrition. As noted above, he served on a government commission that examined food supplies in Europe. He called for the establishment of a National Institute of Nutrition, and presented his plan for a new organization of research that would place a renewed emphasis on the merits of pure,

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[141] Henry Prentiss Armsby, J. August Fries, and Winfred Waite Braman, "The Basal Katabolism of Cattle and Other Species," *Proceedings of the National Academy of Sciences, Vol. 4, No. 1* (Jan. 15, 1918): 1.

[142] Ibid., 4-5.

rather than applied, science. The handful of scientific articles he published were co-authored, usually by his long-time assistant, J. August Fries.

Armsby did start work on a new book in 1919. Unfinished at the time of his death in 1921, *The Animal as a Converter of Matter and Energy* was completed by C. Robert Moulton. Though this fact makes efforts to ascribe specific passages to Armsby necessarily tenuous, certain sections smack of Armsby, both conceptually and linguistically, and serve to show that, until the end of his life, Armsby clung to his belief in the merits of his scientific method.

In the introduction Armsby makes a final use of his "animal as machine" metaphor: "The vitamins…are essential to the life and well being of animals. Apparently they contribute no energy or mineral matter to the organism but are more comparable to the electric spark which fires an internal combustion engine." Though Armsby seems to acknowledge the importance of vitamins, more important is his assertion that they do not act as fuel for the animal. This suggests that, as such, they act outside of Armsby's nutritional purview: the existence of vitamins did not invalidate or challenge Armsby's findings; instead they merely constituted a separate topic for investigation.[143]

The Animal as a Converter of Matter and Energy contains two other references to vitamins. The dates of footnotes referenced in one section make it clear that Moulton wrote passage after Armsby's death. However, the other passage, while of unclear authorship, expresses views that Armsby espoused throughout his career. The author laments that investigation about vitamins "has thus far been almost exclusively

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[143] Henry Prentiss Armsby and C. Robert Moulton, *The Animal as a Converter of Matter and Energy* (New York: The Chemical Catalog Co., 1925), 11.

qualitative…almost nothing is yet known regarding their physiological function." Regardless of its authorship, this passage neatly encapsulates the approach Armsby had preached during the last three decades of his life. First, that answers – and ultimately, principles – can be found only the employment of quantitative, not qualitative, techniques. Second, to understand the role of these vitamins one needed to study not the substances themselves – whatever they may be – but their effects on the animal itself.[144]

Unfortunately, this approach proved but half correct: vitamins had been discovered precisely because of the effect they produced – or more precisely, the effect that their absence produced – on the animal body. Early vitamin researchers proceeded in just this manner, by supplying their subjects with one type of feed and noting the positive or negative results on their subjects. Only by a trial of process and error experimenting on animals did scientists finally isolate the various mysterious, but necessary, elements needed to sustain life. Their contribution will be considered in a later chapter; for the moment it seems only needful to add that their methods required the abandonment, at least temporarily, of Armsby's quantitative method.

Given the embryonic stage of knowledge about vitamins it seems unfair to declare that Armsby's work had been in vain; it appears unlikely that he himself would have done so. After all, he had developed an apparatus and approach that allowed him to answer the questions in ways that he considered meaningful. While one could snidely remark that Armsby's most important discovery was the fact that animals burn a certain amount of energy in consuming and digesting their fodder, doing so fails to appreciate Armsby's many exceptional qualities.

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[144] Ibid.

As a laboratory researcher, Armsby proved indefatigable: his colleagues recount that he personally conducted the bulk of the calorimetric research, an arduous task that required unwavering concentration and the ability to make thousands of readings and calculations without error. Armsby never "fudged" errors; instead he reported them honestly while at the same time attempting to uncover and eliminate possible inconsistencies. He proved a talented and ambitious promoter of the importance of agricultural science in general and nutrition in particular: not only did he publish numerous articles trumpeting the promise of nutritional research, but he served as president of the Society for the Promotion of Agricultural Science from 1905 to 1907. He played a vital role in founding The American Society of Animal Nutrition, and presided over that body for three years.

Indeed, it was his very faith in the quantitative method he endorsed that allowed him to make his greatest conceptual leap – that animals operated under the same chemical and physical laws as the rest of the physical world. That same confidence helps explain the importance he ascribed to the role of science in ameliorating or eliminating world hunger, a topic that became increasingly important to him later in his career. Having proved to his satisfaction that animals operated as essentially "closed" systems, it only took a small conceptual step to envision the world operating under the same parameters; the science of nutrition applied to the whole – the world – just as it did to its parts – the animal. The key lay in rationally and efficiently supplying all the various cells with the necessary nutrients.

It is precisely this belief in the possibilities of science and his conviction in his methods that make Armsby so useful to the historian. His example clearly demonstrates the advantages as well as the dangers of total devotion to a particular method. Though later scientists adopted other approaches, Armsby provided a model of consistency, and this is what makes his a somewhat tragic example. A lifelong opponent of blind adherence to scientific orthodoxy, Armsby himself ultimately became the pawn of a totem of his creation, one that ultimately closed more doors than it opened.

Armsby died in 1921, and while his colleagues and protégés continued research very much in the direction that Armsby had charted for some four decades, his methods represent something of a scientific cul-de-sac: a promising approach that suggested intriguing possibilities but ultimately did not produce the sorts of results that Armsby had hoped they would. Though E.B. Forbes, Armsby's hand-picked successor as director of calorimetric research, could confidently state in 1926 that "I see in the prospect such promise that I am pleased to devote the rest of my life to the study of the subject," Forbes own successor, R.W. Swift, ultimately realized the futility of Armsby's scientific approach. Swift wrote in 1954 "it is quite clear that the…adoption of net energy as the measure of the relative nutritive value of feeds is impracticable."[145]

In the end, it was exactly this dedication to the methodology of quantitative analysis that made Armsby something of a tragic figure: even as he struggled to make sense of the data supplied by his calorimeter, and devised ever more complicated procedures that might account for the unexpected results he sometimes obtained, some of his fellow nutritionists were actively exploring a new, radically different approach to nutrition, one that drew heavily on some of Armsby's advances, but which offered new

[145] Both quotes from Robert L. Cowan, "Henry Prentiss Armsby, 1853-1921: A Brief Biography," *Journal of Animal Science, Vol. 66* (1988): 1839.

possibilities of understanding how animals –human or not – assimilated and employed foodstuffs. They did so because they had discovered, or, more properly, realized, that rations contained important, if still at that point, unknown elements in addition to the building blocks of Armsby's nutritional pyramid – proteins, carbohydrates, and fats.

In a word, these scientists had discovered vitamins, sometimes referred to as "vital-amines" or "trace elements," that existed in foodstuffs and played an unknown but recognizably important role in nutrition. As they explored the ramifications of this discovery, they realized that the accepted rules of animal nutrition failed to account for these new elements: "x" amount of protein plus "y" amount of carbohydrates plus "z" amount of fat did not a perfect ration make – necessarily.

As has been noted in this chapter, Armsby did not turn a blind eye to the discovery of vitamins. In fact, he discussed them briefly in the 1917 edition of *The Nutrition of Farm Animals* – the most widely used and cited general handbook of animal nutrition. However, his comments make it clear that Armsby did not consider that the discovery of these new elements might invalidate his research and approach. Instead, he seems to have thought of them as materials possibly essential – and intrinsic - to the growth process, but not necessarily important to the maintenance of mature animals and, more importantly, elements that, once identified, could be incorporated into this quantitative methodology. Armsby's inability to understand that such a method might not eventually explain the natural phenomena he explored make an understanding of his methods key to understanding the mindset of a generation of scientists.

CHAPTER 5

HOW TO MEASURE SOMETHING NEW: DAIRY SCIENTISTS EVALUATE MILKING MACHINES

On January 26, 1900, the United States Patent Office issued patent number 642044 to a pair of Scotsmen, William Henry Lawrence and Robert Kennedy. The patent documents Lawrence and Kennedy's claim to "have invented certain Improvements in Milking Apparatus." Specifically, their patent pertained to "milking apparatus in which suction is employed to draw the milk." They claimed to have developed an "improved apparatus to impart a pulsating action in a better and more convenient manner than has been hitherto accomplished."[146]

The remainder of the patent document spells out the details of Lawrence and Kennedy's "improved apparatus." Its novelty – which made the milking machine a practical and useful device for dairymen and, eventually, changed the nature of commercial dairying – stemmed from the fact that it successfully resolved the two major challenges that had hitherto impeded the perfection of a pneumatic milking machine: how to successfully employ a vacuum pump to extract milk from cattle, and how do configure the machine so that milkmen could employ a number of the devices at the same time.[147]

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[146] United States Patent and Trademark Office Document No. 642044, Jan. 23, 1900.

[147] Ibid

An examination of the adoption of the milking machine is historically useful because it reveals how dairy scientists, working at agricultural experiment stations, evaluated an emerging technology. In fact, it represents the first widespread testing of a commercial device performed by experiment station scientists; the only comparable invention, the Babcock milk-fat apparatus, had been developed by a station scientist and thus enjoyed a de facto approval. The introduction of commercially viable milking machines in the first decade of the twentieth century afforded scientists the opportunity to apply their scientific techniques and methodology to a new subject.

Experiment station records reveal that scientists employed traditional approaches in appraising the new machines. Rather than consider the machine as a radical departure from the past, they subjected the mechanical devices to the same tests they had used to evaluate accepted techniques and apparatus. Hence, scientists interested in the cleanliness of milk evaluated the milking machine just as they had the various covered milk-pails that appeared on the market: they counted the number of bacteria in machine-drawn milk. Likewise, scientists interested in finding faster methods of milking cattle performed trials in which they timed how long it took to milk a certain number of cows with one machine, with two, etc. These scientists maintained a very conservative approach to the new machines.

Simply put, the scientists who tested the first milking machines in the first two decades of the twentieth century employed the same methodology and approach that they and their colleagues applied to their other researches. Namely, they believed measurement and quantification held the keys to scientific progress. They applied these techniques to the new milking machines as a matter of course: the appearance of new technology did not force them to reevaluate accepted techniques, nor to adopt new research methods.

Of course, the machines also presented new challenges, and forced scientists to devise ways to apply accepted techniques. Most importantly, researchers attempted to determine the short- and long-term effect of milking machines on the production and longevity of dairy cattle. Doing so required that they set up test groups, one milked by hand and the other by machine, and compare results. However, despite the novelty of these experiments, scientists went about their work following accepted techniques: they identified discrete elements that they could measure and then designed research methods that could produce these results.

In short, though the adoption of milking machines would eventually play a key role in the creation of the "modern" dairy, scientists did not treat them as revolutionary devices. Instead, they tended to view milking machines as mechanical aids for dairymen. Their evaluation of milking machines demonstrates the conservative approach employed by most contemporary scientists

It also afforded them the opportunity to act as arbiters of the new devices. Though a number of stations surveyed farmers about their experiences with milking machines, and published the results, the scientists evaluated the machines in ways that were meaningful to them, not necessarily to farmers. Put another way, by evaluating the machines in a "scientific" manner, researchers privileged their own criteria, which allowed them to put their imprimatur on the new devices.

The last three decades of the nineteenth century witnessed a flurry of interest in the development of some sort of milking machine. Though it issued only a handful of patents for milking devices prior to the Civil War, the United Stated Patent Office issued dozens of patents for milking machines of various designs between 1870 and 1900; researchers at the USDA noted that "during the period from 1872 to 1905, inclusive, 127 patents were taken out in this country alone for milking machines or separate parts of them."[148] These machines took three basic forms: milk-straws, mechanical milking machines, and pneumatic milking machines.[149]

Milk-straws, which consisted of a hollow tube or catheter that was inserted through the teat into the udder, had a long history as a veterinary and therapeutic device; veterinarians and dairymen still employ them to treat a variety of diseases. However, in the late nineteenth century a number of inventers patented such devices as practical milking, rather than veterinary, instruments. All employed a similar strategy: inserting the straw through the animal's teat into udder would "tap" some sort of milk reservoir. These devices met with little success. First, the notion that one could tap the udder much as one might tap a maple tree for sap betrayed a basic and widespread ignorance of animal physiology. Farmers and scientists alike generally believed that the udder was simply a balloon-like structure that stored milk and could be tapped like a keg. Only later would physiologists discover that the udder consisted of milk glands that stored milk and secreted milk only when massaged. Put simply, milk-straws did not work because they could not work. True, one could coax some milk through the tube, but the device blocked rather than facilitated the extraction of milk. Second, such devices tended to

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[148] C.B. Lane and W.A. Stocking, "The Milking Machine as a Factor in Dairying," *USDA Bureau of Animal Industry Bulletin No. 92* (1907): 9.

[149] Oscar Erf, "Milking Machines," *Kansas State Agricultural College Agricultural Experiment Station Bulletin No. 140* (1906): 1-2

injure animals. Though they rarely killed the cow, the use of milk-straws often destroyed their teats and udders, which amounted to the same thing. Even when they were "successfully" inserted in a manner that did not immediately injure the animal, their use tended to spread disease and infection that eventually incapacitated the cow. Though the Patent Office issued one John Sullivan of Massachusetts a patent for an "improved milk straw" as late as 1896 – itself indicative of the rudimentary knowledge of the time – the milk-straw proved a failure as a milking aid, and dairymen returned them to the medical kit.[150]

While the use of milk-straws never achieved a wide vogue, designs for mechanical and pneumatic milking machines proliferated in the late nineteenth century. Each attempted in its own way to emulate the two time-tested methods of extracting milk from a cow: mechanical milkers attempted to mechanize hand-milking (or, in contemporary parlance, "stripping") as had been practiced since humans domesticated dairy cattle, while pneumatic milking machines reproduced the suckling motion of the nursing calf. Based on patent records, mechanical milking devices seem to have been slightly more popular with inventors than pneumatic machines. All of these mechanical machines employed the same basic principle: they each tried to recreate the motion and pressure of human hands or fingers to extract milk from dairy cattle.[151]

Though the idea of a mechanical milking machine proved popular with inventors – the United States Patent office issued at least twenty-five patents for such devices

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[150] Ibid., 4.

[151] I have never found an advertisement for such machines in my perusal of nineteenth century agricultural journals and publications. Such an advertisement might exist in some local paper, but such a search is beyond my capabilities.

between 1875 and 1901 – they seem to have met with little commercial success. Of course, records of such devices are scarce, but the absence of advertisements for any sort of milking machines in contemporary agricultural journals suggests that dairy farmers could hardly have known about such developments unless they lived in relatively close proximity to the inventors of such devices. Furthermore, the fact that contemporary Sears' catalogs – which sold everything from girdles to guitar strings – never carried mechanical milking machines in their catalogs, though they did sell virtually every other apparatus a dairyperson might possible require – suggests that mechanical milkers never enjoyed a commercial popularity. That contemporary dairy researchers never considered them viable is shown by the fact that only one bulletin of the thousands issued by the various State Experiment Stations even mentioned these mechanical milking machines, and then only as a matter of historical record; in any event, no stations ever reported testing the practicality of mechanical milking machines.[152]

For a host of reasons, mechanical machines failed to find a market. First, these inventions tended to be very mechanically complicated; they employed countless numbers of springs, levers, and other devices in their attempts to emulate the human hand. Second, the machines generally employed a "one-size fits all" approach to cattle; not all possessed any means by which to adjust the mechanical fingers to individual animals. Third, though contemporary standards of milking hygiene rarely dictated the washing of hands, or even of the cow's udder or teats, these machines, constructed of iron

[152] I looked at reprints of the Sears catalog for the years 1898 and 1901. As for the Agricultural Experiment Stations, I have gone through almost every bulletin issues between 1888 and 1960 and have never encountered any other references to mechanical milking machines. In addition, the USDA's bibliography of dairy literature does record any mention of such devices.

and often employing leather components, would have made any attempts at sanitizing them an exercise in rust and rot prevention. In any event, they mark an interesting, though ultimately negligible, foray into the world of practical milking machines.

The use of pneumatic milking machines that emulated the suckling of a young calf proved the most popular and ultimately most successful approach. However, early designs proved rather ineffectual; most consisted of metal cups that could be fitted to the animal's udder or teats, while a simple belt or strap attached the device to the cow. Having fitted the device, the user produced a vacuum by the use of a hand- or foot-powered pump that, ideally, drew the milk from the animal. Unfortunately, these early attempts ignored the basic physiology of dairy cattle: they failed because they did not combine the suction with pressure to imitate the suckling action of the nursing calf.

By the mid-1890s a number of inventors, both European and American, working independently, had devised an elegant solution to the problem. Their innovations lay in the clever employment of dual vacuums: the first a vacuum that kept the device firmly attached to the animal, the second a vacuum between the metal wall of the teat-cup and the rubber – or, in early examples, leather - liner that actually contacted the cow. Varying the vacuum pressure within this closed space caused the rubber liner to expand and contract, stimulating the suckling of the calf. This design's success can be attributed to its two main advantages. First, these machines eliminated the need for the bulky, complicated mechanical devices employed by earlier designs. Second, a number of machines could be powered by the same vacuum pump, reducing the amount of machinery necessary.

Despite these advances, which promised to deal with the major drawbacks of earlier models, these improved designs proved less than satisfactory in actual use. A research bulletin of the Wisconsin Agricultural Experiment Station summarized the situation: "This milker [the Shiels 'Thistle' milking machine, typical of these designs] was a great improvement over all prior devices, as Shiels was the first to produce vacuum pulsations in the teat-cups by admitting air. The system had, however, some fatal defects." The primary difficulty lay in the difficulty of supplying two separate – and different – vacuum pressures to the milking machines. To address the problem, these devices typically employed two different vacuum hoses or pipes: one to supply the pressure needed to attach the machine and carry the milk from the animal to the pail, the other to supply the "massaging" pressure that stimulated milk flow. In addition, this second vacuum supply required the use of some sort of apparatus, commonly refereed to as a "pulsator," to produce the varying pressure required. The primary problems of the system stemmed from the "pulsating" side. While such devices generally performed acceptably when used with a single milking machine, the use of multiple machines caused widely varying levels of pressure that, at best, prevented the machines from working and, at worst, actually caused injury to animals. Furthermore, while the machines might have functioned adequately when used on individual animals, inventors knew that the primary advantage of the milking machine stemmed not from saving the dairy-hand the labor of milking an individual animal, but by allowing a single worker to milk multiple animals simultaneously. Though a number of inventors attempted to address the problem, none of their solutions met with wide success. [153]

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[153] F.W. Woll and G.C. Humphrey: "The Efficiency, Economy, and Physiological Effect

The aforementioned Scotsmen, Lawrence and Kennedy, successfully solved the problem in a way that was both elegantly simple and – with the benefit of hindsight – rather obvious: they simply moved the “pulsator” apparatus from the vacuum compressor, which was typically located in a separate room that also housed the motor that provided its power, to the milking machine itself. Rather than employ one pulsator to supply impetus to the whole system, Lawrence and Kennedy attached a separate pulsator to each individual milker. Doing so neatly rectified the flaws that had prevented the widespread adoption of the pneumatic milking machine. The significance of Lawrence and Kennedy’s innovation can be seen in the fact that after more than a century their invention remains the core of modern milking machines.

Moving the pulsator from the compressor to the milking machine neatly solved the various problems that had plagued earlier designs. First, it eliminated the need to employ two separate vacuum lines: the new pulsator split and regulated the vacuum pressure necessary for the dual operations of the milking machine. Second, the use of separate pulsators eliminated the huge variations in pressure that had plagued earlier designs. Dairy farmers could employ one, two, or more milkers without having to adjust the pressure of the whole system. This flexibility increased the commercial appeal of these machines; having bought the vacuum pump, dairymen could easily expand their operation by simply purchasing more milking units. Finally, the use of individual pulsators meant that the dairy-worker could (relatively) easily customize the machine to

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of Machine Milking,” *Wisconsin Agricultural Experiment Station Research Bulletin No. 3* (1909): 118.

the individual animal: the dairy worker could adjust both the vacuum exerted and the frequency of the pulsations to suit the requirements of specific cows.

Loomis Burrell, a New York dairyman, combined the Lawrence-Kennedy design with his own improvements and, in April 1905, introduced the B.L.K. (Burrell-Lawrence-Kennedy) milking machine. It became the first commercially successful mechanical machine marketed in the United States. Burrell and his father operated a dairy in upstate New York, and, according to a manufacturer's statement, had experimented with the use of milking machines since the 1860's. Burrell considered the Lawrence-Kennedy design "a vast improvement on other systems." Their design not only was "able to produce sharp vacuum pulsations in the teat-cups" but also "reduced to one-sixth or one-eighth" the power necessary to power the system compared to earlier design. In Burrell's opinion, the Lawrence-Kennedy design "changed the milking machine from something totally unpractical to a practical, simple, successful system."[154]

Burrell licensed the American rights to the new machine and proceeded to make his own improvements. Most importantly, he improved and simplified the design of the Lawrence-Kennedy pulsator in order to produce a "pulsator that could be instantly taken apart to clean with the aid of tools." He also added a clear glass window to the bottom of the milking attachment that allowed the dairyman to tell at a glance when the milking had been completed.[155]

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[154] Ibid., 118-119.

[155] Ibid. See also United States and Trademark Office Granted Patent No. 784780, granted on March 14, 1905.

The B.L.K milking machine met with almost-instant success. Though no records document the number of machines sold, some measure of their promise can be found in the fact that no fewer than five State Agricultural Experiment Stations bought and began trials with the B.L.K. machines in 1905 or 1906, and a number of other stations subsequently acquired the devices. In contrast, before this time not a single Experiment Station in the entire country had reported the use of milking machines; those that did acquire and test the B.L.K. milking machine usually commented that it was the first mechanical milking machine that the station had acquired.

Between 1906 and 1908 five Agricultural Experiment Stations – Wisconsin, Nebraska, Kansas, Pennsylvania, and Connecticut (Storrs) – as well as the U.S. Bureau of Animal Industry, based in Beltsville, MD, issued reports of their initial trials using the B.L.K. milking machine. Each of these experiments lasted at least eighteen months, and three of the six spanned over two or more years. An examination of these illustrates how scientists applied accepted practices and techniques in evaluating the new apparatus. This study not only confirms the essentially conservative manner in which scientists went about their work but shows that researchers often combined their trials of the new machines with their ongoing research. Put another way, scientists did not necessarily regard milking machines as new entities that needed to be investigated on their own, "new," terms; instead, they often viewed the devices as novel ways to evaluate – and presumably validate – their own established theories.

These bulletins demonstrate that workers at the various stations, working independently, shared a common set of assumptions about how to evaluate the new machines. Each acknowledged the labor demands that dairying placed on the farmer, noted the difficulty of securing skilled and reliable workers, and asserted that the development of a practical milking could increase the profitability of the dairy farm by allowing the farmer to milk more animals. F.W. Woll, of the Wisconsin Experiment Station, summed up these feelings in the introduction to his report: “One of the great objections to dairying is the difficulty of securing efficient help to do the milking. This difficulty has doubtless materially retarded the development of the dairy industry, and…caused farmers to keep fewer animals than they otherwise would.”[156] Oscar Erf, a researcher at the Kansas Station, echoed these comments: “With this condition [the difficulty in securing dairy workers]…it is quite essential for many western dairymen either to discontinue the dairy business or secure an apparatus that will do the milking.”[157] Researchers at the Connecticut Station amplified this theme: “occasionally a man give up the dairy business simply because of the impossibility of getting satisfactory help at prices warranted by the returns from the business.”[158] Their colleagues at the other stations echoed the difficulties of obtaining labor on the dairy farm. These complaints took three main forms. First, farmers had difficulty hiring skilled, reliable workers at any price. Second, what workers were available often balked at performing dairy chores. Third, those who did not mind milking often possessed little aptitude for the task. The development of a milking machine that promised to ameliorate these

[156] F.W. Woll and G.C. Humphrey: “Milking Machine Experiments,” *University of Wisconsin Agricultural Experiment Station Bulletin No. 173* (1909): 3.

[157] Oscar Erf, “Milking Machines,” *Kansas State Agricultural College Agricultural Experiment Station Bulletin No. 140* (1906): 1.

[158] W.A. Stocking, Jr. and C.J. Mason, “Milking Machines,” *Storrs Agricultural Experiment Station Bulletin No. 47* (1907): 105.

difficulties seemed a boon to contemporary researchers and, no doubt, to overworked dairy farmers.

For the most part, the trials performed by the Experiment stations assumed a form similar to those employed by researchers interested establishing the values of various feeding stuffs: scientists isolated one specific variable while holding all others as nearly constant as possible. In general, these trials followed an identical procedure. The researchers divided the test animals into two groups, usually with an eye to maintaining a balance of animals that shared as nearly possible ages, breeds, stages of lactation, size, etc. The two sets of animals received identical treatment: they ate the same food, had the same access to water, lived in the same barn, were milked at the same time of day, etc., the sole difference being that one groups of animals were milked with the milker – in almost all cases the B.L.K. machine – while the other group was milked by hand.

These experiments sought to answer, in sometimes interesting ways, five basic questions. First, did the machines have a short- or long-term effect on the animals' production of milk and butterfat? Second, were the machines efficient; that is, did they extract an adequate amount of milk, or did the animals require subsequent hand-milking? Third, were the machines economical, in the sense that they increased the efficiency of the dairy worker enough to justify their expense? Fourth, did the machines produce milk at least as hygienic as that produced by hand-milking? Finally, a number of stations surveyed farmers of their respective states to determine their experience with, and impressions of, the machines.

The most numerous of these experiments attempted to determine the effect of machine milking on the animal's production of milk and butter-fat. Scientists conducted trials which examined whether the machine's caused any differences in either the short- or long-term yield of dairy cattle. The stations found that, in most cases, the use of milking machines did not affect the long term production of animals; animals in the machine-milked group not only averaged about the same production as their sisters in the hand-milked control group, but generally maintained a steady production in comparison with previous lactations.

However, these trials produced some unexpected results. In some cases, animals that had been known as good milkers refused to "let down" their milk for mechanical milking machines. In most of these instances the cows generally became accustomed to the new method, but a handful of animals never reconciled themselves to mechanical devices. In these cases, the department, depending on its practices, either sold the cattle or relegated the animals to the "hand-milked" herd.

A different problem stemmed from the fact that the anatomy of some animals did not facilitate machine milking. In specific, the milking machines worked poorly, and in some cases did not work at all, on animals that had particularly fat or short teats, or possessed a peculiarly shaped udder. Usually these animals did not mind the application of the machine; rather the machine proved ill-equipped to handle these particular animals. The results posed a conundrum for researchers at the Nebraska Experiment Station. After discussing two cows whose anatomy thwarted attempts at machine milking, but which were otherwise excellent animals who not only produced well above-average amounts of both fat and butter-milk, the Station's scientists were forced to conclude that "If a herd of cows could be procured that would respond to machine milking as readily as the above-named animals, the milking machine would be a boon; but if several cows in a dairy herd cannot be milked in this way, the herd totals would be lowered and the resultant loss could hardly be compensated for the by saving of time, labor, etc."[159]

These results caused the scientists to begin to question whether the "problem" might be solved by selecting different cattle instead of modifying the machine. The Nebraskan researchers continued "[The milking machine's] successful application cannot be assured until all the members of the herd are known to be adapted to this method of milking." This passage's importance stems from the fact that it seems to be first mention in the vast literature of the state Experiment stations that hints at the possibility of selecting, and ultimately, breeding cattle, specifically to fit the artificial conditions of the dairy farm. Animal researchers and dairy farmers did, in fact, eventually breed animals that fit the architecture of the dairy. True, dairymen had for centuries attempted to breed animals with desirable characteristics, most specifically animals that would produce large quantities of rich milk. But the case of the Nebraska researchers suggested that other, supposedly "secondary" characteristics like the shape of the animal's udder or teats might play an important role in determining an animal's desirability and profitability. Again, the researcher's quandary stemmed from the fact that until they attempted to apply mechanical milking machines to these animals, they considered these cows to be among the finest in the university herd. [160]

Though experiments that measured and attempted to quantify the short- and long-term effects of milking machines on the milk and butterfat production of dairy animals

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[159] A.L. Haecker and E.M. Little: "Milking Machines," *University of Nebraska Agricultural Experiment Station Bulletin No. 108* (1908): 29-30.

[160] Ibid., 30.

comprised the bulk of research designed primarily to measure how cattle responded to the machines, researchers also conducted a number of other experiments along these lines that reflected the particular interests of that researcher or experiment station. For example, researchers at Nebraska studied the impact of switching back-and-forth between hand and machine milking. The test indicated, as dairy farmers had long insisted, that dairy cattle craved consistency above all else; the production of most animals declined precipitously over the course of these trials, but improved to normal levels once one or the other method was adopted.[161]

In a similar manner, scientists at the Connecticut-Storrs Station combined their milking machines trials with their continuing interest in the "summer-shrinkage" of production in dairy cattle. For a number of years, researchers had attempted to quantify the effect of "the change from June days and fresh feed to 'dog days,' 'fly time' and sparse pastures…" Their goal seems not to have been finding ways to overcome these conditions. Instead, they hoped to be able to estimate such production changes so that dairymen could accurate predict milk yields and profit. Their experiments found no difference in yield of milk or butterfat produced by machine or hand milking; the milking machine proved no remedy for "summer shrinkage."[162]

As researchers in Connecticut examined the effect of heat and dry pastures on production, their colleagues at the Pennsylvania Station attempted to determine the effect

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[161] Ibid.

[162] Ibid. See also The Eighth Annual Report of the Connecticut State Dairy Commissioner and Storrs Experiment Station Bulletin No. 26. C.L. Beach, "Milking Machines, Part II," *Storrs Agricultural Experiment Station Bulletin No. 47* (1907):, 135-136.

of machine milking on the flavor of milk. To this end, they collected samples of both hand and machine drawn milk "in sterile bottles" which were "set in the refrigerator and allowed to stand for a varying length of time." A battery of tasters then sampled the milk, and ranked its flavor on a qualitative scale ranging from "excellent" to "very poor." Their results demonstrated that the method of milking had no effect on the taste of the milk produced. Some cows inherently produced "good" tasting milk, while others produced "poor" milk. This test's historical importance stems not from its "scientific" conclusion that some dairy cattle produce "better" tasting milk than others, but from the fact that this experiment serves as an example in which dairy researchers reverted from quantitative to qualitative standards of measurement. True, other Stations had conducted experiments to determine the "freshness" or "digestibility" – terms that possibly might be construed as weak synonyms for "flavor" – but these tests defined these terms in precise quantitative terms. For example, researchers defined the "digestibility" or "freshness" of milk in terms of temperature and/or the presence of a certain level of bacteria or contaminants. In each case these scientists determined a quantitative measure of the characteristic to be studied and framed results in quantitative terms. The case of the Connecticut "flavor" experiment therefore proved something of an anomaly, the "exception that proves the rule," and demonstrates the how quantification defined the approach of dairy scientists in the decades surrounding the beginning of the twentieth century.[163]

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[163] T.I. Mairs, "Test of a Mechanical Cow Milker," *Pennsylvania State College Agricultural Experiment Station Bulletin No. 85* (1908): 9-11.

Though all of the state Experiment stations that examined milking machines studied the effect the devices on the production of milk and fat, researchers at two stations paid special attention to the efficiency of machine milking. By this they meant how thoroughly the machine extracted the cow's milk. The failure to do so could have serious implications. Most importantly, leaving a large quantity of milk un-extracted could harm the animal. Too, the need to hand-milk after machine milking – whether to avoid health problems or simply to produce a maximum yield – required time and diminished the usefulness of the milking machine. Finally, milk that remained in the animal represented an economic loss that could amount to hundreds or thousands of dollars every year, depending on the size of one's herd. The Wisconsin Agricultural Experiment Station had a history of investigating how to most efficiently milk dairy cattle; for example, Bulletin No. 96 of that Station described a method of manipulating the udder that would maximize milk production. A pair of Wisconsin dairy scientists took the lead in these studies. Those researchers, F.W. Woll and G.C. Humphrey, concluded that machine milking "is practically equal to that done by good hand milking" and added that it "is doubtless superior to that done on many dairy farms."[164]

Most of the evaluations of milking machines performed by the experiment station examined how their use affected cattle and amount of milk they produced, but researchers also paid significant attention to the effect of milking machines on their human operators. Specifically, having determined that the machines were practical, in that they performed reliably and neither harmed the animal nor markedly diminished her production,

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[164] F.W. Woll and G.C. Humphrey: Wisconsin Agricultural Experiment Station Bul. No. 96.

scientists turned their attention from the machines to the dairymen who would employ them. They wanted to find out whether milking machines would, in fact, make farmers more productive. Simply put, they wondered if machines save enough in labor costs to justify their purchase.

To answer this question, researchers compared the amount of time necessary to hand-milk cattle with the time necessary to machine-milk the same number of animals. Discounting the extra time required to clean and sterilize the various components of the milking machine, they found that the typical dairy-hand could milk the average animal using their hands somewhat faster than they could using a single machine. However, machine-milking proved much more time-efficient when dairy farmers employed multiple machines. Independent trials at the various Experiment stations determined that a single dairy-hand reached maximum efficiency by machine milking six cattle at a time once both the men and animals became used to the operation of the machines. Scientists found that milking fewer than six animals at one time decreased efficiency, while milking more proved impractical. (It should be noted that, even today, when virtually all milking parlors are equipped with pipelines that carry milk directly to the cooling tank – eliminating the need to carry the milk – eight animals is about the limit that one person can milk at a time.)[165]

These trials demonstrated the shortcomings of the researcher's quantitative approach. Though the scientists took care in calculating the time required to milk one animal at a time using milking machines, then two animals, etc., none of the researchers

[165] See Haecker and Little, 44-52, Mairs, 6, Wolf and Humphrey, 24-25, C.B. Lane and W.A. Stocking: "The Milking Machine as a Factor in Dairying," *USDA Bureau of Animal Industry Bulletin No. 92* (1907): 18,24.

indicated how they determined exactly how long it took to milk the same animals by hand. In each case, they simply cited a figure that they then used to compare the experimental results of machine milking. Only the Nebraska station even attempted to "scientifically" determine how long it took to hand milk cattle. They had a couple of their hired men each milk a dozen animals while timing the overall milking time. Dividing the total time by the number of animals milked produced an "average" time to hand-milk a dairy cow. To complicate matters, they performed these tests on a different set of animals, not those involved in the machine tests, and thus the times they employed for their comparisons are not necessarily indicative of the time that might be lost or saved by machine milking. Given the researcher's commitment to isolating single variable which they could then compare, such an oversight seems striking. [166]

In the face of the researchers' tacit assertion of the hand-milking figures, one senses that in performing these tests they ran up against the limitations of their approach. Devising an appropriate test seems rather straightforward: one would milk a group of two animals by hand for a period of time, and then milk them by machines for a similar period. But, such an experiment would fail to account for a number of factors. For example, cows produce less milk over the course of their lactation; they typically produce the most milk in the months immediately after delivering their calf. Furthermore, as researchers had already demonstrated, most cattle took some time to become accustomed to machine-milking. Both of these factors would impact the accuracy and usefulness of the findings that might be produced by such an experiment. As will be discussed in later chapters, by the nineteen-teens the shortcomings of simple, comparative quantitative

[166] Haecker and Little: "Milking Machines," Nebraska, pp. 52-53.

analysis began to become apparent on scientists exploring virtually every aspect of dairying, and they would eventually begin to design new testing procedures that they hoped would surmount these obstacles. The (unacknowledged) difficulties encountered by researchers who tried to determine the economy of milking machines demonstrate the limitations of their scientific approach.

Considerations of economy intersected with concerns about the sanitation of milking machines. Though they represented separate factors to be examined, researchers interested in the economy of milking machines factored the time required to clean and sanitize the myriad components of milking machines into their evaluation. The time required to both prepare and clean milking machines could be substantial as dairy-workers had to sanitize machines before use as well as disinfect them after; researchers at the Wisconsin Station estimated that it took "10 to 20 minutes to get two machines ready for the milking, and 20 minutes to clean them afterwards."[167] Scientists at the Nebraska station calculated that preparing and cleaning the machines took even longer: "The time consumed by one man in washing, sterilizing, and caring for three milkers…was found to be one hour and forty minutes per day, on an average." However, they determined that "such a computation would not be fair, because a certain amount of time, 20 to 30 minutes, is used in boiling parts" during which the dairy hand "could turn his hands to other things."[168]

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[167] F.W. Woll and G.C. Humphrey, "Milking Machine Experiments," *University of Wisconsin Agricultural Experiment Station Bulletin No. 173* (1909): 25.

[168] Haecker and Little, 53. From my own experience growing up on a dairy, sanitizing the milking machines prior to milking usually took about five minutes, while cleanup afterwards required approximately twenty minutes.

Ultimately the researchers' recommendations about the economy and profitability of using milking machines ranged from the non-committal to the guardedly enthusiastic, especially when used under certain conditions; significantly, none dismissed the machines outright. Researchers at the Storrs (Connecticut) Station concluded their report by noting simply that "This report of the results with milking machines is submitted without discussion or comment. No conclusions should be drawn from so limited an experience."[169] The Pennsylvania Station researchers merely concluded that "one operator…could milk four or more cows with the machine in less time than he could milk the same number by hand."[170]

Writers at other stations waxed more enthusiastic, though no consensus emerged about the number of animals that would have to be milked to make the purchase of a machine profitable. Oscar Erf, of the Kansas Station, thought the machines best suited for larger operations: "It would appear that the milking machine is fitted for large herds rather than small ones, and we believe it would be impracticable to install them where fewer than twenty cows are milked the year round."[171] Researchers at the USDA Bureau of Animal industry cited a smaller number of animals: "There seems to be no good reason why a dairyman with a herd of even 10 or 12 cows could not use a machine with

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[169] W.A. Stocking, Jr. and C.J. Mason, "Milking Machines," *Storrs Agricultural Experiment Station Bulletin No. 47* (1907): 105.

[170] T.I. Mairs, Test of a Mechanical Milking Machine," Pennsylvania, 3.

[171] Oscar Erf, "Milking Machines," *Kansas State Agricultural College Agricultural Experiment Station Bulletin No. 140* (1906): 67.

profit."[172] Scientists at the Wisconsin Station realized that the decision to adopt milking machines might amount to more than a simple budget decision; they alone recognized that independence from the hassles of unreliable hired help might ultimately decide the issue:

> "It is evident that from a financial point of view alone the number of dairy farmers who will buy a milking-machine will be limited to those owning fairly large sized herds; but the difficulty of securing efficient help for milking and the uncertainty of the work done by ordinary milkers, will render it desirable, in the case of many farmers owning small herds, to be able to attend to the milking personally by use of the machine and thus become independent of hired help in running their dairies. It is therefore, clearly impossible to lay down any absolute rule as to the smallest size for a herd in the case of which it will be advisable to adopt machine-milking."[173]

In the end, then, the decision to purchase milking-machines hinged on more than mere profitability. Because they ameliorated what many hired men considered an undesirable chore, the adoption of machines could make it easier to attract and retain dairy-hands. On the other hand, the purchase of machines could free dairymen from the vagaries of employing outside help. At this time – before legislation would require that farmers who wished to sell their milk for liquid consumption adopt milking machines and eventually, in most states, a pipeline that carried the milk from the cow directly to the storage tank - the decision came down to personal choice as each farmer weighed his priorities.

In addition to studying the effect, economy, and efficiency of milking machines, scientists at the state Experiment stations studied the hygienic aspects of the new devices.

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[172] C.B. Lane and W.A. Stocking, "The Milking Machine as a Factor in Dairying," *USDA Bureau of Animal Industry Bulletin No. 92* (1907): 29.

[173] F.W. Woll and G.C. Humphrey: "Milking Machine Experiments," *University of Wisconsin Agricultural Experiment Station Bulletin No. 173* (1909): 25

Researchers ultimately found that, when properly cleaned, milking machines could produce milk that was at least as sanitary as the produced by hand milking. In fact, scientists at the Wisconsin Station felt confident to declare that: "It was [also] found that under the conditions of these trials, the milking-machine produced milk with a slightly lower bacterial content than that drawn by hand…"[174] Their colleagues at the Kansas station concurred: "Machine milking is cleaner than hand milking" Dairymen at both stations based their conclusions on tests that compared the amount of bacteria in hand-milked samples with machine-milked sample. [175]

However, researchers at Nebraska and Storrs-Connecticut experienced difficulty in obtaining sanitary milk; their early tests showed the machine-drawn milk obtained at their stations contained vastly higher – in some cases by several orders of magnitude – amounts of bacteria then hand-drawn milk obtained at the same time under identical conditions. Scientists at both stations began a series of trials to determine whether the problem was systemic to the new machines – that is, that milking-machines inherently produced more contaminated milk – or was the result of imperfect sanitizing methods. For example, researchers at Nebraska, having obtained unsatisfactory cleanliness following the directions supplied by the manufacturer of the machine – incidentally, the same machine and (presumably) the same instructions employed at three other experiment stations without hygiene problems–began to experiment with alternative cleaning methods. Employing hot water and soda exacerbated the problem, but boiling

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[174] Ibid., 26.

[175] Oscar Erf: "Milking Machines," *Kansas State Agricultural College Agricultural Experiment Station Bulletin No. 140* (1906): 62.

the parts for twenty minutes and then soaking them in limewater produced excellent results. Having found a successful combination of cleaning agents, the researchers then varied the boiling time; boiling the apparatus for twenty minutes proved to be the most effective measure. The scientists at the Nebraska Station concluded that: "These experiments clearly show the magnitude of the milking machine problem from the standpoint of sanitary milk production, and clearly indicate that if pure, wholesome milk is to be obtained more than ordinary care must be exercised in washing and cleaning the parts of the milkers."[176]

Though the Nebraskans appreciated the value of "pure, wholesome milk," W.A. Stocking, Jr., during his tenure with the Storrs-Connecticut Station, combined a zeal for research into milk hygiene and sanitation with an approach, or perhaps more specifically, a mindset, that marked a break not only from the past but from his contemporaries. Stocking thought that the production of sanitary milk that could be sold at a premium would form the basis of profitable dairying. To this end he authored or co-authored a series of Experiment Station Bulletins that examined various sanitation aspects of dairying. Beginning in 1903 he began an ongoing investigation of covered milk pails in an effort to improve the hygiene of milk; the Storrs Station published two bulletins that detailed his findings. He also published the results of his studies of the "germicidal property" of milk.[177]

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[176] Haecker and Little, 64.

[177] Stocking resigned his position at the Storrs-Connecticut in 1906 to take a professorship at Cornell University in New York. However, the articles discussed in the present chapter, which appeared between 1903 and 1907, all appeared under the auspices of the Storrs Station, save the USDA publication, which also noted his affiliation with

Stocking's publications reveal that he was a cautious researcher who approached his investigations in methodical fashion. His experiments with sanitizing milking machines characterize his methodology. To compare the cleanliness of hand and machine milking he began by washing both the milking machines and the buckets used in hand-milking with soap and warm water. The milk extracted by hand proved to contain far fewer bacteria. Next, he followed the same procedure, but finished by rinsing the apparatus in a solution of gold-dust. To his surprise, the bacteria contained in the hand-drawn milk remained constant, but the bacteria count of the machine milk doubled. He then tried sterilizing the metal parts of both milking machine and milk pails in steam, and soaking the rubber components of the milking machine in a solution of formalin; bacteria counts for both methods declined somewhat. Despite this promising result, Stocking discontinued this line of investigation: "In view of the fact that the use of formalin in connection with milk is prohibited by law in some places and is usually seriously objected to by health officers, it seemed desirable to find some other method for sterilizing the machines…"[178]

Stocking began to employ increasingly aggressive techniques in his hygienic quest. Sterilizing all the parts of the milking machine with steam proved ineffectual, and in addition harmed the rubber components. Soaking the various apparatus in a salt

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Storrs. W.A. Stocking, Jr.: "The Covered Pail a Factor in Sanitary Milk Production," *Storrs Agricultural Experiment Station Bulletin No. 25* (1903); "Comparative Studies with Covered Milk Pails," *Agricultural Experiment Station Bulletin No. 48* (1907); W.A. Stocking, Jr,: "The So-called 'Germicidal Property' of Milk," *Storrs Agricultural Experiment Station Bulletin No. 37* (1905).

[178] W.A. Stocking, Jr. and C.J. Mason, "Milking Machines, Part I. Effect on Quality of Milk," *Storrs Agricultural Experiment Station Bulletin No. 47* (1907) 105-113. Quote from page 113.

solution produced high bacteria counts regardless of the manner of milking. Borax also proved an ineffectual cleaner. At this point, Stocking returned to the use of formalin. Despite the legal and health issues associated with its use, it had proved the most effective cleaning agent; furthermore, its use did not harm the rubber components of the milking machine. He increased the concentration of formalin in his sterilizing solution from two and one-half to three and one-half percent. Though bacteria levels dropped, hand-drawn milk continued to contain fewer bacteria than machine-produced milk.[179]

The results perplexed Stocking: "It seemed to the writer that the milk drawn through the machines when they were thoroughly sterilized should contain a very much smaller number of bacteria than milk drawn into the covered pail which was used in these experiments…" In fact, the opposite proved true: "Contrary to expectation the germ content of the machine drawn milk continued to be higher than that of the hand drawn milk." Finally sensing that contaminants must be entering the system from another source, Stocking placed a cotton filter over the air intake on the milking machine. This solved the problem: the bacteria counts plummeted to the lowest levels Stocking had yet recorded, cleaner even than the most hygienic hand-drawn milk he had produced. Ever cautious, he repeated the experiment without the filters, and the bacteria counts soared. The use of formalin in conjunction with cotton air-filters produced exceptionally clean milk. Happily, Stocking found that by "thoroughly rinsing the tubes just before use the formalin is so completely removed that no trace it could found in the milk, even by the most sensitive chemical test."[180]

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[179] Ibid.

It would be easy to dismiss Stocking's adventure as a sort of scientific tragicomedy, but to do so is to ignore the historical significance of his work. First, Stocking's methods, if not his tenacity, seem the rule rather than the exception. Stocking and his contemporaries typically went about their business with a single-minded approach. For Stocking, to do science, and to be a scientist, was to measure bacteria and find ways to make that number smaller. For others, it meant producing a "balanced" feed ration or determining whether supplying warm or cold water to animals resulted in the production of more milk. On one level, Stocking's example seems humorous, but on another, it shows a remarkable dedication. As will be shown in later chapters, not all experiments proved as successful as Stocking's. Eventually, scientists encountered problems that required the consideration of more than one element, or had to make decisions about what aspect of a problem seemed more important. Put another way, the method and mindset of Stocking and his contemporaries worked well if one wanted to study problem "a" OR problem "b," but broke down when one wanted to study "a" and "b" at the same time. Ultimately, scientists devised ways to deal with these sorts of dilemmas, but doing so meant adopting new mindsets and new approaches.

Stocking's case also merits consideration because he, unlike - or perhaps more fairly, much more clearly than - his contemporaries, realized that measuring bacteria represented the means toward a desirable end rather than an end in-and-of itself. So long as milk represented something to analyzed, it did not ultimately matter how much bacteria it contained. Stocking understood that the aim of all his experiments was to produce clean milk fit for human consumption; in his words, "milk must reach the

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[180] Ibid., 113-114, 128.

consumer in as nearly as possible the condition in which it leave the udder of the healthy cow." Furthermore, as "the importance of importance of pure food becomes more widely disseminated among milk users this demand for a cleaner and more wholesome grade of milk will steadily increase." Unfortunately, "few dairymen are now producing a high grade of milk under…conditions that reduce…contamination to a minimum."[181]

Stocking believed that, at least in the short term, a milking machine that could assist farmers in producing hygienic milk could prove a boon for farmers, as clean milk "demands a price considerably above the market price." His experiments revealed that when properly cared for milking machines could produce very sanitary milk, while his colleagues at other stations at least intimated that machines might under the proper conditions increase the profitability of the dairy farm. Therefore, the adoption of milking-machines could prove beneficial to both dairymen and producers: the former would make higher profits, while the latter would enjoy cleaner, healthier milk.[182]

Ultimately, however, Stocking hoped that his research would benefit the public, not the dairyman. The goal of his research was not to help the farmer better his fortune but to solve "the problem of supplying the public with clean, wholesome milk." On some level this seems a curious attitude a scientists employed at an agricultural experiment station. Historian Alan I Marcus had written extensively about the tension that often existed between farmers and Station scientists, and the example of Stocking illustrates that this animosity was not put to rest in the nineteenth century but lingered, in varying

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[181] C.B. Lane and W.A. Stocking, "The Milking Machine as a Factor in Dairying," *USDA Bureau of Animal Industry Bulletin No. 92* (1907): 33.

[182] Ibid.

forms, into the twentieth century. More importantly, Stocking's concern with public health proved prescient: eventually, state and federal health laws, rather than the possibility of increased profits, would mandate that farmers employed milking machines. Stocking's researches also proved prophetic in the shorter term: after the initial flurry of interest in all aspects of milking machines described in this chapter, the vast bulk of the literature on the machines produced by the experiment stations studied problems of hygiene and sanitation, not the practicality of their adoption.[183]

In the end, a historical examination of the ways that scientists at the State Agricultural Experiment Stations evaluated milking machines helps in clarifying an understanding of both early nineteenth century attitudes about science and how it dealt with the introduction of new technologies. Conventional wisdom, and at least some conventional history, tends to focus on technological innovations as turning points whose introduction not only changed the world by virtue of their function – i.e. that cars allowed people to travel faster – but also because they rather quickly changed the way that contemporizes viewed their world – for example, that the introduction of the automobile blurred distinctions between city and country.

Instead of confirming this viewpoint, this study of the adoption of milking machines suggests that, at least some cases, contemporaries did not necessarily attribute the same importance to innovations as did later generations who enjoyed the ultimate benefits of that advance. Scientists did not react to the introduction of the milking

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[183] See Alan I Marcus, *Agricultural Science and the Quest for Legitimacy (*Ames, IA: Iowa State University Press, 1985. See also John L. Shover, *First Majority – Last Minority: The Transformation of Rural Life in America* (De Kalb, IL: Northern Illinois University Press, 1976).

machine by discarding accepted theories and practice. Instead, they asked the same – to them, important – questions that had occupied them before the appearance of the new invention. Researchers at the agricultural experiment stations did not consider the milking machine on its own terms; instead, trials tended to follow established scientific techniques. For example, they investigated the speed of the milking machine just as they investigated the speed of newly proposed ways of hand milkings. They measured the bacterial content of machine-produced milk just as they measured the cleanliness of covered versus uncovered milking pails. They measured the economic benefits of the machine not as a machine but as a new sort of dairy-hand that could somehow milk multiple cattle at the same time that.

That these scientists and researchers clung to traditional practices while acknowledging the (mostly) successful application of the new machines is at least as striking. The authors of the bulletins considered in this chapter occasionally hinted that to fully exploit the new technology might entail new attitudes about all of the factors in the system. As noted above, some scientists suggested that the successful adoption of milking machines would require breeding, or at least choosing, a new sort of dairy cow – one who not only accepted the new machine temperamentally, but also possessed an anatomy the facilitated the application and use of the new machine.

The Station scientists also intimated, though usually in oblique terms, that the adoption of milking machines would require a new sort of dairyman. Because the mechanical milking machine was, at heart, "mechanical," the successful user of these machines would have to understand both animals and machines. Oscar Erf, who authored the first Experiment Station bulletin to consider milking machines, realized the importance of this change: “It is extremely necessary for the man in charge to fully understand how to operate the machine” No longer was an understanding of dairy cattle the sole pre-requisite to successful dairying; the new dairyman also had to possess a certain mechanical aptitude. [184]

Striking too is the fact that contemporary researchers suggested that humans and animals change, not the milking machines themselves or, for that matter, any other aspect of the dairy farm. Though scientists hoped that someone would develop longer lasting, easier to clean rubber components, or more reliable engines to power the machines, they never suggested, let alone demanded, any real alterations to the machines or the farm. They did not suggest a pipeline that would carry the milk to a central tank, or the rearrangement of the dairy-barns architecture that would facilitate the possibilities of the machine. Instead, they recommended that dairymen select or breed cattle that would accept the machine, and that they hire farmhands that were mechanically inclined.

All of these factors tend to indicate that agricultural scientists were essentially conservative in their approach. When evaluating the new devices they did not start with a clean slate. Instead, they evaluated the machines by using accepted techniques to determine how well the devices met established criteria. Though milking machines would eventually comprise an integral part of the “modern” dairy farm, this transformation could not occur until scientists adopted a new mindset that allowed them to envision the dairy in an entirely new way.

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[184] Oscar Erf, “Milking Machines,” *Kansas State Agricultural College Agricultural Experiment Station Bulletin No. 140* (1906): 67.

CHAPTER 6

BREEDING A BETTER COW

Feeding and breeding represented the two investigative paths followed by most researchers. As we have seen, dairy scientists interested in feeding attempted to maximize the production of existing animals. They did so in a variety of ways, but common to all was a belief that establishing animal nutrition on a "rational" or "scientific" basis would allow farmers to enhance their profits by producing more milk and butter-fat at lower cost. Most researchers maintained that the chemical analysis of cattle foodstuffs represented the best way to achieve these goals; they went about their work by isolating various feeds, measuring the digestible nutrients in each, and compounding rations that they believed would offer the best results. The hallmark of these methods was quantification, or the belief that measurement held the key both to understanding the processes of animal nutrition and the establishment of "better" – i.e. cheaper and/or more productive – systems of feeding.

While nutritionists sought to maximize the productivity of existing animals, those interested in breeding endeavored to improve the quality of the nation's dairy stock: the first group hoped to produce better feeds; the second, better animals. Scientists interested in animal breeding set themselves three main goals. First, they hoped to determine how to best select cattle that would produce not necessarily the most milk but, more importantly, the most milk at the lowest cost. Second, having selected profitable cattle, researchers hoped to develop breeding plans that would allow dairy farmers to breed more productive animals; in theory, they believed that each generation of cattle should represent an improvement on the last. Third, a small group of geneticists hoped to understand the genetic basis of animal breeding; these scientists believed that unlocking the secrets of genetics offered the best hope of improving the quality of America's dairy cattle.

This chapter will focus on the efforts of the first two groups: those interested in the selection and breeding of dairy cattle. This decision was made for a number of reasons. Most importantly, scientists interested in selection and breeding represented the vast majority of researchers working along these lines; those interested in the genetic basis of animal reproduction amounted to a small minority. Moreover, despite the occasional appearance popular texts that aimed to explain current understanding of genetics, these tomes offered little practical advice to dairy farmers who were, like the majority of researchers, less interested in understanding the mechanics of genetics than they were in learning how to select and breed more productive cattle. My choice to study the first two groups should not imply that the work of geneticists was unimportant; indeed, their efforts formed basis for the work of later scientists like Sewall Wright and Jay Lush. As such, their contributions will considered at greater length in chapter eight, which places their work in a more proper context.

Investigations into improving the quality of dairy stock took two forms: selection and breeding. Scientists interested in selection hoped to determine best to identify profitable animals, while those interested in breeding attempted to establish how to apply these findings in order to produce future generations of superior animals. These two tasks went hand-in-hand, and researchers often investigated both questions; after all, only by identifying animals with superior characteristics could one hope to improve the quality of stock.

Scientists investigated, and advocated, two distinct methods of selecting cattle. Researchers insisted that production records that recorded not only the milk and fat yield of cattle but also the amount of feed that they consumed represented the only truly reliable means to determine an animal's fiscal worth. Compiling the animal's production – her income – and her feed costs would afford farmers important benefits. Not only would keeping accurate records allow farmers to identify profitable animals, but it would also allow them to ascertain which animals actually lost money. Scientists maintained that simply culling unprofitable animals would result in significant financial gains for dairy farmers. As importantly, such records formed the basis of finding the breeding value of bulls. By comparing the production records of the daughters of a particular sire with that of their dams, dairymen could identify bulls that passed on superior production.

But, while scientists emphasized the importance of record keeping, they also considered the appearance of the animal of great significance. In the absence of records the evaluation of an animal's appearance represented the only way that farmers could estimate an animal's worth. However, even when production records were available, many researchers insisted on the importance of an animal's physical characteristics. Most scientists believed a strong correlation existed between an animal's production and her outward conformity to in ideal dairy type. While they acknowledged "ugly" or "undairy-like" cattle might well produce large amounts of milk at low cost, they also maintained that such animals were, in most cases, genetic "flukes" that would, in all likelihood, fail to pass their productivity to their offspring. An animal's appearance, then, confirmed an animal's value not only as a productive milker in her own right, but of her capacity to produce superior offspring provided, of course, that she was bred to a quality mate.

Proponents of both production and "type" (or appearance) testing – and the majority of researchers admitted the usefulness and necessity of both methods – employed the same methodology as their colleagues investigating other aspects of the dairy; that is, they believed they could make progress by isolating and quantifying the various properties they wished to measure. In the case of production testing this proved a rather straightforward task. Simply measuring the amount of milk produced by an animal produced the necessary results, and, after Babcock's development of a practical test, farmers could readily the amounts of both milk and fat yielded by animals.

Quantifying an animal's appearance proved a more daunting challenge. While most dairy experts could readily identify a superior animal – and could agree with their peers about the qualities that set her apart from other cows – they had a much more difficult time devising a test that judged – and allowed for easy comparison between – less than ideal specimens. Though researchers eventually developed methods that allowed them to quantify various properties of the animal, measuring intangibles such as the "dairy quality" of an animal required scientists to adopt some rather hackneyed measures that retained a certain – though in the eyes of researchers acceptable - degree of subjectivity.

These ambiguities especially manifested themselves when researchers attempted to apply their findings to actual breeding. Most experts maintained that the key to successful breeding lay in the proper selection of the bull. To this end they spent considerable effort trying to establish correlations between the appearance of the bull and the performance of his offspring. This proved a difficult task: it required at least two years for a bull to mature and at least three more before the first daughters he might sire would themselves give birth and start producing milk.

Unfortunately, scientists could not discover fail-proof methods of choosing bulls that would produce quality offspring. Instead, because of the difficulty both in terms of time and cost to find exceptional sires, scientists spent a great deal of time evaluating various breeding schemes that would maximize the influence of any particularly valuable bull. To this end researchers paid special attention to various methods of inbreeding and, especially, to how much use could be made of one sire before the negative effects of inbreeding outweighed the improvements that could be made.

The cases of selection and breeding demonstrate how scientists pushed the boundaries of their preferred methodology. Though researchers in other fields applied the techniques of isolation and quantification with good results, experts who attempted to apply these methods to the problems of breeding and selection met with less success. Their example demonstrates the practical limits of their methodology, and helps to explain why later researchers searched for and eventually developed alternate methods and techniques. Despite their relative lack of success, however, their willingness to apply accepted methods demonstrates both their commitment to a certain set of techniques as well as their commitment to establishing all aspects of the dairy on a rational, scientific basis.

Selecting animals by their physical appearance presumably started well before the advent of writing with the domestication of wild beasts. In any event, cattle were among the first animals tamed, and the Babylonians, Egyptians, and Indians all left descriptions of their cattle. So did the Greeks and Romans, as well as a number of medieval scribes. Marco Polo, too, noted the bovines he encountered in Asia, calling them "well-sized, fat and exceedingly handsome." In the absence of written records that recorded the amount of milk produced by an animal farmers were forced to rely on visual assessments of cattle both for purposes of selecting which animals they might purchase as well as to determine how best to mate their cattle.[185]

Despite millennia of breeding – including active efforts to produce better cattle – authors wrote little about the criteria of cattle selection before the 19th century; the written documents that remain discuss primarily beef, rather than dairy, cattle. Spurred by growing interest in dairy animals, by the early 19th century a number of Scottish and English writers began to consider how to select, and then produce, more desirable animals. These authors introduced two concepts – "conformation" and "dairy type" – that later formed the basis for the work of American dairy researchers interested in cattle selection. In 1811 the Scottish author Alton was the first to write about the notion of "conformation" in dairy cattle in his *Survey of Ayrshire*. Conformation – the notion that cows should be judged and selected according to how close they approached some established ideal – represented an important conceptual shift because it required dairy farmers and breeders to come together and establish the characteristics of the "ideal"

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[185] T.R. Pirtle, *History of the Dairy Industry* (Chicago: Mojonner Bros. Co. 1926), 1-5, quote from p. 5.

animal. Moreover, conformation testing, by its nature, relied on the existence of pure-bred cattle that could be expected to conform to the desired type. In other words, a Holstein cow could not be judged by the same criteria as a Jersey, or vice versa. Hence the development of conformation and the establishment of breed associations went hand-in-hand.[186]

The second important concept introduced by British cattle breeders was the idea of "dairy type." In 1829 William Harley published *The Harleian Dairy System*. In that work Harley became the first author – in English, at any rate – to explicitly suggest that dairy animals should be selected on a distinctly different basis than beef animals. While beef producers desired bulky, block-like animals, Harley maintained that dairy cattle should possess "thin shoulders and large, broad hindquarters." This conception of the cow became popular in both Britain and America, and American scientists would make extensive use of the notion in their work.[187]

American dairy researchers working at state agricultural experiment stations and agricultural colleges devoted considerable attention to the selection of cattle. While virtually all recognized the importance of production testing, they did not neglect to investigate the merits and methods of selecting animals based on their physical appearance. Between 1880 and 1920 authors associated with these institutions produced dozens of bulletins and a handful of texts devoted solely to the topic, and virtually every book on dairying contained a lengthy consideration of judging. Moreover, most

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[186] Charles S. Plum,: *Judging Farm Animals* (New York: Orange Judd Co., 1916), 272.

[187] Ibid.

agricultural colleges included animal judging in their curriculum, and often offered a separate class in judging.

Even the most tepid advocates of selection testing – usually those who most stridently recommended that keeping accurate records of milk and fat production offered the most reliable means of improving dairy stock – admitted that type-testing, or judging animals based on their appearance and/or their conformity to some ideal standard, often offered the only real basis by which to judge cattle. Clarence H. Eckles, Professor of Dairy Husbandry at the University of Missouri and author of one of the most widely distributed dairy texts, commented "The only satisfactory way to select the profitable from the unprofitable in a herd of dairy cows is by keeping records of the amount of milk produced and testing for butter fat." However, he continued, "In case an animal is to be purchased for which no records have been kept, the buyer must depend mostly upon the evidence of dairy characteristics as shown by the animal." Eckles, a firm believer in production testing, even advised "it will be well to depend on" the appearance of an animal "rather than attempt to select by weighing and testing the milk for a single milking, or even an entire day."[188] Henry Jackson Waters, President of the Kansas State Agricultural College, echoed these sentiments. While acknowledging that production testing "is the only reliable way of selecting profitable cows," and that "a cow may rank very high according the score card and still not be a very profitable producer," nonetheless "A dairy herd may be chosen in accordance with the type, or conformation, of the cows." Because "only a small per cent of the cows have records…This has let to a

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[188] Clarence H. Eckles, *Dairy Cattle and Milk Production* (New York: The Macmillan Co., 1911), 132-133.

study of the type of cow best suited to milk production and a definite system of judging dairy cattle."[189]

Proponents of type-testing, on the other hand, maintained that form was at least as, and possibly more, important than function. Charles S. Plumb, Professor of Animal Husbandry at the Ohio State University, posited that the fact that the highest producing animals almost universally descended from pure-bred stock confirmed the primacy of form: "Each breed has reached its present status of importance and perfection, through the efforts of certain breeders who have persistently sought to develop a conformation that in their judgment indicated within reasonable bounds superior producing capacity." Though Plumb acknowledged "Occasionally some one comes forward with a criticism of accepted standards, with the argument that a certain animal not representative of the approved type, was a producer of large capacity, therefore the type should not be a guide," but eschewed such assertions: "An odd case here and there should not weigh heavily against the cumulative experience and observation of the great mass of breeders."[190]

Andrew M. Soule, Professor of Agriculture at the Tennessee Agricultural Experiment Station, also proclaimed the importance of conformation in no uncertain terms: "without ideals stock raising can not be made a success, and this is one reason it has not often been more successfully pursued in the south." While admitting that "conformation does not absolutely measure utility," Soule believed "it strongly indicates

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[189] Henry Jackson Waters, *The Essentials of Agriculture* (New York: Ginn and Co., 1915), 360-361.

[190] Plumb, 4.

the merits and defects of animals, and can be relied on to a remarkable degree." Moreover, conformation not only acted as an assurance of production, but could reveal hidden flaws: "From a critical examination of the exterior points of an animal a fairly correct estimate can always be made of the quality of the interior or hidden parts." Careful examination could even reveal the presence of diseases: "Conformation enables the discernment of hidden diseases, such as tuberculosis or other of a scrofulous nature. These are questions of more than passing importance, for it often happens that ignorance of a conformation indicating tuberculosis results in the purchase and introduction of animals into herds and flocks with disastrous results."[191]

Clearly then, some dairy scientists took the issue of cattle judging quite seriously, believing that it represented a crucial factor in the success of American dairies, a fact demonstrated at least in part by the number of bulletins and texts designed to impart a knowledge of animal judging to their readers. Proponents of production testing, who often bemoaned the fact that few farmers kept records, and even fewer kept accurate, useful logs, admitted – albeit sometimes grudgingly – that judging dairy cattle by appearance and conformation offered at least minimal assurances about the productive capacity of animals. On the other hand, advocates of type testing usually readily conceded the importance of production testing but maintained that reliance on production testing alone might well lead to deterioration of cattle in the future if breeders were to ignore the physical characteristics that (they believed) made such production possible.

[191] Andrew M. Soule, "Conformation of Beef and Dairy Cattle," *USDA Farmers' Bulletin No. 143* (1902): 7-9.

The challenge that exponents of type testing faced stemmed from the difficulty of establishing cattle judging on a scientific basis. Supporters of production testing encountered little difficulty on this score. Like their peers, they believed that isolation and quantification represented the hallmarks of "good" science; production testing – judging cattle by the amount of milk and fat they produced – was, de facto, scientific testing. Though, as will be discussed, researchers disagreed about how, how often, and when to test the production of cattle, they faced a different sort of challenge than did their colleagues who stressed the merits of type testing. "Production testers" had to decide how best to collect and interpret data; type-testing advocates faced the more daunting task of determining how to quantify what were, at heart, subjective aesthetic decisions in a manner that would not only produce meaningful results but could be practically applied by breeders.

Supporters of type-testing found the answer in the form of the score-card. By all accounts the use of the score-card originated on the Channel Island of Jersey. In 1833 the cattle breeders of the island, concerned about maintaining and improving both the appearance and production of their cattle, met to determine how to best achieve these goals. It was decided that this could most easily be accomplished by establishing the properties of an "ideal" Jersey cow and using this as a standard by which to breed future generations. The breeders formed a committee charged with this task. The members of the committee traveled the island observing animals and finally selected two cows – in fact, the front half of one animal and the rear half of the other - that they felt best represented the breed ideal. Using these animals the committed created a composite "ideal" animal. They then devised a score card that enumerated and ranked these qualities. The card employed a twenty-seven point scale that assigned different weights or values to the various constituent parts of the animal: the head, horns, and ears of the animal counted for eight points; the ancestry of the animal for four; the udder and teats for four, etc. The use of the card minimized, though it certainly did not eliminate, the subjective elements of cattle judging. In effect, the score card employed the same methodology employed by scientists. Rather than consider the cow – or other object of study – as a whole, this system isolated various aspects of the animal and then assigned a numerical ranking to each component.[192]

The use of scorecards proliferated in the closing decades of the nineteenth and first decades of the twentieth centuries. This was in part due to the efforts of the various pure-breed cattle associations: the Holstein-Friesian Association of America, the American Jersey Cattle Club, etc. Each of these organizations produced score cards to rank cattle registered by the association. Though similar – each employed a hundred point scale and was organized so that one judged cattle beginning with the head and ending with the udder – the cards differed in the relative weight they allotted different aspects of the cattle, "apparently," according to one text, to "emphasize points in which breed is likely to be deficient. An example of this is the large number of points given to the fore udder in the Jersey score card."[193] In addition, a number of texts as well as state

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[192] Ibid.

[193] C.H. Eckles and G.F. Warren, *Dairy Farming* (New York: The Macmillan Company, 1916), 54.

experiment station bulletins issued their own "universal" score cards which could be used to judge dairy cattle of any breed.[194]

The score card also enjoyed wide popularity among proponents of type-testing, who viewed the use of cards as especially helpful for beginners. According to Eckles and Warren, authors of one of the most popular dairy texts, "The use of the score card is an advantage to the beginner as a means of impressing the points to be taken into account and their relative importance. It helps make the examination systematic and prevents one from forgetting points that should be observed."[195] Charles S. Plumb, author of a popular judging text, reiterated the sentiment: "The value of the score card lesson largely lies in teaching the beginner the location of the various parts and how to study them by a logical, well established system."[196]

Though the use of score cards allowed judges to more easily systematize and rationalize their work, they still allowed wide latitude to the subjectivity of the person making the evaluation. The score card employed by the American Jersey Cattle Club, for instance, devoted ten of the total one-hundred point to the "general appearance" of the animal, which the score card defined as "A symmetrical balancing of all the parts, and a proportioning of parts to each other…with the general appearance of a high-class animal, with capacity for food and productiveness at pail." The Ayrshire Breeders' Association

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[194] For examples, see Ibid., 290-296, which reprints the score-cards from a number of breed associations and makes for easy comparison between the various examples.

[195] Ibid., 56.

[196] Plumb, 20-21.

devoted points to "style," the Guernsey Cattle Club to "Dairy Temperament" and the Holstein-Friesian Association to a "Decided feminine…appearance."[197]

Thus the use of score cards amounted to something of a canard: while giving the appearance of objectivity, the still required the judge to make a wide range of subjective decisions. However, judging texts and bulletins issued by state experiment stations took pains to teach would-be judges "scientific" methods for evaluating the "symmetry" and "femininity" of dairy cattle. Most commonly, texts advised that beginning judges would be wise to mentally picture the dairy cow as consisting of a number of "wedges," or planes, which would allow students to correctly gauge the animal's conformation to an ideal standard. According to one bulletin, "The general angularity of the cow gives her what is known as the wedge conformation which is very evident in the typical dairy cow. This conformation outlines distinctly three wedges." The first wedge was apparent when viewing the animal from the side, and consisted of a base extending "from the hips to the lower extremity of the udder…and the apex…of the wedge at the head." The second edge evaluated the animal from above; its base consisted of a line drawn between the hips with its apex at the withers. The third wedge was apparent when viewing the animal head on; its base was formed by the "wide floor of the chest," and its "apex by the withers."[198]

It remains unclear who first postulated the use of "wedges" as evaluation aids, though Plumb cites an 1875 monograph that explicitly mentioned their use: "in the dairy

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[197] Eckles and Warren, 290-296.

[198] "Live Stock Judging for Beginners," *Purdue University Agricultural Experiment Station Circular No. 29* (1911): 59-60.

breeds…there is a tendency toward accumulation of a larger part of the weight of the animal in the rearmost half…As judged by a side view or from above, there is a certain wedge form…”[199] Regardless of the origin, judging texts almost universally employed the wedge system in teaching judging. These works maintained that the use of wedges best allowed beginners to judge an animal's conformation to the dairy ideal which held that cattle, whose sole purpose was to produce milk and offspring, should only accumulate size in those areas necessary for production – i.e. a wide chest that allowed plenty of space for lungs, digestion, and reproduction, and a deep lower chest that allowed space for the milk-veins and provided a firm attachment of the udder.

Though judging cattle by appearance enjoyed a wide popularity, and, in the absence of records, amounted to the only means available to evaluate cattle – and, for this reason, was grudgingly recognized by advocates of production testing – most experts realized that judging animals by their appearance alone largely ignored the basic purpose of raising dairy cattle: the production of milk and fat. Judging by type enjoyed much more popularity among those who raised other animals, and especially breeders of beef cattle and hogs, where the only alternative to visual evaluation of the animal – besides weighing the creature – lay in butchering the creature.

Fortunately, dairy farmers had another option: production testing. Even proponents of type-testing admitted that the ultimate economic value of dairy cattle stemmed from their production of milk and the quality of the offspring they produced. Production testing, facilitated by the development of the Babcock test which allowed farmers to test the fat quantity of the milk on the farm, soon supplanted type-testing as

[199] Plumb, 253.

the test of choice of most dairy experts. In his *History of the Dairy*, T.R. Pirtle dated the origins of production testing to the organization of the first cow-testing association in the United States in 1905. While the establishment of such communal organizations, in which a group of roughly two-dozen farmers collectively employed a tester who would visit each association herd once a month and collect and tabulate records of food consumption and milk production, certainly played a major role in the widespread adoption of production testing, and will be considered in due course, the actual origins of production testing occurred some years earlier. One sign of this can be seen in the fact that early advertisements for the Babcock apparatus, introduced in the 1890's, trumpeted the fact that by using the device farmers could personally test their cattle.

The Wisconsin Experiment Station published the earliest recorded tests performed by American station scientists in its Bulletin No. 10, "Tests of Dairy Cows," in October 1886. Like all such trials, station workers measured and recorded the amount of milk produced by the test group; in lieu of a practical butter-fat test, they also recorded the amount of butter produced, though the introduction of the Babcock test in the 1890's would make milk-fat the standard measure in later trials.[200]

Other stations, particularly those located in the traditional dairying states, quickly followed suit and conducted their own tests. The New York Agricultural Station at Geneva published the results of their trials 1889, the Vermont Station in 1889 and 1890, Illinois in 1894, Colorado in 1896, and New Hampshire in 1897. In addition, fair-goers at the Chicago Columbus Exposition in 1893 could observe trials that lasted the length of the exhibition; prizes were awarded to the cattle that produced the most milk and the

[200] "Tests of Dairy Cows," *Wisconsin Experiment Station Bulletin No. 10* (1886).

most butter at the fair. Through the 1890's state fairs throughout the country also held competitions that rewarded the most prolific producing cattle.

However, scientists understood that the importance of these trials lay not in attracting fair-goers but in promoting the importance of accurate recordkeeping that would identify unprofitable animals. Therefore they turned their attention to the promotion of regular, accurate record keeping as an everyday practice on the farm; they argued that production testing's true worth lay not in distinguishing between high producing animals but as an everyday technique that could – and should – be employed on entire herds. E.H. Farrington of the Wisconsin Station made this quite clear in the Wisconsin Station's Bulletin No. 75, "Testing Cows on the Farm," published in 1899: "The production of each cow is therefore of importance not only to the manufacturer and producer, but should be to the cow herself, for her life should depend on the amount and economy of her production."[201]

Virtually every author writing on the subject of animal breeding over the next two decades stressed the importance of production-testing and accurate record keeping. Clarence H. Eckles, Professor of Dairy Husbandry at the University of Missouri, believed that such methods offered the best assurance of obtaining and retaining profitable cattle. He cited the results of tests performed by researchers at a number of agricultural experiment stations. Despite the fact that tested herds "were in the hands of men who were making the production of milk their principle business," the tests showed that "at

[201] E.H. Farrington, Testing Cows on the Farm," *Wisconsin Agricultural Experiment Station Bulletin No. 75* (1899): 3.

least one third of the cows in the ordinary herds…were unprofitable."[202] Eckles placed the blame on the farmers, noting "the failure on the part of a large number to appreciate the importance…of individual selection." Frank D. Gardner, Professor of Agronomy at Penn State, echoed these sentiments: "While a breeder can select cows by the eye for many good and desirable points, the only real test of a dairy cow is the record of her milk and butter-fat yield,"[203] as did Clarence Lane, Assistant Chief of the Dairy Division in the Bureau of Animal Industry: "Records of the performances of dairy cows form the only accurate and safe basis for judging their value. No person is able go into a good-sized herd and pick out all of the best cows by examination."[204]

Though these authors wrote in private publications not issued by the various state and federal agricultural agencies, these organizations also recommended production testing. Dozens of agricultural station bulletins, circulars, yearbooks, and other official publications urged the adoption of record-keeping and production testing. The role of governmental agriculture scientists became especially pronounced after various state experiment stations began to participate in the cooperative testing associations that began to proliferate after 1905.

The first cow-testing association seems to have been formed in Denmark in 1895. Thirteen farmers in the Vejen region collectively employed a man to test the amount of milk and milk-fat produced and the amount of feed consumed by each animal in herds

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[202] Eckles, 118.

[203] Frank D. Gardner, *Live Stock and Dairy Farming* (Philadelphia: The John C. Winston Co., 1918), 113.

[204] Clarence B. Lane, *The Business of Dairying* (New York: Orange Judd Co., 1909), 79.

belonging to the association members. The fledgling organization must have proved profitable, as in subsequent years the collectives spread throughout Denmark and the other low-countries.[205]

Helmer Rabild, a Dutch immigrant who had had experience with such cooperatives in his native country, and an employee of the Dairy Division of the Michigan Department of Agriculture, organized the first cow-testing association in the United States in 1905. The enterprise proved successful, and by 1908 dairymen in New York and Maine formed similar associations. The following year Rabild was hired by the USDA to promote the organization of cooperative testing groups and they spread rapidly: by 1910 ten states had associations, by 1920 forty states, and by 1929 all 48 states had established cooperatives.[206]

Cow-testing associations thrived in large part because they worked: farmers who enrolled reported increased production. Pirtle reported results from three herds that he regarded as typical; average annual butter production per cow increased from 237 to 305 pounds per year over a five year span.[207] A report in the 1920 Yearbook for the Department of Agriculture confirmed these findings: "the average dairy cow…produces annually about 4,000 pounds of milk and 160 pounds of butterfat" while "the average cow-testing association cow produced 5,980 pounds of milk and 246 pounds of butter-fat

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[205] For the early history see D.E. Voelker: "Dairy Herd Improvement Associations," *Journal of Dairy Science, Vol. 64, No. 6* (1981): 1269-1277. See also Pirtle 68-71.

[206] Voelker

[207] Pirtle 69-70.

a year…The average dairy cow seems to have plenty of room for improvement."[208] The same report contained an anecdote related by a farmer that, while sounding somewhat apocryphal, echoed the praises of cow-testing associations sung by farmers in the pages of Hoard's Dairymen and other dairying periodicals:

> "Last summer…I saw a fine young herd…As I stepped into the clean, well-lighted, well-built dairy barn the owner said to me: 'It's between me, these cows, and the sheriff. Because my capital is limited my cows have got to pay; if the don't the sheriff will sell me out. My cows must pay and to make sure they will I must know their individual records. That's why I belong to the cow-testing association.'"[209]

Though cow-testing associations no doubt benefited their member farmers, and USDA records proved that the animals enrolled in such associations produced, on average, more milk and butter-fat than other animals, the preceding quote illustrates the difficulties in positing a causal link between the two. While culling un-productive animals certainly raised herd and association averages, it remained –and remains – difficult to isolate how much of this increased production can be attributed solely to production-testing. For example, while the farmer in the preceding example attributed his ongoing financial solvency to his association membership, the author also noted "clean, well-lighted, and well-built barn," and therein lies the problem: it is difficult to determine how much of the improved production resulted from culling low-producing

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[208] J.C. McDowell: "Cows That Make Income Climb," *Yearbook of the Department of Agriculture, 1920*, (Washington D.C.: Govt. Printing Office, 1921), 402.

[209] Ibid., 401.

animals, and how much can be attributed to the care given the animals by farmers obviously disposed to treat their animals – and their farms – with obvious care. The fact that the above quoted farmer had good facilities – suggesting that he also cared for his cattle – and still found cow-testing useful suggests how useful accurate record keeping could be, but never-the-less still makes it difficult to attribute increased production solely to membership in cow-testing associations.

More importantly, the relative success of cow-testing associations demonstrated the widespread acceptance of quantification. Along with their colleagues who spread the gospel of nutritive analysis and the benefits of formulating rations according to a "scientific" nutritive ratio, scientists interested in cattle selection and breeding found a useful vehicle for their message in the testing collectives. Though collectives never counted a majority of farmers, nor a majority of cattle, among their members – before the Second World War association members never amounted to more the ten percent of American dairymen, a fact bemoaned by station scientists who advocated production testing as the single most effective tool available for increasing the productivity of American dairy cattle – those numbers still represented a sizable number of farmers willing to believe that quantitative analysis represented the best means to improve their herds' profitability and their own income.[210]

The scientists who advocated production testing almost universally advocated the use of pure-bred dairy cattle. They believed that the more wide-spread adoption of animals bred – "scientifically" or not – for milk production afforded American dairy farmers the opportunity to benefit from centuries of progressive breeding designed to

[210] F.J. Arnold, "Fifty Years of D.H.I.A. Work," *Journal of Dairy Science*

produce animals that maximized milk and butter-fat production while consuming a minimum of feed. C.H. Eckles claimed that “the importance of making a pure breed of some kind is more apt to be under than overestimated.”[211] George C. Humphrey concurred: “Choosing a dairy breed of cattle is fundamental…The modern improved breeds of dairy cattle are the result of high ideals, carefully laid plans and systematic effort on the part of many generations of dairymen who realized there were great possibilities in the development of breeds of cattle especially adapted for large and economical production of milk and butter-fat.”[212]

Not only did pure-bred animals generally produce larger quantities of milk and butter-fat than their un-pedigreed cousins, they also tended to pass these qualities to their offspring. According to George C. Humphrey, Professor of Animal Husbandry at the University of Wisconsin, pure-bred animals “tend to reproduce themselves from generation to generation with such marked degree of uniformity that one familiar with their history and characteristics would reject any other kind if he were engaged primarily in dairying.”[213] F.S. Putney of Penn State concurred: “pure-bred animals sell better than grade animals, as the offspring are more uniform”[214]

In addition, pure-bred animals also possessed a pedigree – a record – that allowed breeders to make informed choices about which animals to mate. Gardener noted that “The use of both pure-bred sire and dam enables the farmer to follow a more rigid system

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[211] C.H. Eckles, *Dairy Cattle and Milk Production*, 107.

[212] Humphrey 126.

[213] Gardner 126.

[214] Ibid., 111.

of selection and cull out undesirable individuals, which is not always possible in grading and cross-breeding."[215]

Though agricultural scientists sang the praises of pure-bred animals, they did not recommend one specific breed. Instead, they advised dairymen that two criteria should help them decide which breed to purchase. First, they recommended that cattlemen select a breed based on the local market. Eckles remonstration typified this advice: "As a rule a man that will make a success with one breed will be about equally successful with another."[216] Instead, he stressed that dairymen select appropriate stock for their situation: butter-makers should select Jerseys or Guernseys, milk-sellers might better opt for Holsteins. His colleagues echoed this advice and advised dairymen to select breeds appropriate their local market.

They also urged farmers to purchase breeds popular with other local dairymen. This practice promised a number of advantages. For example, buying locally acted as a check against unscrupulous sellers; not only would the buyer be aware of the seller's reputation, he could examine the animals at some length before making a purchase, and would have better access to some recourse should he purchase an inferior animal. Likewise, investing in locally popular breeds would allow the farmer better markets to sell his own surplus cattle in the future.

Most importantly, however, buying locally popular cattle afforded dairymen easier access to proven bulls. Eckles recommended that farmers "consider the matter [breed selection] from a community standpoint." He acknowledged the "importance of

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[215] Ibid., 22.

[216] Eckles 109.

community breeding;" and noted that close proximity to dairymen employing the same breed make possible the "wide use of a bull that is found to sire especially valuable animals."[217]

Scientists repeatedly stressed the importance of using proven sires. In Putney's words, "The bull is half the herd… [and] where in-breeding is practiced he is even more than half."[218] The importance of the bull was such that some scientists, including Eckles, recommended that cost-conscious farmers who could not afford to buy an entire herd of pure-bred cattle at the start could achieve much the same benefit by buying cheaper, "grade" – i.e. un-registered – animals and applying the savings to the purchase of a proven, pedigreed, bull: "Thousands of men make use of a scrub or grade sire on account of mistaken economy in cost, rather than pay a few dollars more for an animal that is almost certain to transmit desirable qualities."[219] Scientists once again stressed that the desirability of pure-bred animals lay not only in the fact that they possessed superior traits but would likely pass these traits on to their progeny.

Unfortunately, distinguishing good bulls from bad proved a difficult assignment. The true test of a sire's merit lay in the production of his offspring. Bulls reach sexual maturity at about two years of age, and most cows are 2-3 years old when they drop their first calf; thus five years might pass between the birth of a bull and the arrival of his first offspring. Many farmers balked at keeping an unproven, and, as it aged, increasingly cantankerous bull on their farms for such a length of time.

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[217] Eckles 109-110.

[218] Gardner 115-117.

[219] Eckles 115

To facilitate the use of proven bulls and increase the possibilities of discovering a truly remarkable sire[220] stations experts recommended and dairy farmers established bull associations that would facilitate the discovery of valuable sires. Organizers modeled these new organizations on the cow-testing associations: the collective would organize itself into a number of "blocks." Each block would consist of a small number of local farms that would utilize the same bull for two years. At the end of this period the blocks would rotate sires. The goal was to allow each sire to father a relatively large number of offspring in a short period of time, which would allow breeders to more quickly determine which bull's offspring, considered as a whole and not as individuals, showed the most improvement over the production of their dams. In essence, the bull-associations hastened the discovery process and spread the risk over a number of farms. Ideally, when breeders discovered an especially valuable bull they would then all share the benefits, as this animal would then be rotated several times a year between the various association farms in order to maximize the number of his potential mates. The bull-testing associations proved somewhat popular: the first was formed in 1908, and by 1925 some 220 such organizations had been incorporated.

Though station experts strongly recommended the adoption of pure-bred animals and the use of proven sires, they warned against the temptations of cross-breeding in the strongest terms. Well into the 20th century many dairymen believed that crossbreeding different strains of cattle could produce an animal that combined the desirable traits of both parents. For example, many Holstein breeders would employ a Jersey bull in the hopes that the resulting offspring would produce large amounts of both milk and butter-

[220] a remark-a-bull?

fat. Eckles warned against crossing different breeds of cattle in the strongest terms; doing so "defeats the very object for which breeds have developed…As a rule, little is gained, and the outcome often is very disastrous."[221] Eckles objected to crossbreeding for two reasons. First, the unpredictable results of crossing cattle made such attempts a gamble. Though one might cross a Holstein with a Jersey and obtain an animal that produced large amounts of rich milk, one might equally obtain an animal that produced small amounts of poor-quality milk. Second, even when one obtained a cow that retained the hoped-for characteristics of her parents, the offspring of this animal usually reverted to form: "Many inferior animals appear in the second generation, making the results of crossing unsatisfactory."[222] Frank Gardner agreed with Eckles. While noting that in some cases a crossbreed might perform as hoped for, these crossbreeds would not pass the benefits to their children. Gardner also recommended against crossbreeding on the grounds that hybrids might prove infertile: "Nothing is to be gained by such method of breeding, as it destroys the pure lines that may have been established…it should never be carried beyond the first generation"[223]

Scientists thus recommended the use of production-testing as the best means to identify and eliminate low-producing cattle. They also advocated the adoption of pure-bred cattle. Because of generation of careful breeding these animals, taken as a group, possessed traits desirable in dairy cattle: large udders, thin frames, prominent milk-veins, etc. While these characteristics did not guarantee that a specific animal would

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[221] Eckles 113.

[222] Ibid.

[223] Gardner 22.

necessarily prove a superior producer, scientists did note a strong correlation between conformation to dairy type and actual production. Conformation could thus serve as a useful check in the absence of detailed production records. Furthermore, pure-bred cattle tended to pass these attributes to their offspring.

As discussed above, agricultural experts almost universally believed that the application of quantitative methods to the problems they faced represented the surest approach to solving these dilemmas. Again, to classify and measure was to "do" science. Nor did they regard this approach as one useful only in the laboratory; the success of cow-testing associations shows that at least some farmers adopted the scientists' mindset and approach. In effect, in adopting production-testing farmers tacitly acknowledged the superiority of a specific scientific approach: to weigh milk and measure butter-fat was, in essence to "do" science."

Such were the perceived advantages of adopting quantitative techniques that breeders soon applied these methods to problems that would seem antithetical to such analysis. Specifically, the adoption of score-cards by the various breed associations demonstrates the extent to which farmers accepted the superiority of the "scientific" approach. Until the first years of the 20th century breeders referred to the qualities possessed by the ideal dairy cow: the ideal animal was marked by her "femininity," her "nervous energy," and her "dairy temperament."

However, the first two decades of the 20th century all of the major dairy-breed associations adopted score-cards that attempted to quantify the various aspects of dairy cattle. Just as nutritional scientists evaluated rations by analyzing the constituent elements of the feed, so did breeders attempt to rate animals by fragmenting the animal into discrete elements each of which could then be evaluated and rated. The score-card adopted by the Ayrshire Breeders Association in 1906 is typical. One judged a cow by considering her not as a whole but by ranking her as a combination of discrete elements: one judged the head separately from the neck, the body independently of the legs, etc. Each element was given a certain number of points; the sum of these scores indicated the overall value of the animal, or, to be more precise, the degree to which she conformed to some breed ideal.[224]

Thus, dairy scientists engaged in advocating the practical benefits of a "scientific" approach to dairying proved remarkably successful in selling their methods to dairy farmers. When farmers began to keep accurate records, join cow- and bull-testing associations, and use score-cards to evaluate their cattle, they effectively emulated the techniques employed by – and the world-views held by – scientific researchers. In essence, dairymen became scientists to some degree, and this perhaps more than any other development marked the beginning of "modern" dairying.

By this point leadership dairying had largely shifted from farmers to researchers. New developments in both breeding and feeding would continue this trend, as scientists in the laboratory could examine phenomena in ways that farmers could not on the dairy. True, farmers – and the general public – could benefit from the discoveries of vitamins, for example, but could not really make advances except by the crudest sort of trial and error. In a sense, these discoveries turned farmers into, at best, junior partners so far as making scientific advances on the farm. Instead, farmers – or at least those who

[224] See Plumb, Appendix 1, for a number of judging cards employed by herd associations.

practiced “modern, “scientific” techniques - became technicians who carried out the instructions of others rather than innovators in their own right.

CHAPTER 7

OF RODENTS AND RUMINANTS: DAIRY SCIENTISTS DISCOVER VITAMINS

In the first five chapters of this work I have considered the various ways that researchers employed a common methodology and understanding of how to go about the scientific enterprise. The beauty of this methodology lay in its adaptability: scientists could apply similar methods regardless of the subject of their investigations. Moreover, the science they practiced allowed virtually all researchers to make important contributions; even those without access to elaborate research facilities could still perform much of the fieldwork and make the necessary observations and measurements.

By the second decade of the twentieth century a handful of scientists made discoveries that resisted, or even defied, analysis by traditional and accepted techniques. In the last three chapters of this work I examine three examples: the discovery of vitamins, the application of vitamin theory to milk, and breeding. In each case researchers found that investigating these topics by orthodox lines led to a dead-end. Instead, they had to develop new methodologies that allowed them to make sense of their findings.

Though they investigated different phenomena these researchers found that the mathematical tools of probability and statistics opened up new investigatory paths. However, the adoption of these tools came with a price: researchers could no longer predict with any certainty what would happen to any individual case – be it an animal, a ration, or sample of milk – but instead could only make predictions about the behavior of a large group. In short, the group replaced the individual as the object of study.

Yet despite this rather radical change of emphasis, and the shift from (relative) "certainty" to mere "probability" that it entailed, scientists did not lose their newfound positions of leadership in the American dairy enterprise. This fact suggests how thoroughly Americans had accepted the authority of scientific experts: the practice of "science" changed yet no evidence suggests that farmers took advantage of this opportunity to re-define what dairying meant, or even that they questioned the findings of scientists. Instead, the historical record shows that farmers continued about their work and, eventually, adopted the recommendations made by this new generation of researchers: the findings of the scientists described in these last chapters have long been accepted; dairy farmers have practiced these techniques for more than half a century.

By the time the United States entered the First World War, nutrition scientists, including many working at agricultural experiment stations, had become increasingly frustrated by their inability to completely understand nutrition within the framework they had employed since the mid-eighteenth century. Despite the Herculean efforts by researchers like Henry Prentiss Armsby, traditional concepts of nutritional science failed to account for a number of glaring anomalies, and researchers found themselves at something of a scientific impasse. Simply put, researchers had stretched their scientific approach to, and understanding of, nutritional science to a breaking point without finding the answers they sought. Frank Barron Morrison, Professor of Animal Nutrition at Cornell University, summarized the extent of contemporary knowledge about nutrition: "It was then generally believed that the only requirements for a satisfactory diet for humans or a complete ration for farm animals were adequate supplies of proteins, carbohydrates, fats, and mineral matter."[225] Unfortunately, these elements failed to adequately account for a number of nutritional mysteries, and despite ingenious plans to formulate "perfect" rations, scientists who clung to an orthodox understanding of nutrition proved unable to solve these puzzles.

The recognition of the importance of a group of nutrients that became known as "vitamins" offered scientists a new approach to their study of nutrition. Researchers discovered that vitamins played an important role in the growth and health of animals despite the fact that they generally appeared in minute quantities and, at least initially, could be identified only by their effects. Scientists found that the presence or absence of vitamins could help explain why scientifically formulated "balanced" rations sometimes failed to produce the expected results.

Dairy scientists played an important role in unraveling the mysteries of these "new" – or newly appreciated – substances, and an investigation of their efforts reveals that they went about their scientific work in a new way. The enigmatic nature of vitamins afforded scientists myriad avenues of exploration. Scientists interested in analyzing feedstuffs attempted to determine the amount of vitamins contained in various rations, while their colleagues who studied animal development examined how vitamins affected growth. Other researchers attempted to understand the role that vitamins played in milk production, in the ability of animals to absorb and synthesize vitamins, and which vitamin deficiencies caused which illnesses.

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[225] F.B. Morrison, *Feeds and Feeding*, 20th Edition, (Ithaca, NY: The Morrison Publishing Co., 1937), 122.

An historical examination of these efforts shows that the “discovery” of vitamins caused scientists to re-conceptualize their basic approach to science. Until this time, science, as practiced by dairy researchers, consisted primarily in analyzing – that is, identifying, categorizing, and measuring – the elements the researchers considered important: the amount of carbohydrates in a ration, the percentage of butter-fat in milk, or the length of time it took to milk a cow with a milking machine. By compiling data scientists hoped that they could discover formulas that would usefully predict results: that a certain amount of a certain kind of feed would produce a certain amount of milk containing a certain amount of butter-fat, etc. Though such formulae quickly became quite complicated, their difficulty stemmed primarily from their mathematical complexity. In short, scientists usually behaved as if scientific formulas amounted to algebraic equations, and the only difficulty lay in discovering which quantities could be added together.

The recognition of vitamins forced scientists to re-conceptualize their approach, primarily because they could not analyze or measure vitamins using accepted techniques. Vitamins proved elusive research subjects for a number of reasons. Most importantly, perhaps, the inability of scientists to discover the chemical makeup of these substances – most vitamins were not chemically identified until after World War Two – thwarted traditional analytic techniques. Scientists could no longer measure the amount of a vitamin in a feed, but could only detect its presence or absence by its effect on test subjects. Furthermore, scientists quickly discovered the existence of multiple vitamins, all of which played some role in nutrition, and had to ascertain the characteristics of each. Compounding the scientists’ difficulties, some vitamins seemed to magnify or diminish the effects of other vitamins, and certain animals seemed either to not need certain vitamins at all, or, perhaps, had the ability to synthesize these substances in other ways, either from bacteria that lived in their stomach or from sunlight.

These discoveries caused a fundamental re-evaluation of nutrition and, ultimately, of dairy cattle. Scientists had to abandon established techniques and develop new methods. In the process, their understanding of dairy cattle changed. Researchers had to confront the fact that animals were not simply machines that transformed given types of feed and water into milk with certain characteristics but, instead, responded differently to various feeds and conditions. They could synthesize vitamins from other nutrients, or from sunlight, and absorbed different amounts of vitamins from different foodstuffs. In short, scientists found that integrating vitamins into their researches meant more than simply measuring one additional component but forced them to reconsider the relationship between these elements.

Though the Polish chemist Casmir Funk coined the term “vitamine” – a contraction of “vital-amines” and later standardized as “vitamin” – in 1912, physicians and healers had long suspected that certain foods could prevent or cure diseases. Hippocrates described the symptoms of scurvy, though apparently not the cure, four centuries before the birth of Christ. About the same time the Chinese discovered that certain foods, which modern scientists would describe as rich in vitamin A, could alleviate night blindness, a condition caused by a deficiency of that nutrient. In the eighteenth century Dr. Lind, serving with the British Navy, found that citrus fruits helped prevent scurvy, and folk-wisdom prescribed cod liver oil for the treatment of rickets, another disease associated with a faulty diet.[226]

In the late nineteenth century a number of European researchers began to question whether a diet consisting solely of the then recognized and measurable constituents of food - carbohydrates, fats, and proteins - would support life. A number of scientists conducted trials during which they fed test subjects, usually rats or mice, simple diets comprised of only one or two foodstuffs. In general, these animals tended to show a decline in health compared to control groups that enjoyed a more varied diet. In 1896 C. Eijkman, a Dutch physician serving in the Dutch Indies, stumbled upon a discovery that eventually pointed the way for scientists interested in nutrition. Eijkman had been conducting experiments on birds and fed them with scraps from the military hospital at which he served. The refuse consisted primarily of polished rice, and the birds soon developed paralysis. However, when the hospital director cut off Eijkman's food supply, he substituted more inexpensive unpolished rice, and the birds quickly regained their health. Eijkman hypothesized that unpolished rice might contain some essential nutrient not found in polished rice. He came to believe that the results might be applicable to humans, and became the first to suggest that unpolished rice might alleviate beriberi in humans. He eventually determined that the essential ingredient that prevented the onset of paralysis was found in the rice husk. Subsequent researchers built on Eijkman's work.[227]

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[226] Leonard A. Maynard, *Animal Nutrition* (New York: McGraw-Hill, 1937), 172-173.

[227] Elmer Verner McCollum, *A History of Nutrition* (Boston: Houghton Mifflin, 1957), 216-217. For a brief yet useful overview of the vitamin theory written by one of the

By the first decade of the twentieth century the fact that animals required nutrients in addition to carbohydrates, proteins, and fats had been firmly established. In *A History of Nutrition* Elmer McCollum, who played a central role in the development of the modern vitamin theory, noted – rather Whig-ishly, it should be noted - "In 1906…there were hidden in scientific journals thirteen papers which contained accounts of nutrition experiments based upon diets which were simplified in the chemical sense…in every such experiment the animals quickly declined…the conclusion was inevitable that some one or more unknown nutrients were necessary for the preservation of health and the maintenance of life." However, the scientists who undertook these researches failed to build upon their findings; in McCollum's words, since "none of these [experiments] were followed up by further inquiries, they did nothing more than prove that unknown nutrients existed."[228] Stephen Babcock, who perfected the fat-testing apparatus, should presumably be numbered among those chastised by McCollum (though it should be noted that in the first edition of his *Newer Knowledge of Nutrition* McCollum carefully reported that Babcock performed his experiments "with the cooperation of Mr. Steenbock and the author").[229] Before he became interested in the question of milk-fat, Babcock had experimented with feeding small groups of animals from the University of Wisconsin's dairy herd rations comprised of a single foodstuff. After two of his first four test subjects died due to malnutrition the director of the agriculture department called a halt to this line

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principle actors, see also McCollum, *The Newer Knowledge of Nutrition* (New York: The Macmillan Company 1918), 5-33.

[228] McCollum, *A History of Nutrition*, 201-202.

[229] McCollum, *The Newer Knowledge of Nutrition*, 10.

of research. Babcock turned his work in other directions, but the example demonstrates the widespread suspicion that traditional nutrition theory might not account for all the ingredients necessary for health.[230]

In 1912 Polish chemist Casmir Funk suggested that conditions such as beriberi (which caused nervous disorders), scurvy (characterized by weakness, anemia, and bleeding gums), and pellagra (whose symptoms included gastrointestinal disorders, skin problems, and mental disorders) were caused by some sort of dietary deficiency. His proposal that foodstuffs might not necessarily contain necessary nutritional elements formed the basis for subsequent research. American agricultural scientists quickly applied Funk's hypothesis to their own ongoing investigations in nutrition. Two pairs of researchers, both affiliated with agricultural colleges or experiment stations, made the most important contributions to the understanding of vitamins and helped establish new theories about nutrition.[231]

In 1915 McCollum and Davis, then working at the Agricultural School at the University of Wisconsin, discovered that animal fats contained some nutrient essential for health. Their research methods typified the efforts of most of the early researchers into vitamins. McCollum and Davis began by feeding laboratory rats a simple diet consisting

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[230] For more on Babcock's experiments with nutrition, see also Harry L. Russell, *Stephen Moulton Babcock: The Man of Science* (Madison, WI: The Wisconsin Alumni Research Foundation, 1943), 7-8. Russell in fact claims that while "The University of Wisconsin is known throughout the entire scientific world for many discoveries…no more fruitful researchers have been undertake, no richer returns have ever been made to the science of animal and human nutrition, than these [Babcock's] 'single grain' ration trials."

[231] For more on Funk's role, see McCollum, *The Newer Knowledge of Nutrition*, 19-20, 93-95. See also E.T. Halnan and Frank H. Garner, *The Principles and Practice of Feeding Farm Animals* (New York: Longmans, 1940), 48.

of a single foodstuff. In most cases the rats quickly displayed symptoms on malnutrition and, if untreated, died. The pair of researchers found that supplementing the diet of these rats with small amounts of animal fats quickly restored their health. However, adding vegetable fat, such as olive oil, to the rat's diets did not ameliorate the condition. The pair of researchers eventually isolated a nutrient which they named fat-soluble A, which later became known as vitamin A.[232]

A second pair of American scientists, Thomas B. Osborne and Lafayette B. Mendel, who were affiliated with the Connecticut agricultural experiment station, had been conducting independent research along the same lines. Their experiments resembled those of their colleagues at Wisconsin, but Osborne and Mendel also experimented with the difference in nutritional consequences of feeding rations which had been heated and dried. They discovered that animals fed dried rations developed deficiency diseases. From these findings they postulated the existence of another, water-soluble substance necessary for health. They called this substance – which eventually became known as vitamin B - water-soluble B, or "the anti-beriberi substance."[233]

News of these scientists' discoveries quickly spread, and researchers at a number of agricultural colleges and experiment stations began to experiment along similar lines. Unfortunately, their efforts were hampered by a number of factors. For example, scientists did not know how many vitamins existed, nor did they know, at least initially, which vitamins ameliorated which nutritional ailments. Furthermore, they had to

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[232] McCollum and Davis, "The Cause of the Loss of Nutritive Efficiency of Heated Milk," *The Journal of Biological Chemistry* 23 (1915): 247.

[233] Thomas B. Osborne and Lafayette B. Mendel, "The Relation of Growth to the Chemical Constituents of the Diet," *The Journal of Biological Chemistry* 15 (1913).

determine which foodstuffs supplied which vitamins. Since they were unable at that time to determine the chemical structure of vitamins they were forced to make educated guesses about the amount of vitamins that various feeds contained based on their effect on test animals.

The discovery that different animals needed different amounts of vitamins added to the confusion. Nutrition researchers employed a number of test animals for their experiments; though most scientists used laboratory rats, others experimented on various kinds of fowl, on guinea pigs, and on a variety of domesticated mammals and at first did not realize that animals might have different nutritional needs. For example, fresh citrus products prevented scurvy in humans, but dried citrus failed to prevent the disease. However, rats afflicted with scurvy quickly recovered when fed fresh or dried citrus fruits. Other experiments revealed that guinea pigs required a nutrient that rats did not. Conflicting and contradictory results proliferated as more scientists began to incorporate the vitamin theory into their methodology.

As a result, dairy scientists found themselves with a job for which they were imminently suited. To determine the vitamin requirements of dairy cattle one had to – at least until researchers perfected other methods - perform tests on dairy cattle; tests of other sorts of mammals proved worthless at best, and could be misleading. Scientists working at experiment stations and agriculture colleges throughout the United States commenced efforts to determine exactly which vitamins dairy cattle required and how much they needed to consume to maintain health. Other researchers attacked the problem from the feeding side. They hoped to determine which feeds contained which vitamins, and in what quantities. Still other workers studied the effects that vitamins, or their lack, produced. They attempted to ascertain whether animals could store vitamins, and, if so, which ones. They also began tests designed to calculate the vitamins needed for growth, for maintenance, and more milk production. Finally, a growing number of scientists began to explore the possibility that animals might be able to synthesize vitamins from other feeds, or from sunlight, and the possibility that it was not the animals themselves, but bacteria living in their stomachs, that transformed feeds into necessary nutrients.

However, the unique nature of these newfound elements thwarted easy analysis. Scientists found they could not apply their usual laboratory techniques in their assay of vitamins. Until after World War Two most vitamins could not be chemically identified but could be detected only by their effects. This represented a twofold challenge for researchers accustomed to analyzing feeds by measuring the quantities of recognized substances contained in the ration and the effect of these foodstuffs on the animal by calculating weight changes or variations in the amount of milk they produced: first, scientists had to define and categorize the elusive elements that became known as vitamins; second, they had to invent some method of measuring the newfound substances.[234]

Because they could not measure the effects of feeds directly, scientists had to adopt both a new mindset and a new approach to their experimental efforts. Researchers first had to ascertain whether a ration contained a certain vitamin. Without knowledge of the chemical makeup of vitamins, scientists were forced to rely on animal subjects. To do so they usually fed animals fixed ration and waited to see if any deficiency diseases appeared. Having established the presence of a vitamin, they had to devise means of

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[234] See Leonard A. Maynard, *Animal Nutrition* (New York: McGraw-Hill, 1951).

estimating how much of the vitamin the feed provided. Again, since they could not directly measure this amount they had to develop not only new techniques but, perhaps more importantly, new measures for vitamins. This process required scientists to modify and eventually abandon traditionally held theories and practices and adopt new techniques. In the end, the discovery of vitamins forced a fundamental shift in the way these scientists approached their work.

To understand role that vitamins played in the nutrition of dairy cattle, scientists first needed to determine which vitamins cows actually required. C.H. Eckles and L.S. Palmer, professors of agriculture at the University of Wisconsin, played the most important role in these efforts by definitively establishing whether dairy cattle required vitamins A, B, and C. In each case, they enlisted the aid of doctoral student from the agriculture college, collaborated with the student on the design and performance of the experiments, and co-authored a paper, drawn largely from the student's dissertation, which found publication in established journals.

Eckles and Palmer assigned the first of these students, S.I. Bechdel, the task of definitively establishing whether dairy cattle required vitamin B to maintain growth and health. The resulting article, "The Vitamin B Requirement of the Calf," began by noting the difficulty of establishing which animals needed which vitamins: ""Five vitamins are now known to be important in human and animal nutrition. Some of them are more important in the life of certain species than they are in others." For example, guinea pigs and humans required vitamin C, while rats and chickens appeared indifferent to the vitamin's presence in their rations. However, rats and chicks, though not hogs, did require vitamin A. Due to these vagaries, the article continued, "The extent to which the results with laboratory animals can be applied to the larger domestic animals, is, therefore, questionable." In short, the only way to determine whether cattle required a specific vitamin was to perform tests on dairy cattle[235]

The first step in making this determination lay in deciding how to detect whether the vitamin affected the health of the animal. In their groundbreaking work on vitamin B Osborne and Mendel had established that baby rats required vitamin B for proper growth.[236] The complete absence of vitamin B halted the animal's development, while the presence of an inadequate supply resulted in stunted growth. Bechdel decided that they would feed freshly weaned dairy calves a ration devoid of vitamin B and track their development in comparison with a group of control animals that received the same ration fortified with vitamin B extract.[237]

The construction of a suitable test ration posed a real problem for the researchers. They lamented that "The selection of a palatable ration that carries all of the known dietary factors for growth and well being of calves, excepting vitamin B, offers a real problem since all of the common hays and cereal grains as well as milk and milk powders are know to contain a considerable amount of this vitamin." In the end, they fed the cattle on rations comprised of feed by-products: a mixture of corn gluten, commercial grade casein, polished rice, and butterfat. To confirm that this diet did not contain

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[235] S.I. Bechdel, C.H. Eckles and L.S. Palmer: "The Vitamin B Requirement of the Calf," *Journal of Dairy Science* 9, No. 5 (1926): 409.

[236] Thomas B. Osborne and Lafayette B. Mendel: "The Relation of Growth to the Chemical Constituents of the Diet," *The Journal of Biological Chemistry* 15 (1913): 311-326. See also Osborne and Mendel, The Journal of Biological Chemistry, 1913, p. 431.

[237] Ibid., 408-411.

vitamin B they first tested the ration on rats, which scientists had previously established did require vitamin B for growth. As hoped, the rats soon displayed symptoms consistent with a vitamin B deficiency.[238]

The animals fed on this rather bland diet – one of the biggest problems the researchers faced was convincing the test animals to consume the unappealing ration - eventually showed signs of malnutrition, but not in the ways the researchers expected. The animals did not show the signs of stunted growth the scientists had predicted might result from deficiency of vitamin B. Instead, they displayed the symptoms consistent with a lack of vitamin A. Fortunately for the scientists, if not for the calves, the animals in the control group showed the same symptoms. Bechdel tweaked the rations to make sure it included ample amounts of vitamin A and the symptoms disappeared.[239]

At the end of the feeding trial the scientists weighed and measured the calves in the test and control groups and compared the results. They found no difference in growth or in health between the two groups and concluded that cattle did not seem to require rations that contained vitamin B to maintain growth health. However, to make certain that latent complications from a vitamin B shortage did not arise, they kept the animals in the university's dairy herd. All of the test subjects subsequently delivered healthy calves and became profitable – and apparently normal – members of the herd.[240]

Eckles, Palmer, and their graduate students performed similar trials to test whether dairy cattle required vitamins A and C. They began by formulating a ration free

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[238] Ibid.

[239] Ibid.

[240] Ibid.

from the vitamin under consideration, tested the ration by feeding it to laboratory animals known to be sensitive to the substance, and conducted trials using two groups of calves, both fed exclusively on the test ration, but with a control group receiving ample amounts of the vitamin being examined. The researchers then compared the health and development of the two groups. If they proved equally healthy they concluded that cattle did not require the nutrient; if the test group showed signs of malnutrition they concluded that the animals needed that vitamin. They determined that cattle did not need vitamin C, but quickly developed symptoms of malnutrition when deprived of vitamin A.[241]

The nutrient that eventually became known as vitamin D proved much more difficult to isolate and identify. Scientists and physicians had long puzzled over the cause of rickets, an ailment that causes a softening and, in extreme cases, a bending of the bones. The Greek historian Herodotus accurately recorded the symptoms, and (as it turned out) one of the causes, of rickets in the fifth century B.C. Visiting a battlefield, the historian examined the corpses of slain Egyptian and Persian warriors and was struck by the fact the skulls of the fallen Persian soldiers were often broken, while those of the Egyptians remained intact. Herodotus theorized that exposure to sunlight might explain this anomaly, since (according to Herodotus) Persians traditionally covered their heads with a turban from a very young age, while Egyptians regularly exposed their scalps to sunlight. Despite the Greek's rather macabre observations, however, the physiological cause of rickets remained a mystery well into the twentieth century. During the First

[241] L.M. Thurston, C.H. Eckles, and L.S. Palmer, "The Role of the Antiscorbutic Vitamin in the Nutrition of Calves," *Journal of Dairy Science* 9 (1926): 37-49, and I.R. Jones, C.H. Eckles, and L.S. Palmer, "The Role of Vitamin A in the Nutrition of Calves," *Journal of Dairy Science* 9 (1926): 315-326.

World War the British, who enjoyed what most scientists considered one of the healthiest diets on the planet because of its ample amount of meat, vegetables, and dairy products it contained, ranked among the leading nations in the occurrence of rickets. Most physicians attributed the disease to filthy homes and poor hygiene. In 1917 the British scientist Leonard Findlay, writing about rickets, admitted that "In spite of the most varied and extensive research we have practically no real knowledge of the nature of the causation of this widespread malady, or the factors which determine its onset," and concluded that industrial pollution might account for its widespread occurrence in Britain.[242]

In 1915 a group of American scientists and physicians, led by Dr. John Howland of John Hopkins Hospital and including among their number Elmer McCollum, concluded that rickets might be caused by a vitamin deficiency. They began their efforts by surveying the available literature, which suggested a link between a lack of sunlight and rickets, and combined these findings with experiments McCollum had performed which suggested rickets was caused by the lack of some nutrient. McCollum, one of the co-discoverers of "fat-soluble A," was intrigued by the fact that cod liver oil, which he had established possessed high amounts of that nutrient, had long been employed as an effective folk remedy for rickets. Suspecting the presence of some new, as yet unidentified nutrient, he performed a series of tests which showed that butterfat and cod liver oil both successfully ameliorated vitamin A deficiencies, but only cod liver oil prevented the onset of rickets. McCollum then ran a trial employing oxidized cod liver oil, and found that oil which had been subjected to oxidation effectively neutralized the

[242] McCollum, *A History of Nutrition*, 266-270.

vitamin A it contained. However, the oxidized oil still proved effective in treating rickets. McCollum concluded that he had discovered the existence of a fourth essential nutritional substance, vitamin D.[243]

However, demonstrating that dairy cattle did not develop the symptoms of vitamin deficiency when fed rations lacking in those nutrients did not necessarily prove that the animals did not require the vitamins. Vitamins B and D in particular posed difficulties for nutritionists investigating their properties. In both cases circumstantial evidence suggested that cattle did, in fact require the nutrients but they could, at least in some cases, somehow synthesize the nutrients from their fodder, though the limited data available suggested that animals produced vitamins B and D in very different ways.

A number of scientists, beginning with August Pacini and Dorothy Wright Russell, believed that the nutrient eventually named vitamin B might be produced by bacteria that lived within the animals' digestive tract. In particular, these researchers believed that vitamin B might be produced by various members of the Bacillus family. Without discounting the possibility that some sort of bacteria might produce vitamin B, in 1921 Samuel R. Damon, a chemist working at Brown University, demonstrated that no known strains of the Bacillus could synthesize vitamin B. Like his comrades, Damon could only detect the presence or absence of vitamin B by its effects on test animals. For his experiment Damon employed laboratory rats which, as a species, demonstrated a marked requirement for vitamin B. To test the ability of each strain of Bacillus to

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[243] Ibid., 276-281; E.V. McCollum et al., "Studies on Experimental Rickets: I. The Production of Rachitis and Similar Diseases in the Rat by Deficient Diets," *Journal of Biological Chemistry*, 45 (1921): 333-341; P.G. Shipley et al., "Studies on Experimental Rickets: II. The Effect of cod Liver Oil Administered to Rats with Experimental Rickets," *The Journal of Biological* Chemistry 45 (1921): 343-348.

synthesize vitamin B, Damon compounded a ration devoid of vitamin B. He then added a live Bacillus culture to the ration. Comparing the growth rates of rats fed identical diets, save that one group received a known dose of vitamin B while the other received feed containing the Bacillus culture, allowed Damon to determine whether the Bacillus culture could indeed synthesize vitamin B. In each experimental trial the test group quickly displayed the effects of vitamin B deficiency, while the control animals maintained perfect health. Damon completed similar tests for all known varieties of the Bacillus bacteria, and, without discounting the possibility that some other bacteria might produce vitamin B, concluded that Bacillus did not produce the nutrient.[244]

The example of vitamin D posed a different set of difficulties for researchers. Cattle proved to be sensitive to vitamin D; a lack of the nutrient caused rickets in calves and osteoporosis in mature animals. However, researchers found that while most of the commonly employed dairy rations contained very low amounts of the vitamin, only a relatively small number of animals displayed the effects of vitamin D deficiency. Compounding the confusion was the well-established fact that rickets occurred most frequently during the winter months when cattlemen were more likely to house their herds and feed them with stored fodder. This fact presented scientists with a pair of possibilities: that storing fodder somehow destroyed the vitamin D it contained and that dairy cattle therefore required fresh feed, or that animals could somehow synthesize the nutrient but required sunlight to do so.

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[244] Samuel R. Damon, "Bacteria as a Source of the Water-Soluble B Vitamine," *The Journal of Biological Chemistry* 48 (1921): 379-384.

Scientists eventually discovered that most cattle fodders contained small amounts of a substance called ergosterol which, upon exposure to ultraviolet radiation, produced vitamin D. Furthermore, this conversion could take place within the plant or within the animal. Hence, animals that received fresh rations usually received adequate amounts of vitamin D, as did animals who consumed dried rations but enjoyed exposure to ample amounts of sunlight. However, this information did not appear at once, and throughout the 1920's scientists continued to debate the role that sunlight played in the formation of vitamin D.[245]

Nutritional scientists working at the nation's agricultural universities and experiment stations devoted the lion's share of their efforts to the study of the first vitamins – A, B, C, and D – that they believed they had positively identified. Much to their chagrin, their further efforts to understand and explain the roles of these substances suggested the presence of still more vitamins. Specifically, researchers found that animals fed rations which, according to test results on both laboratory animals and in actual trials with livestock, seemed to be contain adequate amounts of the known vitamins, developed other health difficulties. These took two forms: some animals experienced stunted growth while others experienced breeding problems. By 1920

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[245] The literature on the link between vitamin D and sunlight is extensive. For an useful overview, see Henderson, Larson and Putney, *Dairy Cattle Feeding and Management*, Third Edition (New York: John Wiley and Sons, 1938), 89-90. For specific case studies, see G.C. Wallis and T.M. Olson, "The Effect of Season and Feeds on the Vitamin D Content of Milk Under South Dakota Conditions," *South Dakota Agricultural Experiment Station Bulletin No. 321* (1938); Harriette Chick and Margaret Honora Roscoe, "Influence of Diet and Sunlight upon the Amount of Vitamin A and Vitamin D in the Milk Afforded by a Cow," *Journal of Biological Chemistry* (1926): 632-649; H. Steenbock et al., "The Antirachitic Value of Cow's Milk as Modified by Exposure of the Cow to Sunlight and to Radiations from a Quartz Mercury Vapor Lamp," *Journal of Biological Chemistry* (1930): 103-126.

scientists suspected the existence of at least two new vitamins: a “growth-vitamin” and a “fertility-vitamin.”

The former, the “growth” element, received the first wide-spread attention. Scientists working on vitamin B discovered that in some cases animals fed rations that ameliorated the affects of pellagra and other neuritic diseases – in other words, rations that scientists had determined contained vitamin B – showed symptoms of stunted growth. Researchers at a number of institutions, including several working at agriculture staions, eventually confirmed their hunch, and by 1927 had posited the existence of a second water-soluble vitamin, which American nutritionists for more than a decade dubbed vitamin G before finally adopting the nomenclature of their European counterparts, who named the substance “vitamin B2.”[246]

The discovery of B2 neatly illustrates the difficulties of vitamin research and the intricacies of scientific practice at the time. The problems stemmed from the fact that in compounding rations designed to ascertain the presence of certain nutrients scientists often subjected rations to some sort of purifying procedure designed to neutralize undesirable properties of the feed; in short, they followed the methodology of science as then practiced. This meant that they attempted to understand the world by isolating a single factor and then exploring the consequences of changes in that variable. However, the difficulties presented in analyzing vitamins whose chemical composition was unknown and whose properties were, in many cases, little understood, required scientists to adopt new techniques in their investigations. In this case, the process of isolating vitamin B usually dictated that researchers purify their feed. They employed two

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[246] Leonard A. Maynard, *Animal Nutrition* (New York: McGraw-Hill, 1937), 213-216.

methods: irradiation or heating in an autoclave. Scientists, led by W.D. Salmon, eventually demonstrated that each method destroyed one of the B vitamins: irradiation nullified vitamin B2, while heating in an autoclave diminished the potency of vitamin B1, as the "anti-neuritic vitamin" was now labeled. Before the fully recognized the characteristics of the newfound substance – and granted it its own moniker - scientists often referred to the new vitamin simply as the "heat-stable B vitamin."

About the same time – the early 1920's – that scientists began to suspect the existence of (what they eventually identified as) vitamin B2 or Riboflavin, they also postulated the existence of a "fertility" vitamin. Their suspicion stemmed from their nutritional experiments with laboratory rats. Scientists often found that rats fed a presumably "complete" ration, though they displayed perfect health and no symptoms of malnutrition, displayed signs of sterility; some reproduced only with difficulty, and many did not reproduce at all. Researchers were aware that a deficiency of vitamin could cause birth defects, but investigators, led by the team of Evans and Scott, demonstrated that a lack of the "fertility" vitamin produced very different symptoms than a lack of vitamin A.[247]

Though knowledge of the exact workings the "sterility" vitamin, which would eventually be dubbed vitamin E, remained obscure, by the 1930's a number of dairy researchers began investigating the effects of vitamin E on dairy cattle. In particular, a pair of researchers at the University of Nebraska, I.L. Hathaway and H.P. Davis, jumped at the possibility that the lack of some hitherto unknown substance might account for the

[247] For the discover of Riboflavin, see McCollum, *A History of Nutrition*, 291-301.

"Difficulties in breeding with the dairy herd and the university of Nebraska have been experienced to a greater or less degree for nearly a third of a century."[248]

The Nebraskans undertook their researches using practices that had, by the 1930's, become standard techniques: they tested the various foodstuffs typically employed by Nebraska dairymen on laboratory rats. Feeding groups of the rodents diets consisting solely of one ration, they then attempted to breed the rats. Measuring the number of litters conceived and the number of offspring produced could, when compared to the fertility of a control group, ascertain the presence and, to some degree, the potency of vitamin E. So confident were the scientists in their ability to evaluate the quality of dairy fodder using rodents, they did not test their findings on actual cattle.[249]

The use of laboratory animals, as alluded to above, presented difficulties. The primary obstacle lay in the fact that, though rats proved sensitive to deficiencies of most nutritional substances, scientists found that farm animals' nutritional requirements did not necessarily mirror those of their caged cousins. Furthermore, the various animals did not share common needs. Cows could, at least in some cases, produce vitamins B and D from other elements, and did not seem to require large amount of vitamin C. Horses, swine, sheep, poultry, and the other farm animals each required a special diet.

Nonetheless, the use of laboratory rats formed the basis for the majority of nutritional investigations about the role played by vitamins. An examination of their use demonstrates not only the ways that the discovery of vitamins forced scientists to adopt

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[248] I.L. Hathaway and H.P. Davis, "The Vitamin E Content of Certain Dairy Feeds," *Nebraska Agricultural Experiment Station Research Bulletin No. 73* (1934): 3.

[249] Ibid.

new methodologies but also the persistence of a quantitative methodology that undergirded virtually all agricultural science.

Scientists interested in problems of malnutrition, and especially those interested in the possible existence of unknown nutritional elements, had long employed laboratory animals. Researchers employed a host of creatures: mice, dogs, guinea pigs, pigeons and other fowl all found homes in laboratories. However, the white rat became the laboratory animal of choice. They possessed a number of advantages: they matured quickly and reproduced almost as fast. Furthermore, rats were cheap to procure, ate little, and required little space.[250]

Most importantly for vitamin researchers, rats possessed two valuable qualities. First, intensive inbreeding had virtually fixed the rat's genetic makeup. Researchers could generally rely on the fact that rats they procured from the various scientific supply houses that sold laboratory animals were a largely uniform product, and they relied on the fact that rat was a rat was a rat. Second, rats proved sensitive to deficiencies of virtually all known nutritional substances: though some farm animals seemed to be able to do without some vitamins, rats required the complete gamut.[251]

At first scientists generally employed the rats as qualitative laboratory instruments. Simply put, they would test rations for the presence of a particular vitamin by feeding rats and noting the onset of symptoms of malnutrition. For example, rats that did not receive enough vitamin C quickly exhibited signs of scurvy, while those deprived

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[250] For an informative look at the history of the use of rats and mice in the laboratory, see Karen A. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900-1955* (Princeton NJ: Princeton University Press, 2004).

[251] Ibid.

of vitamin D developed rickets. Though scientists tried to quantify these measures in various ways, by counting the number of days on a ration before the onset of visible malnutrition, or the percentage of rats that developed symptoms, the primary focus of these investigations using rats remained qualitative.

The case of vitamin E, the "fertility-vitamin," fell somewhere in the middle. Scientists primarily tested rats by noting their fertility: whether or not they reproduced. A team of scientists led by H.A. Mattill determined that rats that did not consume enough vitamin E failed to reproduce: they would absorb their embryos rather than carry them to term. In the words of McCollum, "The young are not aborted but undergo autolytic dissolution and are resorbed."[252] Quantitative measures played a secondary role, as researchers counted the number of young produced in each litter, and measured the amount of time that passed before young rats, derived of vitamin E, developed paralysis.[253]

Of course, scientists undertook the endeavor to devise methods that might allow them to more quantitatively measure the effects of vitamins. For example, H. Steenbock and Katharine H. Coward, researchers in the Department of Agricultural Chemistry at the University of Wisconsin, played an important role by developing statistical methods for approximating the amount, or more precisely, the potency, of the vitamin A contained in rations. They began by advocating a new method of detecting vitamin A deficiency. Instead of looking for signs of stunted growth and decline, they instead measured the incidence of ophthalmia "as a sign of exhaustion of the animal's store of vitamin A in

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[252] McCollum, *A History of Nutrition*, 361.

[253] Ibid., 363.

preference to cessation of growth. The two are often simultaneous, but the use of the former criterion prevents loss of animals." Having established a criterion for the onset of malnutrition, they employed statistical methods that allowed them to assign relative values to the amount of vitamin A contained in a ration. They tested feeds on groups of rats by measuring the amount of time elapsed before animals developed signs if vitamin A deficiency. The use of groups allowed them to compute an "average" which they could then compare to the control group and to groups fed on other feeds. Their employment of groups of rats necessitated the use of statistical methods that minimized the influence of outliers and other data that might skew results. [254]

Though the method developed by the Wisconsin team of Steenbock and Coward showed promise, a competing method gained the most widespread acceptance, so much so that tracking down its origins proved an exercise in futility. Regardless, this method relied on the fact that rats had become so standardized and homogenous in their reactions. Scientists discovered that healthy lab rats generally gained three grams of weight per week. Before scientists successfully assayed the chemical composition of vitamins, which allowed a direct measure of the quantity contained in a feed, they agreed to measure vitamin A in "rat units," a rat unit being defined as the amount of vitamin A necessary to ensure a weight gain 24 grams over an eight week trial. The actual weight gained – as measured by taking the average weight of a group of animals and comparing

[254] H. Steenbock and Katharine H. Coward, "The Quantitative Determination of Vitamin A," *Journal of Biological Chemistry* 72 (1928): 778.

them with a control group – allowed scientists to determine quantify the amount of vitamin A contained in a ration.[255]

The test for vitamin B was identical to the test for vitamin A: a "rat unit" of vitamin B being the amount necessary to cause the rat to gain 24 grams of weight over an eight week span. The test for vitamin C was more vague, because the onset of scurvy did not necessarily stunt growth. In the case of vitamin C scientists instead employed the "Sherman unit," defined as the amount of vitamin C necessary to prevent the onset of scurvy.

The test for vitamin D proved most intrusive, at least for the rat. To measure the potency of vitamin D contained in a ration scientists fed groups of rats a specific ration for a period of time, usually eight weeks. They then dissected the animals and measured the thickness of the bone walls of the radii and ulnae of the animals. Like vitamins A and B, vitamin D was measured in "rat units."[256]

Thus, laboratory rats became not only testing devices which could display the presence or absence of a specific condition – i.e. pregnancy, or scurvy, etc. – but, at least for a time, functioned as measuring apparati: scientists used rats to determine the amount of vitamin A present just as they might employ a calorimeter to measure the number of calories contained in a ration. Furthermore, the use of rats required scientists to adopt statistical methods. Despite the use of standardized rats scientists could not rule out the

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[255] Maynard 187, 207, 221, 222.

[256] G.C. Wallis and T.M. Olson, "The Effect of Season and Feeds on the Vitamin D Content of Milk Under South Dakota Conditions," *South Dakota State Agricultural Experiment Station Bulletin No. 321* (1938): 4-7.

possibility that any one rat might skew the test results, and therefore the researchers relied on statistical studies that minimized outliers.

Such testing sometimes blurred the boundaries between laboratory and dairy farm, or, more specifically, between rodent and ruminant. The discovery of vitamins helped to resolve some of the outstanding problems of nutritionists, but complicated matters by adding a new dimension and, in effect, changing the rules of nutrition, and the way scientists approached their work. Agricultural scientists – and to no small degree, dairy scientists – played an important role in uncovering the mysteries of vitamins. But the "discovery" that vitamins existed, and played an important role in nutrition, does not tell the whole tale. Dairy scientists still had to apply this new-found knowledge, and doing so posed a different set of obstacles.

Dairy researchers first had to determine what nutrients cattle required. This proved a fairly easy task, as dairy cattle displayed the same symptoms as the animals employed in the laboratory. Scientists soon found that the addition of the nutritional elements that would alleviate the symptoms of scurvy, or rickets, or other problems in rats would ameliorate the same symptoms in dairy cattle. Despite occasional discrepancies, such as the fact that dairy cattle did not seem to require vitamin B(1), scientists found it relatively easy to determine which vitamins dairy cattle required.

However, the application of this knowledge proved more difficult. While scientists could easily deduce that dairy cattle that did not receive enough vitamin D developed rickets, figuring out how to compound a ration that supplied the necessary amounts of the various vitamins proved an entirely different matter. Researchers faced two tasks: how to determine the vitamin content of commonly-employed dairy fodders, and how to evaluate how much of the vitamin animals required.

In many ways the first proved a simpler, if still arduous, problem. To determine the amount of vitamins that a ration contained scientists generally employed laboratory rats. Researchers usually proceeded by feeding rats dairy rations and calculating the vitamin content of the various feeds using the methods described earlier in this chapter. Doing so enabled them to determine the amount – often measured in "rat-units" – of each vitamin that the fodder contained. Researchers at Texas led the way, compiling a list of over two hundred different rations and the amount of vitamin A each ration provided. Researchers at the various state experiment stations followed suit, generally by computing the vitamin content of locally available dairy fodder.[257] For example, researchers at the Idaho experiment station studied the vitamin content of regional pasture grasses, while scientists at the Nebraska station measured the vitamin content of the various varieties of corn grown in the state.[258]

Unfortunately, the use of rats as quantitative instruments made such estimates difficult. The biggest obstacle stemmed from the fact that dairy cattle, even those of pure-bred stock, did not exhibit the uniformity in vitamin need of standardized laboratory rats. Therefore, it remained difficult to determine the amount of vitamins, or of a specific vitamin, contained in a specific ration. Compounding this difficulty was the fact that

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[257] G.S. Fraps and Ray Treichler, "Vitamin A Content of Foods and Feeds," *Texas Agricultural Experiment Station Bulletin No. 477* (1933).

[258] I.L. Hathaway and H.P. Davis, "The Vitamin E Content of Certain Dairy Feeds," *Nebraska Agricultural Experiment Station Research Bulletin No. 73* (1934).

dairy animals did not seem to require a uniform number of "rat-units" of a specific nutritional element: some required more, and some required less.

This confusion was perhaps best exemplified by the various editions of *Feeds and Feeding*, the most popular feeding manual of the first half of the twentieth century. W.A. Henry published the first edition in 1898, and new editions, containing the latest developments in animal nutrition, appeared regularly after that date. Though the sixteenth edition of the manual, published in 1918, briefly mentioned the "discovery" of vitamins, it was not until 1923, with the appearance of the eighteenth edition, that the manual contained a table that listed the vitamin content of feeds. The table rated the presence of the then-acknowledged vitamins – A, B, and C – contained in various feeds. However, it did not do so by listing the amount of each vitamin found in each fodder; instead, each ration received a ranking for each vitamin consisting of a number of "+'s" and "-'s." A "+" ranking indicated that the feed contained the vitamin, and a "-" indicated it did not. Multiple "+"'s served to indicate the relative concentration of each vitamin contained in a feed. The 20th edition, which appeared in 1936, retained the same ratings system, an indication of the continuing confusion about the amount of vitamins required by farm animals.[259]

The intensive use of rats as investigatory instruments and lack of consensus about the vitamin needs of cattle caused some dairy science to seemingly lose sight of their objectives. The Nebraskan scientists mentioned above, who were seeking to determine whether a lack of vitamin E might explain fertility problems in the university's dairy

[259] W.A. Henry and F.B. Morrison, *Feeds and Feeding*, Eighteenth Edition (Madison, WI: The Henry-Morrison Co., 1923). F.B. Morrison, *Feeds and Feeding*, Twentieth Edition, (Ithaca, NY: The Morrison Co., 1937).

herd, represent a case in point. Suspecting that many of the fodders employed on the farm lacked sufficient amounts of the nutrient, they assayed the amount of vitamin E present in a wide variety of feeds commonly utilized in the state. However, despite the fact that they hoped to determine which feeds might supply vitamin E for dairy cattle, they never employed actual dairy animals. Instead, they performed all of their experiments on laboratory rats, a seemingly strange oversight for results published in a bulletin entitled "The Vitamin E content of Certain Dairy Feeds."[260]

Of course not all researchers forsook the use of dairy cattle, though those who primarily experimented with cattle seem to have comprised a distinct minority. Certainly the use of laboratory animals instead of productive dairy cattle offered some attractive advantages. Laboratory rats were cheap, easy to house, displayed symptoms of malnutrition quickly, and, compared to cattle, displayed a helpful uniformity. Too, employing herd animals required that researchers purposely induce maladies that, even when they did not prove lethal, often limited the value of the animals as productive members of the university's herd. In any case, scientists most often performed their tests on rats, and only occasionally seem to have verified their results in "real-world" trials on actual dairy animals.

Some scientists did perform experiments with dairy animals, however, especially researchers working at the Texas experiment station. Scientists at that institution conducted lengthy trials that attempted to determine the vitamin A requirements of dairy cattle and the ability of crops commonly available in Texas to meet those needs. Instead

[260] I.L. Hathaway and H.P. Davis, "The Vitamin E content of Certain Dairy Feeds," *Nebraska Agricultural Experiment Station Research Bulletin No. 73* (1934).

of performing these tests directly using laboratory rats, these researchers employed actual cattle in a novel manner: to test the amount of vitamin A contained in a feed, they assayed the amount of vitamin A present in the cow's milk. They first established a baseline level of vitamin A contained in milk produced by cattle on a fixed ration. They then began varying the ration and calculated the amount of vitamin A present in the feed by the change in the amount of vitamin A present in the milk the animals produced.[261]

Unfortunately, these research methods also presented some obstacles. First, the scientists did not measure the effects of vitamin A directly by purposefully inducing visible symptoms of malnutrition but measured it indirectly via the cows' milk. Second, they still had to employ laboratory rats to measure the amount of vitamin A present in the milk produced. Thus, they could not entirely abandon the use of laboratory animals, but their experimental methodology flipped the contemporary orthodoxy on its head by starting with the cow rather than the rat.

In truth, scientists at agricultural experiments stations throughout the country – as will be discussed in the next chapter – attempted to measure the vitamin content of milk, but the majority of these researchers did so because they were interested in understanding the properties of milk, not in finding the vitamin content of cattle rations. For better or worse, scientists assayed the chemical composition of rations by experimenting on laboratory animals, and it remained the standard method of measuring vitamin content at

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[261] O.C. Copeland and G.S Fraps, "Sorghum Silage as a Source of Vitamin A for Dairy Cows," *Texas Agricultural Experiment Station Bulletin No. 473*, (1932). See also G.S. Fraps and Ray Treichler, "Vitamin A Content of Foods and Feeds," *Texas Agricultural Experiment Station Bulletin No. 477*, (1933) and Walter C. Russell, et al., "The Relation Between the Vitamin A Content of the Dairy Ration and Milk," *New Jersey Agricultural Experiment Station Bulletin No. 592* (1935)..

least through the Second World War. However, scientists seem to have only rarely tested the results obtained from laboratory animals on farm animals.

The discovery of (the importance of) vitamins thus required nutrition scientists to approach their work in new ways, and to adopt new research methods. Because contemporary scientist could not discover the chemical composition of vitamins they had to rely on other methods to identify their presence or absence. The earliest vitamin researchers, more concerned with identifying the existence and effects of these substances than with applying their new-found knowledge to farm animals, found that laboratory animals, and especially white rats, possessed desirable qualities.

Scientists who tried to determine how the existence of vitamins might increase dairy production continued to employ rats. Like their colleagues, they used the rats as qualitative instruments. For example, to test the vitamin A present in a fodder they would feed the ration to lab rats. The onset of the symptoms of malnutrition indicated a vitamin deficiency.

These researchers eventually also developed quantitative methods that allowed them to more accurately measure the amount of vitamins present in a ration. To do so they had to first determine what they might measure. For some vitamins they found that they could measure the weight gain in rats; in other cases, they would dissect the rats and measure bone density, the length of the femur, or other factors. To minimize the effect of outliers that would skew the data they experimented on groups rather than individuals. Doing so allowed them to employ statistical methods that produced at least a simulacrum of mathematical certainty.

By doing so they attempted to retain the sort of mathematic precision that had formed the basis of earlier nutritional science. Unfortunately, these researchers could not neatly analyze nutrients as had their colleagues of the previous generation; no longer could they merely measure the quantities of carbohydrates, proteins, and fats present in a ration. Instead, they had to measure elements that proved much more elusive to detect and resisted easy quantification. They therefore had to develop new approaches. While they eventually devised methods that would allow them to assign numerical quantities to vitamins.

Unfortunately, these were different sorts of quantities: they were relative, based on qualitative distinctions, and they were approximate, based on the use of laboratory animals. The unit commonly employed to quantify some vitamins reflects this uncertainty. The "rat-unit" widely employed by nutrition scientists did not represent a known quantity. Instead, it denoted the average variation in weight gain experienced by a group of "standardized" laboratory rats.

Of course scientists only reluctantly acknowledged that their quantitative methods lacked the precision of those employed by their predecessors. To do so would amount to a de facto admission of their shortcomings, which could call into question the validity of their authority. In effect, scientists had to maintain at least the charade of quantitative precision in order to maintain their privileged status as arbiters of progress.

CHAPTER 8

VITAMINS AND MILK

The “discovery” of vitamins – and the techniques adopted by scientists that allowed them to make sense of their findings, also opened new paths for the investigation of milk. Until scientists recognized the importance of vitamins they generally treated milk as a fixed commodity; cows might produce more or less milk, with a greater or lesser quantity of butterfat, depending on how well they were fed, watered, housed, and handled. However, these shifts were rather small except in the most dire conditions, and most researchers directed their efforts to determining how to maximize production while, at the same time, lowering costs.

Most milk investigations examined the properties of milk after it left the animal. Researchers endeavored to discover how best to pasteurize the milk, how to maximize the production of cheese or ice cream, or otherwise convert the milk into edible – or quaffable – food products. The discovery of vitamins required that scientists turn their attention from milk to the cow. Because – and until – scientists discovered the chemical makeup of vitamins they could not simply “enrich” milk to any desired nutritional level; instead, they had to determine how the vitamins in the fodder that cattle consumed found its way into the milk they produced.

The histories of milk and vitamins have long been intertwined. Before the outbreak of the Great War the Polish chemist Casimir Funk, a pioneer in nutritional research and the person who coined the term "vitamine," suspected the milk might prove an important source of vitamins. In his 1913 article "An Attempt to Estimate the Vitamine-Fraction in Milk," written in the very earliest days of the investigation of vitamins, Funk noted that "infantile beri-beri occurs when the children are fed by mothers suffering from beri-beri" and advocated research into the relationship between milk consumption and the occurrence of rickets, beri-beri, and other (mal)nutritional diseases. He continued by suggesting a three-prong approach for future research into the relationship between milk and vitamins: "(1) What is the normal amount of vitamines in milk of different species… (2) Is there a definite relationship between the amount of vitamines secreted in the milk and that ingested in the food?... (3) What effect have boiling and pasteurization on the vitamine content of milk?" Funk concluded by recognizing the difficulties that such investigations would entail, specifically the fact that until scientists could determine the chemical composition of vitamins they would have to rely on oblique methods of analysis: "the ordinary chemical methods for estimating vitamines can hardly suffice and attention at present must therefore be directed to colorimetric methods." Simply put, this meant that scientists could only test rations by feeding them to live subjects; the appearance of the symptoms of malnutrition indicated that the ration did not contain adequate amounts of one or more vitamins.[262]

The realization that milk might well contain significant amounts of necessary nutrients spurred further research. As might be expected, dairy scientists played an important role in these investigations; indeed, they seem to have considered

[262] Casimir Funk, "An Attempt to Estimate the Vitamine-Fraction of Milk," *Biochemistry Journal* 7 (1913): 211.

investigations into cows'milk to fall under their particular bailiwick. Funk's writing proved especially prescient: not only did his suggestions form the basis for the scientific investigation of milk for several decades, but his observation about the obstacles that researchers would encounter largely dictated how scientists went about their work. Dairy scientists actually followed a tripartite research plan that mirrored Funk's proposal: they tried to determine the amount of vitamins that milk "should" contain, they investigated the relationship between the nutrients ingested by cattle and the vitamin content of the milk that the cows produced, and they studied the effect of pasteurization and other processes on the vitamin content of milk. And, as Funk predicted, the lack of direct means of measuring vitamins forced scientists to develop novel quantitative methods.

As a result, an examination into the ways that scientists investigated the relationship between vitamins and milk allows an historically useful insight into both the methodologies and the aims of early twentieth century scientists. In particular, it illustrates two important processes. First, it shows how tightly scientists clung to the notion of quantitative analysis; for these men, "science" was, quite literally, "measuring." By quantifying the various inputs and outputs they hoped to deduce the fundamental principles that guided physical processes. The discovery of vitamins presented a unique challenge. Because they could measure neither the amounts of the vitamins nor their effects directly researchers had to develop alternate methods that would allow them to maintain at least a mirage of scientific accuracy even if the numbers and units they measured were, in fact, averages and abstractions. The tenacity with which these scientists clung to orthodox methods demonstrates their dedication to an increasingly obsolescent mindset.

Second, the discovery of vitamins challenged accepted notions of milk itself. Until this time, researchers typically regarded milk as a more or less fixed product. To be sure, individual animals produced milk that possessed a certain amount of butterfat and other solids, but scientists viewed this as a simple matter of input and output. Most scientists believed that the key to maximizing the profitability of dairy cattle lay in calculating the combination of feeds that maximized the production of milk and/or butter fat, depending on whether farmers sold milk for consumption or for conversion into butter or other products. In either case, they strove to devise rations that would produce the highest profit at the lowest cost, and they believed that doing so lay in formulating a ration that would affect the desired output.

The discovery of vitamins put a new spin on this formula. Scientists soon determined that cows' milk potentially contained large amounts of vitamins A and D. More importantly, they discovered that very few other constituents of the typical American diet supplied these nutrients in appreciable quantities. Researchers turned their attention to devising means of maximizing the production of these elements. In doing so, they made an important realization: milk was, to some degree, a plastic commodity. Researchers found that, in many cases, by varying the rations they fed a cow they could influence the amount of vitamins her milk contained. Furthermore, they eventually discovered that by irradiating the milk itself they could vary the amount of nutrients contained in milk after it had left the cow.

This amounted to something of a revolution in dairy science. Hitherto, scientists had attempted to maximize profitability. Depending on the market – whether milk was intended for consumption or for conversion to butter – researchers attempted to promote either the output of milk or the production of butter-fat. The realization that milk might contain additional nutrients caused scientists to alter their approach.

Scientists had to develop new strategies because they were, in essence, dealing with a new sort of product. Put another way, the discovery of vitamins changed the algebra of dairy science. No longer could researchers balance simple inputs and outputs – i.e. rations and milk – so as to find the point of maximum productivity. Instead, they had to juggle a myriad of factors, some of which they did not yet fully understand. The fact that some of these elements seemed, under certain conditions, to offset or cancel others added to the confusion.

In short, the discovery of vitamins amounted to the realization that milk was not a fixed substance but was, in fact, a plastic commodity. Researchers learned that they could alter the composition of milk produced by varying the ration they fed the animal. They could no longer feed simply to maximize total production of milk, but instead had to take into account a variety of other factors. Furthermore, scientists discovered additional ways of modifying the composition of milk. They believed that exposing the animal to certain wavelengths of light could affect the vitamin content of the milk that she produced. Finally, and perhaps most profoundly, researchers began to understand that they could alter the composition of milk even after it had left the cow. For example, exposing the milk, instead of the animal, to some forms of light could increase amount of vitamins it contained. Until this point, dairymen "processed" milk in the interests of sanitation or greed: pasteurization could destroy dangerous microbes and the adulteration of milk (by skimming the cream and/or adding water or more dangerous substances) increased profit. Milkmen could now add something of value to their product.

The realization that milk contained vitamins, and that the amount of vitamins could be varied, had important implications. Most obviously, milk played an important part of the diet for many Americans in the early twentieth century. The discovery that milk possessed large amounts of several vitamins necessary for human nutrition only increased its importance. Promoters of dairy products quickly seized on this fact and added it to advertisements of mild and other dairy goods.[263] So too did nutritional scientists; those studying human nutrition attempted to determine how humans utilized cows' milk, while animal scientists attempted to produce milk that would meet these needs – and, hopefully, increase the market for milk.

As importantly, however, the vitamin content of milk affected the livelihood of farmers. Though dairy farmers obtain most of their profit from the sale of the milk produced by their herds, most farmers also rely on the offspring of their cattle. At the very least, farmers hope that the heifers produced by their animals will someday join and then replace their mothers as producers of milk, and many dairymen realize a (sometimes not inconsiderable) profit from the sale of calves. As milk comprises the primary diet for these animals for the first months of their lives, maximizing the nutritional value of milk became very important for farmers. In addition, many farmers, especially those living in cheese-producing regions, used the skimmed milk to feed other animals, especially hogs. Like calves, milk formed an important part of the diet of these animals; by producing milk high in vitamin content farmers could help ensure the health of these animals and, in the process, realize additional income. Thus, for a variety of reasons, dairy scientists had

[263] For more on the marketing of milk as a complete source of nutrients, see E. Melanie DuPuis, *Nature's Perfect Food* (New York: New York Univ. Press, 2002).

ample incentive to discover how to maximize the vitamin content of the milk produced by American dairy cattle.

The relationship between vitamins and milk dated from the earliest days of vitamin research. As noted above, Casimir Funk, one of the pioneers of vitamin research, suggested that determining the vitamin content of milk and the relative nutritional values of human milk and cows' milk were tasks of immediate import, and gave a new impetus to investigations into the composition of milk. These were not new; Arthur V. Meigs of the University of Pennsylvania had begun investigations into the merits of human and cow's milk in the 1880's. He hoped to discover how to modify cow's milk to make it a more nutritionally complete food for infants, and published a book describing his findings in 1885. Meigs and his associates at the Robert Hare Chemical Laboratory of the University of Pennsylvania, along with a handful of other researchers on both sides of the Atlantic, focused their efforts on quantifying the composition of milk. To begin, they separated milk into the constituent components of protein, fat, carbohydrates, and minerals. They then paid special attention to the makeup of these components; for example, they separated the protein component into casein and globulin and then tested the mineral and ash content of each. Though these experiments became quite intricate, they reflected contemporary scientific practice, which suggested that scientists proceed by analyzing substances, breaking them down into their constituents, and then measuring the amounts of various elements present. Scientists believed that this process would eventually allow them to find the principles that governed the behavior of these substances. Central to this practice was the belief that the constituent elements of a substance behaved in, and reacted to each other, in a predictable manner; the scientist's task was to uncover the rules that regulated these processes. However, the discovery of vitamins challenged this methodological approach.[264]

Funk based his belief in the relationship between vitamins and milk on the observation that children nursed by mothers who suffered from nutritional diseased such as beri-beri often exhibited symptoms of the same disease. Other pioneers of vitamin research stumbled onto the connection between milk and vitamins when they tried to explain anomalies that appeared in their experiments with purified food substances. At the same time that Funk published his theories about milk, two pairs of American scientists, Thomas B. Osborn and Lafayette B. Mendel at Yale and McCollum and Davis at the University of Wisconsin were also working to discover a cure for nutritional diseases such as beri-beri and pellagra. Osborn and Mendel experimented by feeding rats basal diets consisting of simple rations consisting of one or, at most, a small handful of foodstuffs. They found that rats fed simple diets tended to exhibit signs of malnutrition rather quickly.[265] Elmer V. McCollum and Marguerite Davis experimented along similar lines, but instead fed rats purified rations. Contrary to expectations, some of their rodent subjects thrived, while others declined in health. Checking their methodology, the pair discovered that their method of purifying foods failed to destroy some previously unknown nutrient that proved essential for the maintenance of health. In McCollum's words, "Certain diets happened to contain just sufficient impurities of nutritional value to

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[264] For an overview of the work of Meigs's and his associates, see Edward B. Meigs and Howard L. Marsh, "The Comparative Composition of Human Milk and Cow's Milk," *The Journal of Biological Chemistry* 16 (1913): 147-166. See also Arther V. Meigs, *Milk Analysis and Infant Feeding* (Philadelphia, 1885).

[265] Thomas B. Osborne and Lafayette B. Mendel, "the Relation of Growth to the Chemical Constituents of the Diet," *Journal of Biological Chemistry* 15 (1913)" 311.

permit young rats to grow fairly well and maintain a reasonable standard of well-being when either butter fat…or egg-yolk, was included in the diet."[266]

At roughly the same time, the two teams, working independently, found that animal fat – and, specifically, the fat found in cows' milk - seemed to hold the key. Osborne and Mendel found that adding butter to the animals' diets quickly restored their health, while McCollum and Davis discovered that the addition of animal fats to the otherwise "purified" diets prevented malnutrition. The important discovery was that animal fats contained some element either not contained in vegetable fats, or not contained in sufficient quantity to prevent the onset of malnutrition. Dairy researchers jumped on this discovery; butter was the most important of the animal fats, and the properties of milk and other dairy products did, after all, fall under their scientific jurisdiction. They became even more excited when, in 1915, McCollum and Davis, combining their findings with the published work of army physician Edward Wright Vedder on beri-beri and Funk on the vitamin theory proposed the existence of a second, as yet unknown, substance crucial for nutrition. They theorized that this second substance, which they labeled "water-soluble B," to distinguish it from the earlier "fat-soluble A," prevented the onset of beri-beri.[267]

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[266] Elmer Verner McCollum, *A History of Nutrition* (Boston: Houghton Mifflin Co., 1957), 218. For their original research, see E.V. McCollum and Marguerite Davis< "The Necessity of Certain Lipins in the Diet During Growth," *Journal of Biological Chemistry* 15 (1913): 167-175 and "Observations on the Isolation of the Substance in Butter Fat Which Exerts a Stimulating Influence on Growth," *Journal of Biological* Chemistry 19 (1914): 245-250.

[267] McCollum, *A History of Nutrition*.

McCollum and his associates at the University of Wisconsin led the way in investigating the vitamin content of milk. In 1916 McCollum, Simmonds and Pitz published a paper entitled "The Relation of the Unidentified Dietary Factors, the Fat-Soluble A, and the Water-Soluble B, of the Diet to the Growth-Promoting Properties of Milk." Noting that "These substances [fat-soluble A and water-soluble B], or possibly groups of substances…are indispensable from the diet during growth," the researchers hoped to determine whether animals synthesized the elements from their fodder or whether they merely absorbed them and passed them on in their milk. They suggested the substances "possible formation within the maternal organism;" and hoped to test the possibility that "the gonads…or the mammary tissue, may be capable of producing one or both of these two dietary factors."[268]

To test their hypothesis the Wisconsin researchers experimented on rats. They compounded rations complete save for substances "A and B" and fed the rations to nursing rats with the belief that the "results should enable us to decided whether these two essential substances pass into the milk only when they are furnished in the diet." Their experiments revealed that the young rats nursed by mothers whose diet lacked one or the other nutritional substances failed to grow as quickly as rats nursed by mothers receiving a diet replete with both substances. The researchers were forced to "conclude that these two constituents of the diet pass into the milk only as they are present in the

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[268] E.V. McCollum, N. Simmonds, and W. Fitz: "The Relation of the Unidentified Dietary Factors, the Fat-Soluble A and Water-Soluble B, of the Diet to the Growth-Promoting Properties of Milk," *Journal of Biological Chemistry* 27 (1916): 33-45.

diet of the mothers." Furthermore, the scientists determined that the substances "cannot be formed within the animal body."[269]

Perhaps most importantly, however, the Wisconsin trio found that "milks may vary in their growth-promoting power when the diets of the lactating animals differ widely in their satisfactoriness for the growth of young."[270] In essence, though they did not follow up on this in their paper, the researchers acknowledged the plastic character of milk: by varying the feed supplied to nursing mothers (or lactating cattle), scientists (or dairymen) could vary the character of the milk itself. This discovery would eventually have a profound effect on dairy science; it marked a realization that animals do not produce a fixed product but instead are capable of producing different sorts of milk depending upon their ration.

These discoveries coincided with the work of dairy scientists, some of whom, like Osborne, also pioneered the work on vitamins that examined what they often referred to as the "new" – but distinctly non-vitamin - constituents of milk. For example, in 1916, Osborne and Wakeman, working at the Connecticut experiment station, determined that the phosphoric content of milk, which had long been known to scientists, stemmed largely from phosphatides, essential elements of the proteins contained in milk.[271]

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[269] Ibid.

[270] Ibid., 35.

[271] Thomas B. Osborne and Alfred J. Wakeman, "Some New Constituents of Milk, Second Paper," *Journal of Biological Chemistry* 28 (1917): 1.

Spurred by these findings, the following year the pair discovered a “new” type of protein in milk which they labeled the “alcohol-soluble milk protein.”[272]

In 1918 Osborne, once again working with his “vitamin” collaborator Lafayette B. Mendel, published a new paper on vitamins, “Milk as a Source of Water-Soluble Vitamin.” That same year, a pair of young researchers at the University of Wisconsin’s Laboratory of Agriculture Chemistry, H.H. Sommer and E.B. Hart published the results of their work on how temperature affected the citric acid content of milk. About the same time, another trio of chemists at the University of Wisconsin reported the results of their investigations into the “fat-soluble vitamine,” while their colleagues at the same institution revealed the findings of their experiments on the effect of heat on the antiscorbutic properties of milk products. Nor did Connecticut and Wisconsin possess a monopoly on research. Scientists at the Massachusetts General Hospital explored the “non-protein nitrogenous constituents of cow’s milk,” and researchers at the Laboratories at the Department of Health in New York studied the relationship between the salt content of dairy fodder and the antiscorbutic properties of cows’ milk.[273]

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[272] Osborne and Wakeman, “Some New Constituents of Milk, Third Paper,” *Journal of Biological Chemistry* 33 (1918): 243.

[273] Thomas B. Osborne and Lafayette B. Mendel, “Milk as a Source of Water-Soluble Vitamine,” *Journal of Biological Chemistry* 34 (1918): 537; H.H. Sommer and E.B. Hart, “Effect of heat on the Citric Acid Content of Milk,” *Journal of Biological Chemistry* 35 (1918): 313; H. Steenbock, P.W. Boutwell, and Hazel E. Kent: “Fat-Soluble Vitamine,” *Journal of Biological Chemistry* 35 (1918): 517; E.B. Hrt, H. Steenbock, and D.W. Smith, “Effect of Heat on the Antiscorbutic properties of Some Milk Products,” *Journal of Biological Chemistry* 38 (1919): 305; Denis and A.S. Minot, “The Non-Protein Nitrogenous constituents of Cow’s Milk,” *Journal of Biological Chemistry* 38 (1919): 543; Alfred F. Hess, L.J. Unger, and G.C. Supplee, “Relation of Fodder to the Antiscorbutic Potency and Salt Content of Milk,” *Journal of Biological Chemistry* 45 (1920): 229.

The findings of these investigations will be considered in turn, but the rapid proliferation of research on milk suggests the rapidly changing nature of these scientists' investigations. Until this time, most scientists regarded milk as a fixed commodity. To be sure, they had yet to unravel the complete chemical composition of milk, and the ways the various elements interacted with each other, but isolating and identifying those elements was simply a matter of time and laboratory expertise. Put simply, until this time, milk was simply milk. It might vary slightly in the percentage of butterfat or the number of bacteria it contained, but dairy researchers regarded these as stable and fixed. Only as the second decade of the twentieth century drew to a close did scientists begin to view milk as a somewhat dynamic substance, one that could be altered not only by varying the fodders fed to cattle, but also, in some cases, by chemical or physical procedures on the milk itself. In short, though they did not yet understand all the implications of their findings, scientists began to view milk in a whole new way, and began to re-evaluate virtually every constituent of milk in light of these new, and quickly evolving, findings.

The scientists' relative ignorance about the constituents of milk dictated that investigations into the vitamin content of milk and the presence of "non-vitamin" factors such as citric acid or calcium in milk continued apace. In fact, the findings of scientists investigating non-vitamin factors often pointed their colleagues in productive directions. The various groups of scientists working at the Agricultural Laboratory of the University of Wisconsin neatly demonstrate the sort of scientific symbiosis that marked this period of research.

In the late nineteen-teens H.H. Sommer and E.B. Hart, researchers at the University of Wisconsin, studied the citric acid content of milk. That cow's milk normally contained citric acid had been confirmed in the nineteenth century. A number of scientists, basing their assumptions on the fact that the juice of citrus fruits had long been known to alleviate the symptoms of scurvy, speculated that the antiscorbutic properties – or, in modern terms, the vitamin C content – of milk stemmed from its citric acid content. Furthermore, some reports suggested that heating milk destroyed the citric acid it contained. Sommer and Hart sought to verify whether "this fact might be used to explain, as is often claimed, why heated milk should be more conducive to the production of scurvy than raw milk."[274]

The pair proceeded by first confirming that milk as it comes from the cow does, in fact, contain citric acid. Having obtained a sample of milk and ascertained the amount of citric acid contained, they subjected the milk to increasing levels of heat in an autoclave. They found the even high levels of heat neither destroyed the citric acid in the milk nor transformed it into an insoluble form. In the end, heating milk did not diminish its level of citric acid.[275] Though Sommer and Hart did not elaborate on their findings, their research at least hinted that either citric acid did not supply the anti-scorbutic element or that heating did not alter the antiscorbutic properties of milk.

The following year Hart, this time partnered with H. Steenbock and D.W. Smith, attempted to determine whether heating milk destroyed the antiscorbutic elements it

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[274] H.H. Sommer and E.B. Hart: "Effect of Heat on the Citric Acid Content of Milk," *Journal of Biological Chemistry* 35 (1918): 313.

[275] Ibid., 318.

contained. First, they confirmed that a ration compounded solely of rolled oats and hay would induce scurvy in guinea pigs. Next, they ascertained that feeding raw cow's milk to guinea pigs suffering from scurvy would ameliorate the condition. They also found that "On a diet of rolled oats and hay the prevention of scurvy by the use of raw milk will depend upon the amount of raw milk allowed."[276]

Up to this point, they were merely confirming work done by others. Their innovation lie in then attempting to cure the scurvy-ridden guinea pigs by augmenting their ration with milk which had been sterilized at 120 degrees Celsius for ten minutes. Guinea pigs fed the sterilized milk showed no signs on improvement. Their results confirmed two separate hypotheses. First, it demonstrated that the antiscorbutic element was unstable and could be destroyed by heat. The researchers acknowledged this fact in their paper. Second, their experiment implicitly proved that citric acid contained in milk was not the source of its antiscorbutic properties. Hart and Sommer had, after all, found that heat had no effect on the level of citric acid in milk. However, the researchers were quick to warn that their work raised as many question as it answered: "The point of view that there does exist a relatively unstable antiscorbutic vitamine in our foods offers a satisfactory explanation of the prevalence of scurvy among infants fed milk of which the origin and heat treatment may have been variable. It opens for study the question of the variation in antiscorbutic vitamine content of milks produced under various conditions."[277]

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[276] E.B. Hart, H. Steenbock, and D.W. Smith, "Effect of Heat on the Antiscorbutic Properties of Some Milk Products," *Journal of Biological Chemistry* 38 (1919): 305.

[277] Ibid., 306, 314.

For many dairy scientists, the most important of these "various considerations" was the effect of different feeds on the vitamin content of milk. Nutritionists, dating back to the work of Casimir Funk, hinted at the possibility that the antiscorbutic quality of milk might be related the rations consumed by the animals that produced it. Though other scientists echoed this suspicion it remained a matter of conjecture until Hart and Steenbock of the University of Wisconsin, who had previous explored the antiscorbutic properties of milk joined their colleague N.R. Ellis to put the hypothesis to a scientific test.

The Wisconsin trio started with the belief, established by Steenbock the previous year, that air-drying crops destroyed whatever antiscorbutic properties they might have contained.[278] As part of other testing, the University of Wisconsin maintained in its dairy herd a number of animals fed exclusively on dried rations. Steenbock and company began by comparing the antiscorbutic content of milk produced by animals that spent several hours each day grazing in the university's pasture with the control group who received only dried rations. They found that the pasture-fed group produced milk produced milk that contained a larger amount of the antiscorbutic element. During the winter months, they compared the antiscorbutic properties of milk produced by the cattle receiving only dried rations and those in the greater herd that received, as part of their winter ration, root crops consisting primarily of sugar mangels and sugar beets. Again, the animals that consumed the dried rations produced milk lower in the antiscorbutic property, though milk produced by the group receiving roots contained less of the antiscorbutic compound than did they did during the summer while on pasture. The trio

[278] H. Steenbock, *Science*, 1919, I, 352.

concluded that “Manifestly then the diet may have a very pronounced influence upon the concentration of the antiscorbutic vitamine in the milk, being richer in this substance when the animals receive fresh green materials in the diet or ration than when the ration is made up of air-dried materials.”[279]

Later the same year a team of researchers working at the Bureau of Laboratories at the New York Department of Health published the findings of their own investigations into the relation between fodder and the antiscorbutic property of cow’s milk. This group made it clear that they had examined the work done at Wisconsin but noted that their own investigation “differs…mainly in a delimitation of the duration of the feeding periods, and in the inclusion of a chemical examination of the two varieties of milk.”[280]

They began by selecting five milking cows from the laboratory’s herd which had been receiving a farm’s “normal” ration of ensilage, hay, and concentrates and substituting a ration known to be deficient in its anti-scorbutic properties. The cattles’ production declined and their health began to deteriorate. After three weeks on this ration the scientists took a sample of the cows’ milk. They then put the cattle out to pasture. Though the production of the cows did not improve, their health did return. Again, after three weeks the scientists took a sample of the animals’ milk. In both cases the scientists dried the milk samples and then tested the samples on guinea pigs fed a ration compounded of the dried milk samples and a ration known to be lacking in its

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[279] E.B. Hart, H. Steenbock, and N.R. Ellis, Influence of Diet on the Antiscorbutic Potency of Milk,” *Journal of Biological Chemistry* 42 (1920): 389.

[280] Alfred F. Hess, L.J. Unger, and G.C. Supplee, “Relation of Fodder to the Antiscorbutic Potency and Salt Content of Milk,” *Journal of Biological Chemistry* 45 (1920): 229.

antiscorbutic effect. The guinea pigs fed the dried milk produced by animals whose diet contained no antiscorbutic elements developed signs of malnutrition, while those guinea pigs fed dried milk produced while the cows were on pasture maintained their health. The New York team believed that their findings confirmed those of the Wisconsin researchers, namely that the fodder provided to milking cattle could influence the antiscorbutic properties of the milk that they produced.[281]

In the decades following the realization of Funk, Osborne and Mendel, McCollum and Davis and others of the important role played by the nutritional elements that eventually became known as vitamins, research into the properties of the various vitamins and the "discovery" of new vitamins continued apace. For example, the existence of an "antiricketic" substance – which eventually received the moniker vitamin D – stemmed from the research performed by McCollum and a trio of scientists associated with Johns Hopkins University. The group had discovered that certain foods, known to be rich in vitamin A, tended to prevent the onset of rickets. However, when they assayed a wide variety of foods known to contain quantities of vitamin A, some of their test animals developed symptoms of rickets while others remained healthy. They eventually concluded that their "studies of the past year have revealed the existence of a distinct nutritive principle which has been confused hitherto with the vitamin A."[282]

Noting that "such results tend to discourage the acceptance of the interpretations as final which now appear as satisfactory to account for malnutrition of specific types," in

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[281] Ibid.

[282] E.V. McCollum et al., "The Production of Rickets in the Rat by Diets Consisting Essentially of Purified Food Substances," *Journal of Biological Chemistry* 54 (1922): 249.

1922 the quartet undertook a study of the etiology of rickets. Within a year they had shown that the onset of rickets resulted from the lack of a newly identified substance which they named vitamin D. Dairy scientists began to investigate the new vitamin, and in 1925 Steenbock – whose work on the antiscorbutic properties of milk made him a leader in the study of the vitamin content of milk – and A.L. Daniels published the results of their assay of amount of vitamin D contained in cow's milk. They found that produced under "normal" conditions contained only nominal amounts of vitamin D.[283]

However, this marked a beginning rather than an end of the study of study of the vitamin D content of milk. Scientists had long suspected a link between the rickets and sunlight; specifically, anecdotal evidence suggested that exposure to sunlight tended to prevent the onset of rickets. In 1923 a pair of British researchers, E.M. Hume and H.H. Smith, reported that laboratory rats "housed in previously irradiated glass jars containing sawdust" did not develop rickets. Though the pair at first believed that "irradiated air" might prevent rickets, they soon discovered that the sawdust held the key to the riddle: the rats which consumed irradiated sawdust seemed to have acquired a resistance to rickets. Other scientists soon confirmed this as well as the fact that exposing animals to sunlight or other irradiation also prevented the development of the disease.[284]

In addition to their determination that "normal" milk contained only small amounts of vitamin D, Steenbock and Daniels also found that exposing milk to radiation could increase the amount of vitamin D it contained. Steenbock and a team of colleagues

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[283] H. Steenbock and A.L. Daniels, Journal of the American Medical Association, 1925, v. 84, p. 1093.

[284] E.V. McCollum: *A History of Nutrition*, 285-286.

at the University of Wisconsin applied all these finding to their investigation of milk. The title of their report spells out the direction of their research: "The Antirachitic Property of Milk and Its Increase by Direct Irradiation and by Irradiation of the Animal."

The team operated from a pair of principles. They knew that irradiating milk could increase its vitamin D content. They were also aware that sunlight and other forms of direct radiation could prevent the onset of rickets. However, because irradiating milk was not "without its practical difficulties," they decided to determine the effect of irradiating the cow herself, a technique which they believed had "much to offer."[285]

The researchers' plan illustrates the extent to which the way that scientists viewed the world had changed. They no longer viewed any of the elements of their investigations as inherently fixed and immaleable. This marks a radical change in outlook. Before, scientists believed that their aim was determine how to maximize production and efficiency; that is, they wanted to figure out how to produce the most milk at the lowest cost. Since researchers believed that milk, feed, and cattle were basically "fixed" elements with static characteristics, their approach to their work was essentially algebraic. Scientists first determined which elements were important and then constructed methods that allowed them to measure these fixed discrete quantities. In a nutshell, the application of science consisted of juggling the inputs – in this case, feed and cattle – in order to produce the maximum amount of the desired output – milk – at the lowest price.

[285] H. Steenbock et al., "The Antirachitic Property of Milk and Its Increase by Direct Irradiation and by Irradiation of the Animal," *Journal of Biological Chemistry* 66 (1925): 441.

The Wisconsin researchers' plan shows that the discovery of vitamins fundamentally altered their approach. Their knowledge of the "anti-rachitic" vitamin (vitamin D) suggested that feed, cattle, and milk were not, in fact, fixed elements, but substances that could be manipulated in order to affect desired ends. For example, "orthodox" understanding suggested that the amount vitamin D contained in milk was dictated by the fixed amount of vitamin D contained in the food and the animal's efficiency in transferring the vitamin to the milk she produced. The new, "plastic," approach suggested that vitamin D was not, in fact, fixed at all but could be manipulated in a number of ways: by irradiation of the feed, of the milk, or of the animal herself.

In 1925 the Wisconsin team decided to perform a series of experiments intended to determine whether irradiating animals or milk with ultraviolet radiation produced an increase in the amount of vitamin D in the milk or in the milk produced by the irradiated animals. They began by testing the anti-rachitic properties of irradiated dairy milk, and found that exposing milk to ultraviolet radiation increased its anti-rachitic effect. Having ascertained to their own satisfaction that irradiating milk proved an effective means of increasing its vitamin D content, they turned their attention to the irradiation of animals. They chose to experiment with goats, noting that "a goat is obviously a more convenient animal than a cow."[286]

The scientists proceeded by exposing a pair of young milking goats to light produced by a quartz mercury vapor arc lamp. They subjected the animals to this light for thirty minutes per day, though the distance between the goat and the lamp varied "depending on whether the animal was standing up or lying down." They fed milk

[286] Ibid., 444

produced by the irradiated animals to rachitic laboratory rats. Rats fed milk produced by the goats exposed to the arc lamp showed more rapid improvement than did rats fed milk produced by animals not exposed to radiation. As a result of these experiments, the Wisconsin scientists concluded that exposing milk to a quartz mercury vapor lamp increased the antirachitic properties of the milk by "eight or more times." Furthermore, an increase in the antirachitic effect of milk could "also be induced rather promptly, though to a lesser degree, by direct irradiation of the animal.[287]

Unfortunately, later research called into question whether exposure of dairy cattle to ultraviolet light could increase the antirachitic properties of the milk she produced. The Wisconsin team believed that they had demonstrated that irradiating goats influenced the amount of milk that they produced, and later research confirmed these findings: "There appears no question but that the antirachitic potency of goats' milk can be increased by irradiation of the goat…"[288] However, debate continued about whether cattle reacted to irradiation in the same manner.

Early reports by scientists working at various agricultural experiments stations suggested that irradiating cattle did in fact influence the quality of her milk. J.W. Gowen and his associates working at the Maine agricultural experiment station believed that they had proved that exposing cattle to radiation increased the antirachitic properties of the milk they produced.[289] Dutcher and Honeywell of the Pennsylvania station studied the

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[287] Ibid., 449.

[288] Goat findings confirmed in H. Steenbock et al., "The Antirachitic Value of Cow's Milk as Modified by the Feeding of Irradiated Yeast," *Journal of Biological Chemistry* 88 (1930): 197.

antirachitic content of butter rather than milk, but also believed that they had demonstrated a link between radiation and the antirachitic property.[290]

Unfortunately, the work of other scientists, led by researchers in Germany, called these results into question. After surveying the various claims and counter-claims, the Wisconsin team decided to perform a new series of experiments which hoped would settle the matter once and for all. They reiterated their conclusion that "irradiation, at least with they goat, produced a marked increase in the antirachitic properties of milk…As a result…we were very optimistic over the possibilities of improving cow's milk in a similar manner."[291]

They conducted a series of trials involving the exposure of dairy cattle to both sunlight and to the radiation produced by a quartz mercury vapor lamp in an effort to establish whether exposure to these radiations might increase the antirachitic properties of milk produced by the cattle. They performed six separate trials each involving three or four animals. Unfortunately, their results contradicted their earlier hopes: "Daily exposure of cows to sunlight or artificially generated ultraviolet radiations has little if any effect on the antirachitic potency of milk…No improvement in milk or butter fat secretion was observed."[292] However, even this "negative" result increased knowledge.

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[289] J.W. Gowan, "Studies in Milk Secretion XV," *Maine Agricultural Experiment Station Bulletin No. 328* (1925).

[290] R.A. Dutcher and H.E. Honeywell, "40th Annual Report," *Pennsylvania Agricultural Experiment Station Bulletin No. 213* (1927).

[291] H. Steenbock et al., "The Antirachitic Value of Cow's Milk as Modified by Exposure of the Cow to Sunlight and the Radiations from a Quartz mercury Vapor Lamp," *The Journal of Biological Chemistry* 87, 104.

Having proven to their own satisfaction that irradiating animals did not increase the vitamin D content of their milk allowed them to turn their attention to the other possibilities: "The well recognized superior quality of summer-produced milk and butter fat must therefore have its primary origin in other factors than sunlight acting directly upon the cow."[293]

When irradiating animals directly failed to influence the amount of vitamin D in milk produced by the animal, scientists focused their efforts to increase the antirachitic properties of milk on the irradiation of the rations fed the cattle and on the irradiation of the milk itself. Though both methods showed promise, dairy scientists dedicated most of their attention to the possibilities of irradiating feed. The link between sunlight and the amount of vitamin D contained in a food had long been suspected; several teams of researchers found that feeding summer alfalfa caused an increase in the amount of vitamin D contained in the milk of animals who consumed the grass.

Scientists quickly determined that irradiating yeast vastly increased its vitamin D content, and that feeding yeast to milking cattle increased the amount of vitamin D in their milk. Researchers began a series of trials to determine whether feeding animals irradiated yeast would in any negative effects, such as lower production of milk or butter-fat. They found that feeding even large amounts of yeast, containing more vitamin D than the animals could assimilate, had no effect on the cows' output. A team working at the University reported that: "50 gm. Of irradiated yeast fed to cows were found to increase the antirachitic potency of milk. Even 200 gms of yeast did not lower the milk

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[292] Ibid., 125

[293] Ibid.

production nor did it decrease the butterfat content" and conclude that "It appears that the feeding of a standardized irradiated yeast may be considered as a practical measure for the production of milk of standard antirachitic potency."[294] Furthermore, consumption of irradiated yeast over long periods did not seem to harm the animals: "Milk production was well sustained during the 8 months of irradiated yeast feeding, and there was no indication of disturbed physiological functioning during this period."[295]

Investigations of vitamin D also highlighted the scientists' relatively primitive, though rapidly increasing, knowledge of how cattle – and other animals - utilized other components of their rations. For example, researchers had determined that foods naturally containing large amounts of vitamin D also usually supplied significant quantities of elements such as phosphorous, calcium, and copper. However, they found that while irradiating yeast boosted the amount of vitamin D it contained, it did not seem to lead to a measurable increase in the amounts of what scientists had often regarded as associated, if not related, elements.

The experience of scientists at the University of Wisconsin exemplifies this process. They had demonstrated that irradiating yeast increased the amount of vitamin D it contained, but not the amount of calcium. Milk produced by animals fed the yeast showed the same properties: the amount of vitamin D increased while the amount of calcium remained fixed. However, the researchers found that laboratory mice fed the milk showed increased levels of both vitamin D and calcium. Though initially baffled by

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[294] Steenbock et al., "The Antirachitic value of Cow's Milk as Modified by the Feeding of Irradiated Yeast," *Journal of Biological Chemistry* 88, 213.

[295] E.B. Hart et al., The Influence of Irradiated Yeast on the Calcium and Phosphorus Metabolism of Milking Cows," *Journal of Biological Chemistry* 86 (1930): 155.

these results, they eventually determined that vitamin D helped the animals to assimilate more of the calcium present in their rations. These results highlighted the "plastic" nature of nutrition: the absorption of calcium stemmed not only from amount of calcium supplied in feed, but could be stimulated by the presence of vitamin D.[296]

By the mid-nineteen-thirties nutritional scientists had identified at least six vitamins: A (the "growth" vitamin), B (anti-neuritic, and by that point usually treated as a combination of related elements rather than a single substance), C (anti-scurvy), D (anti-rachitic), E (anti-sterility) and G (another anti-neuritic, sometimes identified as vitamin B2). Of this group, vitamins A and D were of the most interest to dairy researchers. Though milk contained small amounts of all of the vitamins, it contained fairly large quantities of A and D.

More importantly, while the amount of the other vitamins contained in milk seemed to be relatively fixed, researchers found that by varying feeds they could manipulate the amount of vitamins A and D contained in the milk. This marked a significant conceptual change. Until this point, scientists and dairy farmers had regarded milk as a "fixed" commodity; they believed that the physiology of the individual animal determined the character of her milk. Therefore, feeding became simply a matter of compounding a cattle ration that maximized milk production at the lowest possible price. In addition, farmers should ideally feed only the amount of feed that maximized production and provided enough calories to maintain her weight and health and allow her to produce a healthy calf. Thus, feeding became a exercise of economy and conservation: feed enough to maintain production and health and no more.

[296] Ibid., 151.

The discovery of vitamins upset this approach. It transformed milk from a fixed to a plastic commodity. Until this point, feeding had been a matter of minimizing cost while maximizing production. While scientists maintained that the physiology of the cow fixed the quality of her milk, this approach made economic sense. The discovery of vitamins upset this notion by introducing a set of variables that could be manipulated toward desired ends. The fact that milk could furnish relatively large amounts of vitamins A and D – both of which scientists had determined to be vital to human nutrition – made research into this phenomena important to dairy scientists and, eventually, dairy farmers.

Though this chapter has concentrated on the efforts of scientists to understand exactly how cattle rations affected the vitamin content of milk produced by the animals who consumed them, feeding manuals quickly conveyed these new findings to dairymen. The twentieth edition of Feeds and Feeding – by far the most popular and important of these feeding manuals – made the relationship between the vitamin contents of feed and milk explicit: "Recent investigations have shown that the vitamin A content of milk depends on the supply of the vitamin provided in the ration of cows."[297] Furthermore, "One of these methods [of producing milk high in vitamin D] is the feeding of irradiated yeast to the cows."[298] The relationship between and fodder and vitamins therefore became more than a laboratory curiosity but an issue that dairymen had to address.

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[297] F.B. Morrison, *Feeds and Feeding*, Twentieth Edition (Ithaca, NY: The Morrison Publishing Co., 1937), 127.

[298] Ibid., 131.

Hence the discovery of vitamins had two important ramifications to dairy research. First, as discussed in the previous chapter, the fact that the chemical composition of vitamins remained a mystery for decades dictated that scientists adopt new research methods. No longer could they simply identify and quantify vitamins. Instead, they had to devise new measures. Furthermore, since they could not measure the amounts or effects of vitamins directly they were forced to measure them by their affect on laboratory animals. This in turn required them to adopt new mathematical techniques, and, in particular, the use of statistics, in order to make sense of their findings.

Second, as shown in this chapter, the discovery of vitamins meant that scientists had to think about milk in a fundamentally different way. No longer a fixed commodity, scientists learned that they could manipulate milk in certain important ways. Most importantly, they could vary the amount of vitamins A and D contained in milk by their choice of fodder. Henceforward, milk production became more than a matter of producing the most milk at the lowest cost. It became, instead, a balancing act, in which scientists and farmers tried to maximize a number of factors: production, cost, and the milk's content of various nutritional factors.

CHAPTER 9

BREEDING BY THE NUMBERS

Animal nutritionists were not alone in adopting statistical techniques to make sense of their findings; geneticists, too, began to explore the use of similar methods to understand why mating superior animals often, or even usually, failed to produce high-quality offspring. As discussed in chapter five, an earlier generation of researchers had employed two methods of ranking animals: production testing and type, or conformation, testing. They believed that employing these tests would not only identify valuable animals, but, as importantly, reproduce animal with desirable characteristics.

Unfortunately, researchers and farmers alike noted that the offspring of superior animals usually reverted back to the group norm; only rarely did the progeny of excellent cattle out-produce their parents. Scientists puzzled over this phenomenon, and proposed numerous solutions. Yet, despite their efforts, their efforts at producing superior cattle failed more often than they succeeded.

A number of researchers, led by Sewall Wright, came to believe that the application of the mathematical techniques of statistics and probability to Mendelian genetics offered not only an explanation for these failures but suggested possible solutions to the dilemma. This chapter examines the career of Jay L. Lush, who more than anyone else applied these tools to dairy cattle. Like the scientists who worked with vitamins, Lush's employment of these methods marked a sharp break from the past. But, as in the case of vitamins, Lush's theories were adopted not only by fellow researchers but by dairymen, and his contributions form the basis of modern dairy breeding methods. His example once again demonstrates the widespread acceptance of scientific authority and how completely dairymen had come to employ the latest findings of dairy experts.

Discussing his own introduction to animal breeding in the years before the Great War, animal scientist Jay L. Lush reminisced: "I remember being told that the first principle of animal breeding was: like produces like; while the second principle was: like does not always produce like!"[299] He noted that while "By countless centuries of trial and error, certain practices had come to be recognized as generally a bit more successful than others in producing animals more like the breeder's desire…only a little was known about *why* things happened as they did."[300]

Lamenting the fact that "the art of animal breeding was far in advance of science,"[301] Lush spent the bulk of his professional scientific career trying to discover and establish the scientific principles of animal breeding. Beginning in the late nineteen-teens Lush published numerous articles that examined animal breeding from a variety of perspectives, from the existence of "double ears" in Brahma cattle and the genetic history of Poland-China swine to "a herd of cattle bred for twenty years without new blood."[302]

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[299] Jay L. Lush, *Genetics in the 20th Century*, ed. L.C. Dunn (New York: The Macmillan Co., 1951), 496.

[300] Ibid., 493. Italics in original.

[301] Ibid.

[302] Jay L. Lush: "A Herd of Cattle Bred for Twenty Years without New Blood," *Journal of Heredity* 25: 209.

Lush's proved ecumenical in his research interests: no matter how trivial a genetic trait, he subjected the object of his study to scientific analysis.

Lush published the first edition of his *Animal Breeding Plans* in 1937. The book served at once as an introduction to animal genetics and breeding and as a summation of his work to that point. To that end, the work included sections on Mendelian inheritance, the genetic basis of variation, and the (often hard to distinguish) effects of heredity and environment as well as a chapter on "Topics Relating to Reproduction" which touched on "Hermaphroditism and Other Abnormalities Pertaining to Sex" as well as the more mundane subjects of sex ratios and gestation periods.

However, *Animal Breeding Plans* differed from contemporary introductory genetics texts in three important aspects. First, Lush's unabashed recognition that "Animal breeding is a business." While most other animal scientists maintained a careful distinction between the laboratory and the university or experiment station's "ideal" farm and the conditions faced by farmers in the "real" world, Lush instead "sought to state the most probable truth concerning questions which may guide [the practical breeder's] actual decisions." Noting that "the scientist," when faced with difficulties, "might retire to his laboratory and design an experiment which in due time would reveal the truth," Lush also realized that "the man engaged in the business of animal breeding cannot wait for that." Lush's frank statements about his desire to produce a work useful to animal breeders rather than a strictly scientific treatise placed his work at odds with most of those published by his contemporaries.[303]

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[303] Jay L. Lush, *Animal Breeding Plans* (Ames, IA: Collegiate Press, Inc., 1937), v, vi.

Second, though Lush, like other geneticists of the time, and especially those, like Lush, who had studied with Sewall Wright, relied heavily on the use of statistical methodologies and techniques to explain the processes and ramifications of animal breeding, he employed them in a rather different way then most of his contemporaries. Specifically, Lush, throughout his career, maintained a special interest in what was meant, when employed by scientists or by farmers, by the concept of breed, and employed statistical methods primarily to answer this question. In the Introduction to *Animal Breeding Plans* Lush explained, "After all, a breed is a population." Therefore, "any attempt at precision in discussing methods of changing its characteristics must necessarily be phrased in terms of the measurements of populations." In short, this meant that any scientific discussion of "breeds" must be couched "in terms of averages and of variability."[304]

Lush employed this mathematical understanding of "breed" to two ends. On one hand, he tried to build a statistical underpinning for the widely used – especially by breeders – "fractional" language used to describe cattle. For example, many breeders would (and still do) refer to a cross of an Angus and a Hereford as "half-Angus, half-Hereford." Similarly, if they bred this cross to a "pure" Angus, the resulting offspring would be described as "three-quarters Angus, one-quarter Hereford." Lush hoped to provide a more scientific basis for the use of this terminology.

As importantly, Lush attempted to understand – and again, provide a more rational explanation of – the differences between animals that most people, scientists and

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[304] Ibid., v.

laymen alike, refer to as “breeds” or “breed characteristics.” He hoped to determine at what point of inbreeding, whether strictly between parents and offspring or among a small, closed group, genetic characteristics became more or less “fixed.” Like most of his research, Lush’s inquiries had an economic as a well as a scientific basis. Specifically, Lush wondered whether farmers should buy animals with specific, desired traits – for example, a Jersey or Holstein cow – or if they might more economically breed their own cattle from grade (or mixed-breed) stock. Buying pure-breed animals certainly saved time and offered some assurance as to the animals’ traits, but purebred animals often sold at a (sometimes significant) premium.

Third, again unlike most of his contemporaries, Lush spent most of his career studying animals that reproduced slowly and thwarted easy genetic understanding due to the large number of chromosomes they carried. In particular, he examined the genetics of horses and, especially, dairy cattle – animals that posed a number of obstacles to easy genetic study. These animals were, compared to most others, expensive to purchase; as a result few farmers or research institutions could afford to dedicate large numbers of creatures to long-term – and quite possibly unprofitable – study. Besides the initial cost of such animals, they also matured slowly: most cattle and horses became productive only at two or three years of age, and until this point represented a net loss for their owners. In addition, horses and cattle required a long period of gestation and produced small (usually one, occasionally two, and very rarely more) litters. All of these obstacles combined to make the study of horses and cattle very different than that of other, less-valuable animals - such as fruit-flies, gerbils, poultry, or even swine – that matured quickly, reproduced in a relatively short time, and produced large litters.

Hence the emphasis that Lush placed on *Animal Breeding Plans*: more so than most other animals, breeding cattle and horses required considerable foresight. Though the breeder of poultry or hogs could quickly determine whether the cross of two animals produced desirable offspring, the biological – and economic – realities of cattle and horse breeding dictated that animals might well be culled or sold before one could determine the merit of the animal's offspring.

Therefore *Animal Breeding Plans* is, for the most part, exactly that: an examination of the history, merit, and genetic implications (as Lush understood them) of various plans of animal breeding. Consistent with his interest in the definition of animal breeds, Lush devotes a goodly portion of his book to an understanding of the role of animal registry clubs, the means by which mixed-blood animals might be bred with pure-bred animals to qualify them for inclusion in the animal registries, and the potential results that might be obtained by such breeding plans. Lush also pays considerable attention to the ramifications of breeding animals to other, closely related creatures including "inbreeding" (breeding mother to son, or father to daughter) and "line-breeding" (breeding within families but not as close as inbreeding, i.e. grand-sire to daughter, uncle to niece, etc.).

Lush also considered the various testing means than in wide use as methods whereby to select the best animals and suggests breeding plans that should, in theory, maintain those desirable characteristics as long as possible within the herd. To this end he examined production tests that measure the milk and butterfat output of cow sand progeny tests that measure the production of sons and daughters. Lush also looks at visual assessment means such as conformity tests by which trained judges determine how closely a specific animal visually conforms to that breeds "ideal" and the relationship between performance in the "show-ring" and actual production and profitability.

However, none of the breeding programs Lush examined proved absolutely reliable. Though progeny testing offered the best means by which to determine which animals produced desirable offspring, such tests, even under the best conditions, took more than half a decade to produce any truly useful conclusions: not only the 2 to 3 years for the animal under consideration to mature but a similar length of time for her first offspring – if female – to themselves begin producing milk. Lush studied several plans aimed at hastening this process, such as trying to determine the correlation between visual testing of young animals and their eventual actual production, but such tests proved suggestive, rather than reliable, at best and, in most cases, inconclusive.

It is exactly in these failures that Lush proves most interesting to the historian for, in the end, Lush offered his readers something of a canard: despite his best efforts, none of Lush's plans offer the animal breeder anything truly novel. His conclusions that progeny testing offered the best means by which to assess the value of animals as capable of siring productive offspring was already in wide use by progressive breeders, and the (often serious) drawbacks of other breeding or evaluation plans had been pointed out by other authors. True, Lush did subject these various plans to rigorous scientific testing by keeping accurate records, employing double-blind tests, and other means, but his conclusions differed little from those found by other researchers and animal breeders.

What Lush did do was put the whole business on a firm scientific basis. For example, he drew on his knowledge of statistics and Mendelian genetics to demonstrate the imprecision of such terms as "half-blooded" as well as demonstrate that widely used concepts such as “breed” were, in fact, as much statistical construct as concrete fact. He also, though not by any means the first to do so, explained that widely known genetic anomalies such as “reversion” and “atavism” occurred according to laws of Mendelian genetics and not because of the quaint means to which they had been attributed in the past.

He also consistently reminded his audience of the limitations of breeding plans, and most specifically the fact that until (and if) scientists devised some means – short of actual breeding - of distinguishing dominant from recessive genes that breeding would remain something of a crapshoot. To this end, Lush, throughout his career, paid more attention than most geneticists to animals in their entirety rather than concentrating on the handful of factors – for example, the size of the udder or width of chest of dairy cattle – that most directly contributed to production. Instead, Lush believed that seemingly trivial traits such as the shape of animal’s ears, or the presence of distinctive coloration might be linked to – and act as a marker or predictor for – more desirable characteristics. By doing so Lush provided a valuable service not only by reminding breeders of the large number of genetic factors made animal breeding unpredictable but also that these scientists did not understand how the various genetic factors were linked. Despite his advocacy of, and extensive studies into, a Mendelian understanding of genetics and breeding, Lush also consistently admitted the fact that “scientific” animal breeding was only in its infancy, and much work remained to be preformed before it might offer consistently reliable breeding information.

But, in the end, Lush’s contributions amounted to style rather than substance. By employing the terminology of statistics and mathematics Lush changed, or at least influenced, the language and terms of the debate. Unfortunately, he ultimately failed to produce the foolproof breeding plans he so obviously hoped to discover. This should not condemn him, or his life-work, as a failure: seven decades after the appearance of his work cattle breeding remains something of a crap-shoot despite advances, such as the use of artificial insemination, that allow scientists and breeders to more quickly discover the "real-world" worth of animals who, at least on paper, seem to offer the best hope of producing desirable offspring. And, to be sure, Lush's efforts to promote a scientific understanding of breeding and genetics, and to dispel the myriad wives'-tales and backwoods rumors did much to advance rational breeding alone justify his work.

Ultimately, Lush failed exactly because he was a product of his time. Like most scientists, his training and approach to science dictated how he understood the world. For his generation, this often meant a faith in the powers of statistical mathematics to reveal scientific "truth." Lush diligently applied these methods to his own research. Unfortunately, re-casting the debate in new terms and applying new techniques did not provide truly new results, but rather the same results called by new names and couched in new terms.

Lush outlined his vision of the possibilities of genetic research in his first publication, "Inheritance in Swine," which he wrote while completing his graduate education at the University of Wisconsin. Lamenting the demise – because of America's entry in World War I – of an extensive swine-breeding experiment conducted at the Kansas Experiment Station, Lush hoped to glean some knowledge from the curtailed program, which "was undertaken with the idea of securing information about the inheritance of all the well-defined characters which differed in the breeds used."

Unfortunately, "conclusions have only been reached in regard to the shape of the face, set of ears, color, growth factors, and litter size." Happily, "the experiment was not a complete casualty" because "four years of breeding make a few conclusions almost inescapable and point to others which can perhaps be verified, one at a time…"[305]

Lush proceeded to enumerate the results generated by the experiment. He looked at data obtained about the litter size produced by various breeds and crosses, the set of the ears and shape of the face, the coloration, and differences in growth. Throughout his research Lush hoped to determine which animals possessed dominant or recessive genes, and whether this knowledge might allow breeders to produce more desirable animals.

However, his results proved inconclusive. For example, his research suggested that, as regards the size of litters, "the wild [hog]…is dominant" but, unfortunately, "tells us no more about its inheritance."[306] Similarly, the findings of the Kansas station found that the "typical erect ear of the Berkshire is dominant by at least one and probably not more than three…principal factors" when crossed with Durham hogs, but "Neither breed is homozygous thoroughly for all the factors concerned I the production of its own peculiar ear shape." The difficulties Lush faced in drawing conclusions stemmed from a number of factors: incomplete information, the shortened nature of the experiment, and anomalous results that did not offer easy explanation.

More importantly, however, Lush's difficulties demonstrate the subjective nature of genetics. For example, Lush attempted to interpret the data obtained about the face shape of various crossbreeds. He distinguished three characteristics to help him to

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[305] Jay L. Lush, "Inheritance in Swine," *Journal of Heredity* 12 (1929): 57.

[306] Ibid., 57.

classify the various faces: the shape of the forehead, the "dish" of the face, and the length of the face. However, the decision to classify or sort the various animals ultimately required making a subjective decision: "Since no practicable means of measuring these three elements of face shape was discovered, they were simply classified according to their resemblance to one or the other of the grandparental types."[307]

Despite the lack of objective means by which to gauge the animals, Lush believed that the experiment still yielded useful results. For example, despite the fact that "the characteristics are not definitely measurable ones," and that "the variation from one extreme to the other was by no means continuous" suggested that "the number of phenotypes which result from this cross is quite limited." Lush stuck to his beliefs even though some animals exhibited anomalous characteristics, suggesting that their presence was due to "developmental or anatomical rather than genetic" factors.[308]

In making these assertions Lush outlined two of the main difficulties that faced genetic researchers. First, that without the ability to determine which gene or genes affected which characteristics researchers had to make a subjective judgment not only about which traits to measure but, as importantly, how to measure them. In the case of the face shapes, Lush and his fellow researchers selected a number of face types and then attempted to fit the various animals into one or another classification. Second, when the offspring of animals failed to fit neatly into one or other of the predicted classifications Lush – rightly or wrongly, but with little evidence either way – shifted the blame from genetics to other factors.

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[307] Ibid., 60.

[308] Ibid., 60-61.

While on one level this might seem the antithesis of "science," it was, in fact, science as Lush understood and practiced it. For Lush, the scientific process progressed by exactly the sorts of hypotheses he advanced; it remained for later research, by himself or others, to prove, disprove, or modify his assertions. He stated this plainly in the conclusion to his article: "It is not believed that all the hypotheses which have been tentatively advanced in this article will stand in every detail without amendment the test of further research." One can plainly see that, as Lush pursued his scientific goals, hypotheses were merely stopping points on the way to truth, not ends in and of themselves. He concluded that if his hypotheses "awaken enough interest to stimulate further research…they will have served a very useful purpose.[309]

After his time at Wisconsin Lush took a position at the Texas agricultural experiment station. While there he published a number of papers that introduced themes that would form important parts of *Animal Breeding Plans*. Lush used the first paper he wrote while a member of the Texas station, "An Hereditary Notch in the Ears of Jersey Cattle," to attack the Lamarckian notion – evidently still apparent in some quarters – that offspring could sometimes acquire the acquired physical traits of their ancestors. In this case, Lush examined a herd of Jersey cattle, many of whom possessed notched ears that bore a striking resemblance to the ear notches used by some cattlemen to identify animals belonging to their herd.[310]

Lush discovered that all the animals with the distinctively notched ears were descendents of one bull, who himself possessed notched ears. Though Lush at first

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[309] Ibid., 71.
[310] Jay L. Lush, "An Hereditary Notch in the Ears of Jersey Cattle," *The Journal of Heredity* 13 (1930): 8.

suspected that “the condition could be regarded as accident of development…due either to…a doubling of the ear or to a blocking of the blood vessel” the fact that “so many of his offspring possess this notch” left “no doubt that this result is due to definite hereditary factors and not to mere prenatal accidents.” [311] Further consideration led Lush to posit that a single dominant genetic factor explained the anatomical anomaly, though he noted, “Further proof of this simple hypothesis waits upon the production of more calves by [the bull] and more calves from his daughters with the notched ears.”[312]

However, Lush used the example of ears notched due to some genetic fluke as a springboard to propose a much more interesting notion, one that would echo throughout his later work: namely, that trivial genetic characteristics might, through some as yet unknown genetic linkage, signal the presence or absence of more (or less) desirable characteristics. He explained “There is no intrinsic economic value in this notch just as there is none in the peculiar color marking of different breeds, but it is possible that other factors economically important, but difficult and expensive to trace, such as the factors for high milk and fat production, may be linked quite closely to the factor for the notch.” Lush believed that further study of genetically complex animals would eventually allow breeders to make the same sort of accurate predictions about the characteristics of offspring as his contemporaries who studied simpler creatures such as Drosophila, or fruit flies. He posited that such knowledge would have two effects. First, that the genetics of large animals differed only in complexity – rather than type – from smaller, simpler

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[311] Ibid., 9.

[312] Ibid., 10.

creatures, and that, furthermore, this might eventually prove a boon to animal breeders: "the idea will be dispelled that domestic animals are a law unto themselves, untouched by the knowledge gained form the study of insects and rodents in genetics laboratories." Second, that the study of "unimportant" characteristics was as valuable as the study of more economically important factors: "Progress in livestock breeding in the near future will depend as much on the study of all classes of characters, regardless of whether they economic or non-economic, as on an intensive study concentrated on one or two particularly valuable traits."[313]

Lush's paper "'Double Ears' in Brahma Cattle," published roughly a year after his investigation of the notched ears found in a herd of Jersey cattle, reiterated Lush's beliefs. Lush used the paper, subtitled "Another Case of Apparently Simple Mendelian Inheritance in Farm Animals," to again make his case for the study of all – rather than merely those considered economically valuable – genetic factors of livestock. Again, he suggested that without further knowledge it would be impossible to learn whether – and, if so, which – trivial traits were linked with other, more economically valuable characteristics. He wondered "May it not be that [with further study] it would be found…that the useless character was accidentally associated with others which did have a high value in natural selection?"[314] Furthermore, Lush posited that this knowledge might serve both economic and scientific ends: "…it seems to the writer that the evidence is sufficient to add this to the growling list of characters found to be inherited in a

[313] Ibid., 13.
[314] Jay L. Lush, "'Double Ears' in Brahma Cattle," *The Journal of Heredity* 15 (1932): 96.

Mendelian manner in our domestic animals. Each addition to this list seems to the writer to make it more probably that all or nearly all of inheritance is Mendelian…"[315]

Lush's investigations of the Brahma cattle that some Texan cattlemen experimented with due to their resistance to heat and, more importantly, their resistance to "tick-fever" revealed the widespread use of fractional terminology to designate the amount of Brahma ancestry in an animal – most typically, in bulls offered for sale to producers of other breeds who hoped to introduce some of the advantages of Brahma cattle into their herds. Lush found such fractions as "three-quarters" and "seven-eighths" in common usage, and even smaller divisions – "fifteen-sixteenths" and even "thirtyone-thirtyseconds," for example - occasionally appeared. Lush posed two questions: first, whether under the precepts of Mendelian genetics such phrases held any meaning and, second, whether the differences between these grades justified the large price increase that accompanied each "upgrade" in the amount of Brahma heredity possessed by an animal.[316]

Lush answered the first question in the affirmative. By applying statistical techniques to his understanding of Mendelian genetics Lush believed he proved that, given enough genetic factors – as few as nineteen would do, a number Lush thought probably underestimated the true number of genetic factors carried by an individual animal – the fractional terminology employed by breeders did possess sufficient meaning to make such claims useful to cattle-breeders. However, anticipating the assertion that he

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[315] Ibid., 95.

[316] Jay L. Lush, "'Percentage of Blood' and Mendelism," *The Journal of Heredity* 18 (1935): 351.

made in *Animal Breeding Plans* that to properly apply statistical techniques one must deal with populations rather than individuals, Lush warned that most individuals would vary somewhat – and, in a tiny percentage of cases, drastically – from the predicted norm. For example, given the nineteen factors that Lush considered the minimum necessary to make statistically useful comparisons, he demonstrated that sixty-four percent of animals described as "three-quarters" blood would possess seventy to eighty percent and that breed's genetic factors, and that ninety-four percent of animals would carry between sixty-five and eighty-five percent. Put another way, given nineteen genetic factors, an animal breeder could rest-assured that ninety-four percent of the time the animal described as "three-quarters" blood would, in fact, possess approximately three-quarters of the desired genetic characters, and could assume that other fractions would, in almost all cases, closely approximate the claimed percentage of hereditary factors.[317]

Lush employed a useful analogy to distinguish, and, to some extent, reconcile the differences between "fraction of blood" and Mendelian inheritance. According to Lush, the former method of "Mating two animals together was conceived to be a process analogous to the thorough mixing of salt and sugar solutions," while the latter he likened to "card-shuffling, coin-tossing, matching strings of beads [or] drawing black and white beans out of a bag."[318] However, Lush believed that, given enough genetics factors, little difference existed between the two systems: "*Thus the crux of the whole question of the accuracy of a percentage-of-blood description of individuals is the number of factor pairs involved in the cross.*" According to Lush, the only difference between the consisted of

[317] Ibid., 356-357.
[318] Ibid., 351.

the sample-size: "The apparent difference between them (the above mentioned analogies) is due solely to the relatively small number of black and white beans in the bag as compared to the truly enormous number of salt and sugar molecules in the mixed solution."[319]

Lush's findings prompted him to draw one important conclusion and one crucial caveat. Under most conditions, and given enough genetic factors, "percentage-of-blood" descriptions of an animal's genetic makeup was a useful tool for animals breeders. However, the system apparently worked only when comparing animals from independent bloodlines. Lush warned that the system "does give rise to serious errors when applied to individuals which are collateral relative of each other. Measuring an animal's degree of inbreeding by the percentage of blood common to its sire and dam is not accurate enough to be of much practical use." This difference was crucial to Lush, and understanding and designing useful systems of inbreeding would comprise an important part of Lush's later work and, in particular, *Animal Breeding Plans*.[320]

In the early nineteen-twenties Lush accepted a position at Iowa State College, where he would remain through the rest of his career. Among his first publications while at Iowa State were a pair of reviews that not only allowed Lush to iterate his place within the community of scientists investigating breeding but also to clarify his own beliefs. In his review of German geneticist C. Kronacher's – leader of the animal breeding institute of Berlin - *Genetik und Tierzüchtung* (Genetics and Animal Breeding) Lush generally

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[319] Ibid., 361.

[320] Ibid., 367.

praises both Kronacher's original work as well as his account of the ways that the rediscovery of Mendelian genetics had contributed to a scientific understanding of animal breeding. However, Lush's few criticisms of Kronacher illustrate that Lush had his own ideas about how such research should continue.

Lush notes Kronacher's enthusiasm for the possibilities promised by an application of Mendelian genetics: "In many places it is stated or implied that the breeder's task will be vastly easier once he has a Mendelian list of the genes involved and knows which are dominant and what their linkage relations are." Unfortunate, in Lush's eyes, was "some overstressing of the importance of dominance" stemming "from the assumption that the number of important genes affecting each characteristic ifs very small." Lush took a more pessimistic view: "How would the author…advise the breeder to proceed even if he had perfect knowledge of the genes and their inter-relations but if there were 10 or 20 or more pairs of genes affecting each important characteristic?" It seems that Lush was questioning the practical value of relying solely on Mendelian genetics as a basis of making breeding decision, in large part because the number of genes carried by complex mammals such as cattle and horses would allow literally millions of possible combinations: "as soon as we embark on a breeding program dealing with anything except characters inherited in the simplest manner, such as color, the problem becomes too complex to keep track of single genes."[321]

Lush then takes to opportunity to question Kronacher's "enthusiasm for the von Patow hypothesis of the genetics of milk and fat production in dairy cattle." Lush's

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[321] Jay L. Lush, "Genetics and Animal Breeding," *The Journal of Heredity* 27 (1944): 201-202.

criticisms centered on his belief that Kronacher placed at once too much and too little faith in the power of statistics. While earlier Lush admonished Kronacher for not fully appreciating the fact that even perfect knowledge of the working of genes would force breeders to make choices among literally millions of possible combinations, Lush now questioned Kronacher's faith in (to Lush's eyes, the hopelessly simplistic) von Patow hypothesis: "It seems unlikely that the von Patow method can ever lead to a Mendelian analysis which corresponds at all to reality except in so far as both it and the reality can be described at least roughly by something like a normal bell curve." Lush echoes these concerns later in his review, and his comments deserve to be considered in full:

> "Finally, there is repeated at many places the assertion that statistical investigations lead nowhere and or of little or not practical benefit. That is an opinion also held by others, to be sure, but surely the reader is entitled to be told when and how this was proved, especially when its futility is contrasted with the supposed fruitfulness of the superficially Mendelian methods."[322]

In the end, the article serves as a useful summation of Lush's beliefs. Like Kronacher, Lush accepted, and operated within, the framework of Mendelian genetics. However, at the same time, Lush's faith in the power of statistical analysis caused him to question whether other geneticists fully appreciated the complexities and ramifications of Mendel's hypothesis. In particular, Lush doubted that even perfect knowledge of the function and role played by each gene would, of itself, lead to improved breeding methods. Though Lush concluded, "These criticisms are of minor importance after all,"

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[322] Ibid., 202-203.

they represent the sorts of quandaries that he would consider at much greater length in *Animal Breeding Plans*.[323]

Lush also reviewed Kronacher's (this time with the collaboration of D. Sanders) *Neue Ergebnisse der Zwillingsforschung beim Rind* (Identical Twins in Cattle). In this work Kronacher reviewed work performed by his colleagues on fraternal and identical twins. The review's usefulness stems from the fact that it demonstrates how Lush viewed "similarity" and even "identity" as statistical constructs. The same reliance in statistical methods that allowed Lush to claim "a breed is a population" could, on a much smaller level, allow him to distinguish between – to "classify" and, hence, "identify" - identical and fraternal twins: "The diagnosis of identity rests on similarity in a long series of characteristics, each of which is determined to considerable extent by heredity." The differences or similarities between these characteristics allowed the researcher to determine whether twins were, in fact, fraternal or identical: "Fraternal twins might of course happen to be as like each other as identical in one or a few characteristics but, as the number of characteristics is increased, it becomes more and more improbable that fraternal twins would be alike in all the genes involved." Again, Lush employed the statistical power of groups, in this case the large number of genes, to classify animals.[324]

Lush noted that the method of classifying genetic groups by external characteristics "has been used for a long time in such sciences as anthropology where…it may be used to identify an individual as belonging to one or the other of two races which overlap in…their characteristics." As applied to twins by the German researchers, they

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[323] Ibid., 203.

[324] Jay L. Lush, "Identical Twins in Cattle," *The Journal of Heredity* 28 (1945): 415.

classified twin dairy cattle as identical or fraternal by comparing characteristics of each pair of twins with each other and assigning each characteristic a numerical rating (a "similarity quotient"), 1 denoting "different" characters, 2 "similar" characteristics, and 3 "surprisingly similar" characteristics. Rating a range of factors allowed the researchers to produce an average "similarity quotient." Fraternal twins averaged a 1.62 quotient, while most identical twins averaged "about 2.5 to 2.6."[325] The German's conception of identity tended to mirror Lush's own beliefs; after all, just as Lush defined breeds as animals sharing a statistically meaningful group of characteristics, so too did the Germans. In short, the only real difference between Lush and the Germans was the size of sample they employed.

As might be expected, Lush praised the work as "a valuable reference work for students of…cattle genetics." As importantly, he used the conclusion of his review to make a point that offers valuable insight into his own work. Lush noted, "the conclusions (reached by the German scientists) are not entirely quantitative and some of the them seem to be considerably subjective." However, since the researcher "is often faced with something of that choice," it was "Perhaps…better to be somewhat subjective and fairly complete than to be altogether objective but very incomplete."[326] This passage illustrates Lush's commitment to obtaining meaningful results as well as the fact that he maintained a certain scientific flexibility; in other words, it demonstrated his commitment to a scientific method that did not insist on perfectly objective inputs. As borne out in *Animal*

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[325] Ibid., 416-417.
[326] Ibid., 418.

Breeding Plans, this commitment to practical, useful results formed a cornerstone of Lush's work.

With various collaborators at Iowa State Lush also compiled "genetic histories" of Poland-China swine, Rambouillet sheep, and Holstein-Friesian cattle since their introduction to the United States. All three studies followed similar methodologies. The researchers began by randomly choosing a sample set of animals from the breeding associations breed books. They then traced them back, choosing to follow the line of dam or sire by a coin-flip for every generation. They then compared the selected animals for common ancestry and shared bloodlines. The main object of each survey was to "find the part played by development" of the animal in this country, to determine "whether the breed has developed as a homogeneous unit or has shown a tendency to split into genuinely distinct families," and, finally, to calculate, if possible, "how much influence individual animals have had upon the breed."[327] In performing these studies the researchers followed the work of Sewall Wright, who not only performed some similar surveys but, from their results, had proposed a "coefficient of inbreeding" that predicted "the degree of likeness (within the breed)…relative…to the average amount of genetic unlikeness between the gametes produced by the foundation stock" or, "in stockman's language…the percentage of those characteristics which were fixable but not yet fixed…which the inbreeding system itself has brought into fixation."[328]

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[327] W.F. Dickinson and Jay. L. Lush, "Inbreeding and the Genetic History of the Rambouillet Sheep in America," *The Journal of Heredity* 24 (1931): 20.
[328] Ibid., 21.

Despite studying three different species – cattle, sheep, and swine – the researchers drew remarkably similar conclusions from each group. First, over the period examined (roughly 1880-1930) each showed a slow but statistically significant increase in the coefficient of inbreeding – the average amount of common blood shared by any randomly selected individuals: 5.5% for sheep, "a little over four percent" for the cattle, and approximately 6% for the swine. Furthermore, there was only "a faint tendency for distinct families to be formed," despite the fact that, for each species considered, a handful of animals – roughly a dozen – served, in effect, as "grandmothers of the breed." For example, the Holstein-Friesian cow De Kol "probably furnished about one tenth of the genes of the breed today," "about 45% of the ancestral lines" of Rambouillet sheep came from animals "bred by von Homeyer in Germany," and that three distinct Polish-China swine "each contributed more than twelve percent of the genes of the breed." However, in each case these animals represented creatures that were considered exceptional and, thus, their offspring were widely distributed, spreading the animals lineage while at the same time gradually diminishing that amount of influence they played on the heredity of the breed as a whole.[329]

The tests tended to confirm Lush's suspicions about animal "breeds." First, within these relatively closed populations the amount of both inbreeding and common ancestry slowly but appreciably increased, though the two factors were not necessarily linked. Second, the records suggested that the proliferation of a particular bloodline

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[329] Ibid., p. 33. Jay L. Lush, J.C. Holbert and O.S. Willham: "Genetic History of the Holstein-Friesian Cattle in the United States," *The Journal of Heredity* 27 (1944): 71. Jay L. Lush and A.L. Anderson, "A Genetic History of Poland-China Swine," *The Journal of Heredity* 30 (1947): 224.

occurred in clusters as breeders tended to breed heavily to the offspring of noted examples of the breed. The fact that the "spotlight of that fame sometimes [shifts] from one animal to another not very closely related to the reigning favorite which it displaces" had two important consequences. It allowed new blood into the favored breeding pool. Moreover, it suggested that, over time, breed "ideals" – that is, what breeders considered the most desirable traits – changed. Once again, Lush touched on themes that he would consider in more detail in *Animal Breeding Plans*.[330]

In addition to his histories of breeds, Lush also a pair of studies that examined two private breeders who had maintained, for long periods, "closed" herds, one of Shorthorn cattle, the other of Belgian horses. In each case, attention had been drawn to the herd by the fact that each farm had produced animals that had dominated their respective classes at the Iowa State Fair for a number of years. Further investigation revealed that not only had these animals come from the same farms, but, more importantly for Lush, had come from farms that had had "closed" herds – that is, that had not acquired any outside cattle – for a lengthy period: over fifteen years in the case of C.G. Good and Son's Belgian horses, and over twenty years in Burt Neal's herd of Shorthorn cattle. Further, not only had these herds remained closed to outside animals and their genetic influences, but all of the animals produced by these farms during the entire period, save a handful of Shorthorn cattle bred and subsequently eliminated from the herd at the beginning of the period under consideration, were decedents of a single sire.[331]

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[330] Ibid.
[331] P.B. Pearson and Jay L. Lush, "A Linebreeding Program for Horse Breeding," *The Journal of Heredity* 24 (1931):185; Jay L. Lush, "A Herd of Cattle Bred for Twenty Years Without New Blood," *The Journal of Heredity* 25 (1932): 209.

Good and Neal had both paid large sums of money for animals they considered especially promising; at a Shorthorn sale Neal and his father saw an animal "which as an animal pleased them so well that they purchased him, although his price was more than twice what they had intended to pay,"[332] while Good bought the Belgian stallion "for $47,500, the record price in America for a draft horse."[333] Having obtained what they considered an outstanding specimen, both men decided to institute a breeding plan that would maximize that animal's genetic influence on their respective herds. To that end, each breeder – who, it should be made clear, did not know each other – began a system of line-breeding that, they hoped, would minimize the potential hazards of intensive in-breeding while maximizing and maintaining the desired characteristics of their prize sires. As such they were of keen interest to Lush, who would publish *Animal Breeding Plans* within four years of the appearance of these articles.

Mr. Good, the breeder of Belgian horses, devised a system that he hoped would eventually produce a herd "with every animal in it containing 50% of the blood of Farceur," – the name of his prize stallion. His plan was to save Farceur's "two best sons" for use on Farceur's daughters. In each generation that followed, one of the sons of each of these sires – i.e. Farceur's grandsons – would be retained and mated to the progeny of the other and "each of these stallions would in turn be followed by a son which would be used on the daughters of the other stallion." Such a plan would eventually result in a herd every member of which would carry "50% of the blood of Farceur," but would also avoid

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[332] Lush, "A Herd of Cattle," 209.

[333] Pearson and Lush, 185.

inbreeding "as far as could be done in a closed population having only two sires in service at any one time." According to Lush, the most dangerous step occurred in the first mating, which would unite half-brother and sister, but Good, who recognized the potential hazards, reasoned that "if the results of these matings were satisfactory, there would be practically no inbreeding risk in subsequent generations…"[334]

Lush analyzed Good's plan, subjected it to statistical scrutiny, and ultimately praised it as a model by which to favor the possibility of retaining Farceur's desirable qualities. In fact, Lush referred to Good's scheme as "the essence of linebreeding," whose goal was "to stop the diluting effects of outcrossing so as to conserve the desired qualities of an esteemed ancestor but at the same time to keep the intensity of the inbreeding milk dough that the effects of random survival or elimination of the genes from the population will not get beyond the control of selection."[335]

Mr. Neal, the Shorthorn breeder, followed a somewhat different plan. Like Good, Neal started with what he considered an outstanding animal, Sultan's Banner. However, while Good devised a breeding plan that he implemented as soon as he acquired Farceur, Neal had no such strategy, and "Sultan's Banner was placed in service with no deliberate intention of making any permanent change in the breeding policy of the herd." Neal had, like many breeders, traditionally employed two unrelated stud animals in an effort to minimize the potential ill effects of inbreeding, and initially used Sultan's Banner in such a manner. However, as Sultan's Banner's progeny matured Neal like them so much that he decided to use him exclusively. Sultan's Banner was replaced by one of his sons,

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[334] Pearson and Lush, 185-186.

[335] Ibid., 188-189.

Bannerview, who was eventually succeeded by another son, Banner's Last, the bull in use at the time Lush wrote the article.[336]

Neal's use of a one-sire herd contrasted with Good's employment of two sires, and furnished Lush the opportunity to make some comments about the two systems. While he had praised Good's scheme as an ideal vehicle by which to maintain as much as possible the traits of a prized sire while avoiding the potential pitfalls of inbreeding, Lush admitted that though "it might well be doubted whether even the wises selection can keep enough control over a one-sire herd to make that an advisable policy," he concluded that "Perhaps we are still too much afraid of inbreeding in cattle."[337] However, even Mr. Neal expressed some reservations about his one-sire policy despite his success; Lush reported that Neal "hesitates to look up the pedigrees of most of his animals lest on the one hand he become frightened by the amount of inbreeding that might be there."[338]

In fact, Neal's herd showed a higher than typical amount of inbreeding, and his herd did contain a high amount of genetic uniformity that caused Lush to describe the herd as "more uniform than any herd could be without the use of some inbreeding." Despite the potential for genetic hazards, Lush believed that the use of one-sire herds might prove a benison despite the risks: "The principles of genetics indicate that the average merit of the whole breed would be improved more rapidly than at present if there dozens of good herds using some such policy" as Neal's. These closed, one-sire herds would eventually result in the creation of "distinct families" that could eventually "be

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[336] Lush, "A Herd of Cattle," 209-210.

[337] Ibid., 214.

[338] Ibid., 212.

crossed with each other to find improved combinations." Lush admitted that such a plan had drawbacks; most importantly, Lush believed that many Shorthorn breeders would fear introducing such "inbred" animals into their own herd. However, he also noted that these financial losses would, to some extent, be offset by "the lack of expense for breeding stock," the "slight decrease of health risks due to failure to introduce outside stock," and "some saving in travel, transportation, and advertising costs."[339]

Lush brought the many and varied strands of his genetic investigations together in *Animal Breeding Plans*, which he completed in December of 1936. The book was the outgrowth "of seven years of teaching animal breeding to college students," in which courses Lush hoped to instill in his students "a clear understanding of the means available for improving the heredity of farm animals…"[340] *Animal Breeding Plans* contained three major sections: an introduction to the genetic – and, specifically, Mendelian – basis of animal breeding, a consideration of the various means of selecting animals, and a discussion of the best means by which to maintain, as much as possible, the genetic traits of desirable animals. In addition, short chapters discussed – and dismissed – a number of widely accepted, though scientifically discredited, "myths" about animal breeding. But at its core, *Animal Breeding Plans* represented Lush's attempt to address what he viewed as the most important concerns facing breeders: the selection of animals, and the best means to maintain the desirable traits of those animals in their offspring. The rest of the work revolved around these concerns.

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[339] Ibid., 214-215.

[340] Lush, *Animal Breeding Plans*, v.

Lush's discussion of the scientific basis of genetics made this clear. In most respects it mirrored the explanation provided by other Mendelian geneticists like R.A Fisher and Sewall Wright.[341] Like them, Lush discussed the latest theories of dominance, homo- and hetero-zygosity, the number of genes scientists then believed different animals carried, and the genetic basis of variation. Too, he discussed the role played by random mutations in shaping the development of animals, and, like other authors, downplayed the importance ascribed by some earlier geneticists to the role of mutation played in evolution.

However, Lush's consideration of genetics primarily served to emphasize the importance of selection and breeding. To this end, Lush employed his sophisticated understanding of biometry to demonstrate the complexities of Mendelian genetics. Discussing the "consequences of the large number of genes" possessed by most mammals – and employing rhetoric that predated astronomer Carl Sagan by several decades - Lush attempted to bewilder the reader with – and bolster his claim that his plans might allow some control over – the dizzying number of genetic combinations that could be produced by even a relatively small number of heterozygous genes within a population: "if the number of different genes…in each species is as large as 40 (and it may well be thousands), the number of different hereditary combinations possible in each species is millions on millions of times as large as the number of animals which can actually be alive at any one time." Put another way, "In a species with only 200 pairs of

[341] See R.A. Fisher: The Genetical Theory of Natural Selection, 1930 and Sewall Wright: Evolution in Mendelian Populations, Genetics 16:97-159.

genes…there could be 10^95 different kinds of individuals. This is a million billion times as many as there are electrons in the universe!"[342]

The huge numbers involved brought with them some important consequences. Most obviously, the figures represented the sheer complexity of genetics and suggested why breeders had met with relatively little success in their efforts to improve their stock. Simply put, Lush demonstrated that genetics, as then understood, was vastly more complicated than earlier generations had guessed. However, Lush also believed, and attempted to demonstrate, that the sheer complexity that stymied breeding efforts also contained the glimmer of a silver lining. Specifically, Lush posited that the large number of gene combinations should behave like any other group of large numbers, and could therefore be analyzed using the mathematical techniques that had been developed to study such groups. In short, Lush maintained that the employment of the mathematical tools of probability and statistics could be employed to make sense of animal breeding, and the bulk of the book, and, indeed, of Lush's life-work, involved precisely this: the application of statistical techniques and the predictions they allowed to the study of genetics and breeding.

The use of such methods had far-reaching implications. For example, Lush demonstrated, in a manner similar to his method for distinguishing fraternal and identical twins, that statistics could be employed to identify members of a certain breed. The technique was relatively straightforward. One simply compiled a list of characteristics – length of head, shape of ear, etc – and developed a way to quantify these characteristics. Provided one made enough observations, the characteristics of a specific breed would

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[342] Lush, *Animal Breeding Plans*, 48-49.

tend to cluster around a statistically meaningful set of numbers – that is, around the "average" scores of that breed for each characteristic. Identifying an animal's breed thus ultimately became a matter of mathematical comparison: "By considering a sufficient number o f characteristics in which the averages of two breeds are different, it is possible to identify with an desired degree of certainty the members of each, even though both breeds overlap each other's range in all those characteristics."[343]

Lush considered selection the most important facet consideration facing animal breeders: "The first step in any animal breeding program is to decide what is a ideal. Until a breeder knows what kind of animal he wants, he is stopped in his tracks and can neither select the best nor discard the worst."[344] This decision would be shaped by the breeder's "physical resources" and "markets" as well as "his own personal inclinations" and would "often be a compromise" between market demand and individual considerations. For example, "the butcher's interest in…high quality meat, if carried too far, might result in animals with vital organs too small for them to be as healthy and thrifty as the farmer wishes." Moreover, the breeder faced a changing "commercial ideal...which can change much more rapidly than the breed average."[345]

Despite these difficulties, selection of stock represented the most important decision made by breeders as this represented the "most effective method for changing the…genetic averages of the breed for various characteristics." Again, Lush reminded his readers that a "breed" is, as Lush conceived it, essentially an average. In doing so,

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[343] Ibid., 92-93,95.

[344] Ibid., 335.

[345] Ibid., 336.

Lush replaced the conception of a breed as a set of animals sharing physical characteristics and some common heredity with statistical definition that substituted numerical quantities for physical qualities. In one sense Lush offered his readers a canard: in effect, he merely "quantified" the "qualities" that he measured; again using the example of twins discussed above, Lush (and others who worked in this vein) assigned subjective numerical values to quantify such characteristics as "resemblance." However, this seeming innocuous shift allowed Lush to employ statistical techniques which he believed represented the most powerful weapon in the breeder's arsenal.

Applying statistical techniques revealed some important ramifications. Most significantly, Lush believed they proved that selection "is an effective tool for changing the average of a characteristic in a population," although he warned "considerable time may be required for that. Moreover, it had "little effect on heterozygosis" and was "usually most effective when first practiced." Unfortunately, the statistical law of averages also predicted that most of the offspring of exceptional parents would revert to the herd average; outstanding animals represented statistical outliers, and the laws of large groups implied that, in most cases, the offspring of these animals would revert to type. Furthermore, "low reproductive rates which prevent intense culling," the trap of "mistaking the effects of the environment for the effects of the genes," and a tendency for many breeders to "[pay] attention to so many things that progress for any one them must be slow" not only underscored the obstacles standing in the way of herd (or breed) improvement but made selection that much more critical.[346]

[346] Ibid., 123.

Having made selection the most important consideration of his method, Lush turned his attention to the various means by which breeders could select stock. He first examined the use of lifetime averages: the amount of milk and fat produced by a cow in each lactation, the weight of fleece produced by a sheep in a year, the numbers of piglets farrowed by a sow, or the speed of a racehorse. The technique was inexpensive, and tended to minimize the effects of "temporary environmental conditions." Despite this, it could only be applied to those animals – and those circumstances – that "can be observed more than once" and could hence not be applied to qualities such as "age of sexual maturity" or "carcass qualities."[347]

Unfortunately, selecting animals on an individual basis did little to guarantee that the animal's offspring would itself retain the desired characteristics. However, pedigree testing – comparing the ancestors of an animal for several generations – could be "helpful in overcoming some of the mistakes…made in culling on individual merit alone," but came with its own limitations. Not only was the "accuracy of pedigree selection…. limited by the sampling nature of inheritance," but "one can never be entirely sure of the inheritance the parents had…"[348]

Progeny testing – the evaluation of animal's worth based on its offspring – de facto solved some of these problems but presented a different set of problems. The long gestation period and small litter size of large mammals like horses and cattle made obtaining a useful sample size a difficult proposition; Lush calculated that at least five

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[347] Ibid., 130-131.

[348] Ibid.

offspring needed to be evaluated to make a statistically meaningful analysis. This meant that cows and mares, which averaged about a year and a half between births, had reached the tail end of their productive lives before one could make concrete judgments about their ability to reproduce their desirable traits. Bulls and stallions could, of course, sire offspring much more quickly, but one then ran the risk of producing a large number of undesirable animals before their breeding value became apparent. To offset this risk Lush concluded that "progeny tests have their widest use in making pedigree selections more accurate" because they could be used to confirm those "sires and dams whose offspring are most likely to be worth saving for breeding."[349]

Judging animals by "type," or their conformity to some ideal, had long been the most widely employed method of judging an animal's merit. It was important, "especially among meat animals ready for market" because "a certain conformation not merely indicates production but actually comes close to being production…"[350] Furthermore, testing by type could prove useful where production records "are not available" or, as in the case of dairy cattle, "come slowly and expensively." And, in some cases – particularly the breeding of "pets and fancy stock" – type testing largely determined an animal's value.

Lush took the next step and attempted to determine whether any correlation existed between type and actual production; that is, he inquired whether animals rated by judges superior in their conformity to their breed ideal actually produced (in the case of the Holstein-Friesian cattle examined by Lush) more milk. Despite some difficulties,

[349] Ibid., 154-155.

[350] Ibid., 166.

such as the fact the judges tended to favor large cattle over smaller animals, and large cattle tended to produce more milk regardless of ranking, Lush believed that his studies "established the existence of correlations between type and production," though the actual correlation proved quite low and led Lush to conclude that one "should pay more attention to production records than to estimates of type," though one could not "afford to neglect type altogether."[351]

Lush also considered whether conformation to breed type – how closely an animal's exterior characteristics like color or horn size mirrored the breed ideal – or success in the show ring could serve as accurate predictors of an animal's productive value. As regards breed type, Lush admitted that the "practical breeder cannot afford to ignore it altogether if his market places some value on it," but warned that breed type could become "positively harmful when so much attention if paid to it that animals above the average in real usefulness are discarded because they do not conform to breed types."[352] Likewise, an animal's ranking in the show ring could help "to a very limited extent" in the absence of reliable production records. However, that fact that the show ring served as "a means of emphasizing current ideals" did not bode well for long term success, primarily because "there have been times when the breed ideal changed, even against opposition from the show ring."[353]

Unfortunately, none of these breeding methods could be relied on to consistently identify truly exceptional animals. Production records could confirm an animal's value,

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[351] Ibid., 169, 171.

[352] Ibid., 176-177.

[353] Ibid., 186-187.

but offered little assurance of the productive capacity of its offspring. Progeny testing accurately measured the value of the offspring, but suffered from serious drawbacks: the lengthy period of time to identify valuable females required that farmers maintain the animals in question for a period of years, and while males could produce offspring much more rapidly, one ran the risk of producing a large number of undesirable offspring before the sire's true value became apparent. Judging an animal by it's conformity to ideals offered some assurance of that animal's value, but offered little promise of predicting whether the creature would pass desirable traits to its progeny; conformity to breed and success in the show ring likewise offered no assurance about the animal's offspring, and, in addition, relied on physical characteristics rather than productive capacity.

Lush's hope to produce a guide that would aid breeders in their selection of stock amounted, in some ways, to little more than smoke-and-mirrors. Each of the selection methods he examined suffered from serious flaws, and, even when used in combination, offered only a modest reassurance that they would identify valuable animals that would pass their advantageous qualities to their offspring. Moreover, Lush offered no new plans; in fact, progressive breeders had been practicing the methods – though to be sure, few practiced all of them – examined in *Animal Breeding Plans* for more than half a century.

Nor were the breeding plans Lush described – which formed, along with selection, the backbone of his work – novel. The aim of all these plans lay in maximizing the number of "good" genes passed from parent to child while avoiding, as much as possible, the transmission "bad" genes. With a handful of important exceptions, Lush warned against the dangers of inbreeding. To aid in this process Lush employed a "coefficient of relationship," which he defined as "the percentage of genes which are probably identical in…two related individuals on account of their relationship by descent." However, like much else that Lush studied, this measure could also be misleading: the coefficient of relationship measured "the probability that individuals will be alike in the genes," not "their actual physical outward likeness." In other words, similar physical appearance did not necessarily guarantee similar performance.[354]

Lush began his discussion of breeding plans with an examination of the consequences of inbreeding, which he defined as the "mating of closely related animals," even though many "practical breeders" restricted the use of the term to the mating of "full brother and sister, or of parent and offspring."[355] Lush began with the observation that "all animals that can be mated at all are related, at least slightly," implying that all the member of a species are inbred at some point. Pure breeds of animals also resulted from inbreeding, and maintained their purity by inbreeding within the breed. However, the most potent, potentially dangerous, and, possibly, useful inbreeding occurred, as per common parlance, in breeding between two closely related animals.

Inbreeding could more quickly alter the heredity of a group of animals than any other method. The practical effect of inbreeding was to increase the amount of homozygosity within the breeding population. This could have both good and ill effects. Practiced indiscriminately, one risked the possibility of fixing "undesirable traits at so rapid a rate" that the herd as a whole would soon possess these genetic faults. On the

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[354] Ibid., 204-205.

[355] Ibid., 206.

other hand, inbreeding could also, when applied judiciously and with an eye to culling animals carrying "inferior" genes, quickly produce new families of animals within the breed, a situation that Lush thought desirable for the improvement of the breed as a whole if not for the individual breeders involved: "It seems reasonably certain that more opportunities for breed progress are lost by not inbreeding when it would be advisable than are lost by too much inbreeding."[356]

Linebreeding represented one such method. Lush defined the practices as "mating animals so that their descendents will be kept closely related to some animal regarded as unusually desirable." The case studies of Good and Neal, the Shorthorn and Belgian breeders, represent such breeding plans. Though linebreeding represented a form of inbreeding they differed primarily in that inbreeding bred within a closed group without respect to individuals, while linebreeding involved the systematic breeding to a specific animal. Lush advocated linebreeding as a useful system that allowed farmers to maximize the genetic properties of superior animals while minimizing the chances that faulty traits would become fixed.[357]

Lush also examined systems of outbreeding – breeding animals "distinctly less closely related to each other than the average of the population" – as well as "breeding like to like," – the practice Lush himself had been taught as an undergraduate. Though widely practiced, primarily by farmers who held an, in Lush's view, unwarranted fear of any amount of inbreeding, neither method offered real promise for sustained

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[356] Ibid., 220, 227-228.

[357] Ibid., 235-241.

improvement of herds. Lush believed that each method offered only temporary, and random, improvements; further, the plans offered little possibility of fixing these traits.[358]

But, once again, Lush offered nothing new. All of these methods had been practiced for decades, if not, as in the case of breeding "like with like," for centuries. Lush did offer of his opinions about which methods seemed most likely to produce long-term results, but as with the selection methods Lush examined, he proposed nothing that had not appeared in breeding texts since at least the end of the nineteenth century. In one sense, then, Lush offered his readers little but a rehash of widely-known, if not necessarily widely-practiced, animal breeding techniques. As such, it would be easy to dismiss his work as redundant.

But, more importantly from both a scientific as well as a historical perspective, Lush did something very different than his predecessors. Namely, he established animal and selection – especially of larger animals like horses and dairy cattle - on a firmer scientific basis than they had previously enjoyed. Rather than repeat hearsay and accepted wisdom, he subjected each technique to rigorous testing and reported his results. Some long-accepted methods, like breeding "like to like," he discredited, while he found that some methods considered taboo by many breeders – inbreeding, for example – offered the possibility of real improvement when used judiciously.

The key to his reconceptualization of animal breeding lay in his use of statistics. More so than any other genetics researcher doing work on large animals, Lush embraced the use of biometrics, the application of statistical techniques to the study of genetic populations. Lush realized that the large number of genes possessed by large mammals,

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[358] Ibid., 253, 258-261.

combined with his understanding of Mendelian genetics, implied that even the mating of two random individual could produce a finite, but still bewildering, number of combinations. Applied to species as whole, Lush believed the staggering number of genetic combinations that were possible implied that any study of group genetics could only proceed by the use of mathematics.

Of course, Lush ran into obstacles. Primarily, this method required him to “quantify” various factors that resisted easy numerical evaluation - the presence or absence of horns or spots on animals, or the color of the coat, for example. Having done so, Lush believed that this statistical analysis of breeds could serve as a real boon to farmers. Most importantly, it allowed him to apply the laws developed by researchers such as Sewall Wright that promised to calculate how much of the genetic content of a population could change within a specified number of generations. Lush realized, and promoted, the notion that farmers could not expect huge changes from one generation to the next, and that when such changes occurred they were almost certainly statistically rare outliers – whose progeny would mostly like revert to form – than mutation that marked a definitively new genetic chain.

In short, then, *Animal Breeding Plans*, and, for that matter, most of the research performed by Lush throughout his career, offered at the same time something old, and perhaps even threadbare, and something entirely new. Farmers had for decades practiced the selection methods and breeding plans he examined for decades; in one sense, Lush merely re-examined well-trod ground. However, the manner in which he examined these methods represented a new method of considering the breeding of large animals. Furthermore, this change amounted to more than a change in terminology. In fact, Lush did much more than substitute one term for another. Instead, by his use of statistical techniques that dealt with quantities, rather than qualities, Lush recast the debate in a whole new light, one that he believed offered real possibilities for helping breeders make informed decisions.

CHAPTER 10

CONCLUSION

By the 1930's agricultural scientists, most of them working at land-grant universities and state experiment stations, had assumed leadership of the American dairy enterprise. While not all farmers followed their suggestions, scientists had successfully wrested control of the terms of the debate. Even when they rejected the advice of researchers, farmers employed their language: farmers' letters published in the leading dairy publications regularly mentioned "milk-fat," "carbohydrates," and "bacteria-counts." The articles contained in the journals also assumed a basic knowledge of "scientific" dairying, and periodicals like *Hoard's Dairyman* and *Kimball's Dairy Farmer* routinely reported the latest scientific bulletins issued by universities and experiment stations.

As the articles describing the completion of the bovine genome sequence suggest, scientists remain the pacesetters of American dairying a decade into the 21st century. The techniques that scientists hope to perfect will allow farmers an almost god-like power to basically create whatever sort of animal they desire: animals that produce more milk, and/or more fat while consuming less grain, animals resistant to temperature extremes and bovine disease, animals of virtually any color or size, with horns or without. And, they will be able to create any number of them. No longer will farmers have to slowly build the quality of their herds; instead, they will be able to quickly produce as many animals as they desire.

This work has described the various ways that scientists came to wield this sort of authority. Perhaps most importantly, they did so by adopting common scientific methodologies. Doing so offered numerous advantages: it fostered communication between researchers by allowing them to easily understand the work performed by others; it allowed workers at poorly equipped or funded stations to make valuable contributions to; it was easily adaptable and could be applied to new circumstances and situation. Finally, adopting common techniques allowed scientists to present their work to (often skeptical) farmers as a complete system or mindset rather than a piecemeal group of suggestions.

This seems to have been especially important to the first generation of researchers I discuss in the first five chapters of this work – the group that considered quantification the hallmark of science. As a group these scientists took pains to promote their vision of dairying to American farmers. The introductions to the various works clearly demonstrate that researchers took pains to carefully explain to farmers why adopting their suggestions was in the farmers best interest. Moreover, they attempted to do so in a manner that did not frighten or offend farmers but instead by assuring farmers that “book-knowledge” should supplant, rather than replace, their own experience.

Starting from practically nothing scientists achieved a remarkable amount of success within a few short decades. By the second decade of the twentieth century their influence was such that they had changed the language of dairying. Moreover, the growing number of farmers who joined milk-testing and dairy-herd improvement associations suggests that many farmers were willing to adopt any measures that promised to increase the profitability of their farms.

Of course, the adoption of a common scientific methodology entailed new set of problems. As I explored in the second and third chapters, nutrition scientists readily agreed how they should go about their work, but new debates emerged as researchers disputed the meaning of their findings. The discovery of vitamins exacerbated these controversies, particularly in the early years of vitamin research when scientists struggled to isolate, identify, and measure the newfound – and difficult to analyze – nutrients.

What is especially interesting is that farmers did not, as a group, use this opportunity to call into question the authority of scientists. By all accounts, by the nineteen-teens farmers generally recognized the advantages offered by adopting "scientific" or "rational" dairying practices, despite the fact that the way that scientists understood the world and went about their work was undergoing rather radical changes.

By this criterion, it seems safe to posit that by the 1930's large numbers of farmers had begun to practice "modern" dairying. A significant percentage of farmers participated in herd improvement associations, employed "rational" feeding techniques, kept careful record of their herds' production, and practiced the breeding methods advocated by geneticists like Jay L. Lush. An even greater number subscribed to the leading dairy journals and were, at the very least, regularly exposed to the latest advances in dairying.

The architecture of the farm also offered some clues. Most obviously, by 1940 many dairy farmers had constructed silos that allowed them to feed succulent corn ensilage throughout the winter months. The inside of the barn often also saw changes:

many contained a Babcock milk-fat test, and a growing number housed one or more milking machines.

Despite these changes, however, the dairy farm of the 1930's still more closely resembled the dairy of a century earlier than the recognizably modern dairy that would emerge in the 1960's. In the 1930's most dairy farmers – the notable exception being those farms located on the outskirts of major metropolitan areas to provide fresh milk for the citizenry and had always been something of an anomaly - still practiced dairying as one facet of their overall agricultural operation. Most raised their own crops and kept hogs – which fattened quite readily when fed skimmed milk – as an adjunct to the dairy. Moreover, dairymen still milked in traditional stanchion barns, usually the same large, multipurpose edifices that housed other animals, equipment, and, in the loft, enough hay to see the farmer's livestock through the winter.

By the 1960's the built environment of the dairy farm had assumed a very different form. In most cases the actual milking had moved from the barn to a dedicated "milk house" whose sole purpose was the extraction and storage of milk. The milk-house contained a large, electrically-cooled and –stirred storage tank that preserved the milk, but the heart of the milk-house was the milking "parlor" - which was, in essence, a pit dug into the ground. Cattle – usually three or four on each side – filed into the parlor and assumed their stations in a staggered formation usually referred to as a "herringbone" pattern in dairy manuals. This configuration allowed the milker to milk eight animals simultaneously: the pit allowed the milker to work at chest level rather than continually squat beside each animal while a milk pipeline carried the milk directly to the bulk storage tank, minimizing the milk's contact with the air as well as the chance of contamination.

Though the milk parlor was the most obvious physical manifestation of the "modern" dairy, other, more subtle, clues would inform the careful observer that something new was afoot. Most glaringly, fewer dairies maintained their own bull, instead relying on artificial insemination to breed their cattle. British scientists first experimented with artificial breeding techniques in the 1930's, and by the 1960's the practice had become fairly widespread, especially on large farms that could afford to purchase and maintain a nitrogen cooling tank and acquired the skills necessary. On a more subtle level, by the tail-end of the 1960's dairy farmers began to experiment with the use of hormone-laced feeds that not only boosted production but encouraged the faster maturation of heifers.

Though all of these changes were first investigated by dairy researchers – most of them employed at state agricultural experiment stations – the most obvious of these changes was instigated not by scientists but by changes in government regulation. Beginning in the 1960's many states required that farmers who wished to sell their milk for liquid consumption (as opposed to sale for the production of cheese or butter) required that farmers install a milk pipeline that carried milk directly from the udder to the storage tank. The passage of these regulations forced dairy farmers to make a choice: invest in modern equipment or exit the industry. The result was the rather dramatic reshaping of American dairies within a fairly short period of time.[359]

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[359] Growing up on a dairy in the 1980's we milked a small (30 head) herd of registered Jersey cattle and, as we did not have a pipeline system we sold grade "B" milk that

Though these changes took place in the 1960's the seed for these transformations were already in place by 1940. By that point agricultural scientists had firmly entrenched themselves as arbiters of what constituted "modern" dairying, and farmers – most of whom appreciated the larger profits they realized by adopting "rational" techniques – generally fell in line. Having done so, they had little basis by which to dispute further suggestions. As such, we can trace the roots of the modern American dairy almost directly to the passage of the Hatch Act in 1890 even though more than a half-century would pass before the full ramifications of that event would come to light.

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eventually was turned into cheese and butter despite the fact that the bacteria counts from milk on our farm consistently measured well below the maximum allowed for grade "A" milk. With the rise of the "organic" movement in the 1990's several states – most notably Vermont – have created special regulations for small dairies that primarily sell un-pasteurized milk or produce cheese or butter in relatively small quantities.

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