

Judith P. Klinman

ANNUAL REVIEWS

Annual Review of Biochemistry

Moving Through Barriers in Science and Life

Judith P. Klinman

Department of Chemistry, Department of Molecular and Cell Biology, and California Institute of Quantitative Biosciences (QB3), University of California, Berkeley, California 94720, USA; email: klinman@berkeley.edu

Annu. Rev. Biochem. 2019. 88:1–24

The *Annual Review of Biochemistry* is online at biochem.annualreviews.org

https://doi.org/10.1146/annurev-biochem-013118-111217

Copyright © 2019 by Annual Reviews.
All rights reserved

ANNUAL REVIEWS CONNECT

www.annualreviews.org

- Download figures
- Navigate cited references
- Keyword search
- Explore related articles
- Share via email or social media

Keywords

quantum tunneling, origins of enzyme catalysis, oxygen activation, novel redox cofactors

Abstract

This first serious attempt at an autobiographical accounting has forced me to sit still long enough to compile my thoughts about a long personal and scientific journey. I especially hope that my trajectory will be of interest and perhaps beneficial to much younger women who are just getting started in their careers. To paraphrase from Virginia Woolf's writings in *A Room of One's Own* at the beginning of the 20th century, "for most of history Anonymous was a Woman." However, Ms. Woolf is also quoted as saying "nothing has really happened until it has been described," a harbinger of the enormous historical changes that were about to be enacted and recorded by women in the sciences and other disciplines. The progress in my chosen field of study—the chemical basis of enzyme action—has also been remarkable, from the first description of an enzyme's 3D structure to a growing and deep understanding of the origins of enzyme catalysis.

Contents

EARLY LIFE 2
TURNING TOWARD SCIENCE 3
THE WORLD OPENS UP 4
FAMILY AND TRAVELS 4
HOME AGAIN 5
MOVING TOWARD SCIENTIFIC INDEPENDENCE 6
A LABORATORY OF MY OWN 6
WESTWARD HO 7
EARLY YEARS AT UC BERKELEY 8
SECOND DECADE AT UC BERKELEY AND THE DISCOVERY OF QUINO-ENZYMES 9
THE DISCOVERY OF ROOM TEMPERATURE QUANTUM TUNNELING IN ENZYMATIC C-H CLEAVAGE REACTIONS 10
NEW WAYS OF LOOKING AT ENZYME CATALYSIS 12
OXYGEN ACTIVATION 14
BACK TO THE BEGINNING (OF QUINO-COFACTORS): UNRAVELING THE PATHWAY FOR PYRROLOQUINOLONE QUINONE PRODUCTION 16
ON BEING A WOMAN IN SCIENCE 16
NOTES TO MY FAMILY 17
LOOKING BACK, LOOKING FORWARD 19

In composing this article, I have worked hard to integrate aspects of my personal life with a description of the origins of my research interests and scientific discoveries. Although some of the scientific details may be difficult to follow for biochemists with a less chemical perspective, my intention has been to showcase a scientific path that has been driven by curiosity and persistence—the latter becoming especially important as findings arose that differed from mainstream thinking. The final three sections are primarily personal, with reflections on the role of women in science, the importance that family has held for me as inspiration and stability, and finally, a summing up in the section titled Looking Back, Looking Forward.

EARLY LIFE

I was born in 1941 in Philadelphia, eight months before the attack on Pearl Harbor that brought the United States into World War II. I have only a few memories from that time, consisting mainly of black velvet cloths being hung over our windows. As much as I can recall, war did not really infiltrate my awareness until many years later when I came across photographs of the American GIs liberating Jews from concentration camps in Europe. These had been sent to us by the part of our family that, though Jewish, had somehow survived the war in Paris.

The marriage between my mother and father was short lived, as my father, who was a union leader, returned from organizing farm workers in Gilroy, California, to announce that he had fallen in love with someone else. My mother thus found herself as a single mom in the 1940s, a situation considered quite a disgrace to her immediate family. Her response to this onslaught of rejection was to sell off all of her belongings (including our home) and to move to a beachfront



Figure 1

My sister (on right) and I on the street outside our home in post–World War II Philadelphia, 1948.

residential hotel in Miami Beach. This was how my early, formative years unfolded: largely living as a carefree, fairly unsupervised "beach bum."

The return to Philadelphia coincided with the end of our savings, at which point we went to live with my aunt and her family and my mother went to work. It was not very long before my mother met and married the man who was to become my stepfather. At that point, life changed dramatically, as we approximated the ideal/norm of that time, comprised of mom, stepdad, stepsister (almost one year my senior), and me (**Figure 1**). With the arrival of the 1950s and the beginnings of an era of extreme conservatism that included McCarthyism (my uncle lost his job) and the Cold War (my sister and I identified where to hide in the event of a nuclear attack), this new family configuration offered me a much-needed blanket of security and conformity that lasted until I left home to go to university.

TURNING TOWARD SCIENCE

As a young girl, my original passion was ballet, and incredibly, in the context of the excessive protectiveness of modern-day parenting, I was allowed to take public transportation alone to a Center City studio where I trained with the "maestro." My heroine in those days was Anna Pavlova (rather than Marie Curie), and my exposure to science was nil until 9th grade. Although I attended a huge inner city school, where the hero was the to-be-famous basketball player Wilt "the Stilt" Chamberlin, there was a strong program for a cohort of approximately 30 of us labeled as academically

gifted. My interest in ballet morphed first into a focus on French language and literature, then a life-changing series of classes on physics and chemistry completely altered that plan. These science classes, taught by a dynamic, committed, and curmudgeonly duo (of men), were truly the inspiration that led me to a lifetime of science. My experience at school contrasted with the pressure at home to be a "traditional woman," though my stepdad's training in engineering at Drexel University was an incredible asset when I encountered my first roadblocks in physics and math.

Although our family was solidly middle class, my parents balked at sending me to the University of Pennsylvania (U Penn), the far more expensive choice in relation to Temple University. Their agreement was that I could attend U Penn if I was offered a scholarship (I was) and if I commuted from home (I did). Later, seeing how hard the long commute was for me, my mother went back to work to provide me the funds so that I could live on campus.

THE WORLD OPENS UP

Attending U Penn was another fortunate turn of events. In 1958, I was officially admitted to the College for Women. On the one hand, we were encouraged to convene for afternoon tea and all of its rituals; however, I gravitated toward an intoxicating mix of new friends, the new stirrings of political activism on campus, and the opportunity to explore both the humanities and science. With a cohort of researchers interested in pursuing science at the interfaces of chemistry, biology, and physics, U Penn turned out to be a perfect environment in which to hone my interests. My first "serious" research experience was as a laboratory technician at the Johnson Foundation, a magnet for world-class researchers in biophysics. In spite of my proclivity to break pH electrodes and my general terror in the face of so many giants, I thrived there and was encouraged by senior scientists, including Britton Chance, Ron Estabrook, and Helen Davies.

Finishing with an A.B. in chemistry in 1962, I moved to New York University (NYU) downtown for my first year of graduate studies. I had been denied a teaching assistantship at Columbia University, my first choice, but in the end found the Bohemian environment of the Village to suit that time of my life. Although I only stayed one year, I am indebted to NYU for opening my eyes to the excitement and beauty of organic reaction mechanism.

FAMILY AND TRAVELS

Given the prodding from my nuclear family to lead a traditional life, as well as the social climate of the early 1960s, it is not surprising to me that I was married by the end of my first year of graduate school. My husband, Norm Klinman, and I were a perfect match for that time in our lives. He was a recent MD whose strong interest in science led to his enrolling in a PhD program in immunology at U Penn and, later, a brilliant career in immunology that included the first preparation and characterization of monoclonal antibodies (1). I turned back to my alma mater for further graduate training, continuing a relationship with U Penn that led to my PhD in 1966, an honorary degree in 2006, and receipt of the Penn Chemistry Distinguished Alumni Award in 2018. My choice of a PhD advisor in chemistry was Edward Thornton, a wunderkind from the laboratory of Frank Westheimer. Many credit Westheimer for creating the field of mechanistic enzymology. Given a predetermined, short time line before my husband and I were scheduled to move on to postdoctoral studies, the next three years were spent intensely focused on solution studies of the role of imidazole in catalysis. Somehow, I managed to complete a thesis and to give birth to my first son during this time. I owe a great deal to my mother-in-law, Miriam, who stepped in to provide loving childcare as our deadline for departure approached. It did not hurt that I was only 24 years old at the time—a fountain of energy!

Our next stop in 1966 was Israel, for a 14-month visit to the Weizmann Institute in Rehovot. Israel was a young, idealistic, and pioneering country, with a primary goal to put food on the table and a lifestyle free of televisions and telephones. Chaim Weizmann had been a chemist in Manchester and a Zionist; the research institute he founded was destined to become world class. I arrived in Israel pregnant with my second son and ready to begin work with David Samuel, who had promised access to approximately 100% oxygen-18 enriched water for mechanistic studies. The next period was filled with the birth of my second son, new friendships, discovery, and unanticipated drama (that included the Six-Day War of 1967). Scientifically, I designed and performed a series of experiments aimed at understanding the role of metal ions in the hydrolysis of high-energy acyl phosphates using the labeled water promised by David Samuel. This was a timely topic, as Peter Mitchell's chemiosmotic theory of ATP production was not yet proven or generally accepted, and researchers were still focused on the possible role of a chemical intermediate (i.e., the elusive X~P). These studies ignited in me an interest regarding the role of metal ions in enzyme catalysis, a subject I have continued to pursue until the present day. The younger scientists who are reading this account may also be surprised to learn that it was during this time that the first X-ray structure of an enzyme (lysozyme) was reported. I heard about it firsthand, when David Phillips visited the Weizmann Institute from the University of Oxford to present a seminar on his spectacular advance.

Although we left Israel in January of 1968, we would not return until the following summer to the United States, where my husband had a faculty position waiting for him at the University of Pennsylvania Medical School. The interim period was spent living in London, where I was able to wrangle a nonpaying apprenticeship at University College in the laboratory of Charles Vernon, ironically one of the scientists involved in dispelling the notion of a high-energy chemical intermediate in oxidative phosphorylation. It was during this time that I began to delve seriously into biochemistry, in preparation for joining the laboratory of Irwin Rose at the Fox Chase Cancer Research Institute upon our return to Philadelphia.

HOME AGAIN

The late 1960s and 1970s were a convulsive time, both for US politics and for my personal life. Upon our immediate return to Philadelphia in 1968, we settled into a high-rise apartment in Center City, and I began an association with The Institute for Cancer Research (ICR) (a part of the Fox Chase Cancer Research Institute), where I was to remain for a decade. The initial years as a postdoctoral associate with Irwin (Ernie) Rose were incredibly challenging and satisfying. Those days, though circumscribed by a very tight commuting schedule between the downtown and outskirts of Philadelphia, were my initiation into experimental biochemistry—and I loved every minute of the experience. Despite the designation as a center for cancer research, there was a strong commitment to basic research. Much to my delight and future training, ICR had become a world magnet for researchers interested in the study of enzyme mechanism and, in addition to Ernie Rose's laboratory, there were quite a few related research groups, for example the laboratories of Larry Loeb, pursuing the mechanism of DNA repair; Jenny Glusker, studying protein X-ray crystallography; and Al Mildvan, who had established a center for NMR mapping of metal centers in enzymes.

After much discussion with Ernie, I chose a project that would use my previous training in chemistry to study an enzyme-catalyzed isomerization of *trans*-to *cis*-aconitate, in which the former is a polyanion that accumulates in plants and the latter is an intermediate in the conversion of citrate to isocitrate in the citric acid cycle. Using a series of isotope probes (deuterium, tritium, and carbon-14), we could show that the isomerization did not require any bond rotations at all and was

achieved by a simple 1,3 prototropic, suprafacile proton shift within a suitably aligned substrate via the participation of a single base on the enzyme (2). A bonus of this study was the ability to use this newly characterized property of aconitate isomerase to analyze the stereochemical outcomes of two enzymes that catalyze aldol cleavages/condensations involving chiral methyl groups (3). This was the first instance in which Ernie and I seriously matched wits, each of us going home with the newly collected data to determine whether the new bonds were related by a retention or an inversion of configuration. The beauty of biological stereochemistry is that generally only two answers are possible, with a 50:50 chance of being correct. Although the fact that we had different answers to the problem, and mine was correct, may have been fortuitous, this felt like a monumental step in the direction of forging my own identity as a scientist. Many years later, near the end of his life, Ernie confessed to me that he had initially been quite uncertain about my future in science.

MOVING TOWARD SCIENTIFIC INDEPENDENCE

By 1970, I was itching to start my own research laboratory and looking for projects that would be both original and forward-looking. This was an era when kinetic probes were undergoing heavy development for the study of the chemical mechanisms of enzyme reactions. One issue that troubled me was a lack of knowledge about rate-limiting steps, raising serious issues of interpretation. Could I use my graduate school training, in the use of solvent kinetic isotope effects (KIEs) to study acid–base catalysis of small-molecule reactivity, to good effect? Though solvent KIEs were beginning to be applied to enzyme reactions, I wanted a probe that would permit a more straightforward interpretation. The $NAD(P)^+$ cofactors looked like a good place to start, with a growing number of enzymes dependent on these cofactors undergoing structural and kinetic characterization. Importantly, the reduced form of $NAD(P)^+$ had been synthesized with nonexchangeable isotopes in their reactive positions. I chose yeast alcohol dehydrogenase as a test system and studied its reaction in the forward and reverse directions using either labeled cofactors or a series of ring-substituted benzyl alcohols (with protium or deuterium in their reactive positions). This approach provided a combined analysis of isotope effects and structure reactivity correlations, in which the KIEs could be used to show conditions in which hydride transfer was rate limiting, and the substituents on the alcohol ring would provide insight into charge distribution at the enzymatic transition state (4, 5). This approach, toward the application of a combination of physical–organic probes to the study of enzyme mechanism, is now a mainstay in the field. Looking back, I am indebted to Ernie, who both provided funds from his own research grant for these studies and was fully in support of my early sole-author publications.

A LABORATORY OF MY OWN

By 1972, in part catalyzed by the offer of an Assistant Professorship from UT Southwestern, ICR appointed me to an equivalent research position, and I moved into the ranks of the staff scientists. By this time, my two sons were 5 and 7 years old, and at 31 years old myself, I was filled with excitement for my personal and professional future! Alas, my life has shown itself to be a long winding path, with repeated lessons of change and unexpected consequences.

My first independent laboratory was quite small, with space for at most four researchers. I did not feel deprived and had free access both to equipment in Ernie's adjacent laboratory and to that of other researchers at ICR. A strong cohort of young scientists in my age group was at the ICR, and we became good friends and colleagues. This group included Ruth Angeletti, Helen Berman, and Byron Rubin. Both Helen and Byron were trained in X-ray crystallography and

Ruth in protein chemistry with a strong interest in the biology of chromaffin vesicles. Ruth was influential in my second choice of a long-standing research project, pointing out the fascinating copper enzyme, dopamine β-monooxygenase (DβM), that is primarily located within the storage vesicles of adrenal glands and the sympathetic nervous system. It would be many decades before a suitable protein expression system would become available for biochemical studies of DβM and related enzymes. I remember well the days spent dissecting out the medullae of bovine adrenals as a starting point for the preparation of pure enzyme. I would cover the laboratory bench tops with padded paper, and the entire crew of friends would show up to aid in the process, some of us using a device invented by Byron to speed the dissection. What is so astonishing, in the context of the pace of research today, is the number of friends who were willing and happy to contribute their time to this tedious procedure.

I was also fortunate in having a handful of excellent coworkers in the laboratory: Kate Welsh, Judy Voet, Hope Humphries, Don Creighton, Radmila Markovic, and Michael Summers. The use of isotopes remained a prominent theme in these early studies, with Kate and Don continuing to pursue deuterium isotope effects in dehydrogenase reactions, and Judy and Hope studying tritium isotope effects in DβM. It had been assumed, prior to studies of DβM, that reactions catalyzed by O_2-dependent enzymes would need to bind their substrate(s) prior to the creation of a site for O_2; however, Judy and Hope's results showed very clearly that the addition of substrate and O_2 was random, implying a mode of interaction of protein with molecular oxygen that was independent of the presence of substrate (6). At the suggestion of Michael Summers, a postdoc initially in Ernie's laboratory, I also became interested in a second class of copper proteins, the copper amine oxidases (CAOs). Although Michael was primarily interested in the stereochemistry of these enzymes (7), he pointed out to me the confusion and uncertainty regarding the active site cofactor, a problem we would finally resolve many years later. In retrospect, it is clear that many of my scientific values and interests were forged in these early years at ICR.

WESTWARD HO

By 1975, my marriage was failing, and a messy and painful personal transition to a new kind of independence was taking root. As it became increasingly evident that divorce was inevitable, I needed to step back and take a hard look at both my options and my heart's desires. The protracted United States–Vietnam War was just coming to an end. The accompanying breakdown in societal norms that had resulted from extended social protest of this war had taken its toll at many levels including the personal lives of many people of my generation. The West Coast had emerged as a growing progressive place, with outstanding public education at the university level. I "screwed up my courage" and wrote letters to two University of California campuses: University of California, Los Angeles (UCLA) and University of California, Berkeley (UC Berkeley) to inquire about possible positions. The Department of Chemistry at UCLA wrote back saying they were not interested, while the Chemistry Department at UC Berkeley wrote to invite me for an interview. After many interactions and negotiations (and with a great deal of intimidation), I accepted their offer of an Associate Professorship. I did not realize at the time that this appointment was as the first woman not only in chemistry (**Figure 2**) but in all of the physical sciences at UC Berkeley. Importantly, the biophysicists (including the theorist Bob Harris and his wife Christine) within Hildebrand Hall were quite welcoming, and I was assigned a large laboratory space in Hildebrand Hall that had been occupied by Jim Wong prior to his relocation to Harvard University. In the interim between Philadelphia and Berkeley and after I had sold our home in Mount Airy, my two sons and I went to live with Helen Berman and her husband, Peter. Together with a visiting scientist from New Zealand, we created our own version of a 1970s hippie family



Figure 2

The members of the Department of Chemistry at the University of California, Berkeley, shortly after I joined the faculty in 1978. Photo courtesy of the University of California, Berkeley.

that was nurturing, fun, and I think a great shock to my in-laws when they would come to visit their grandsons.

EARLY YEARS AT UC BERKELEY

By the time I arrived in Berkeley in 1978, my two sons were 11 and 13, on the cusp of full-blown adolescence, and I had never taught more than two or three classroom lectures a year. The new responsibilities, especially as a single parent in a new city, were overwhelming. Two important components critical to success were the strength and character of my sons, Andrew and Douglas (a well-justified prejudice of Mom), and my decision to vigorously prioritize my family and research/teaching, with very little time or energy left over for coffee klatching with colleagues and administrative duties. After some sampling of different teaching responsibilities, I began a cycle of alternating between lecturing in a second-year organic chemistry class tailored to biologists and a graduate course on enzyme mechanism that had been originally developed by Jack Kirsch and that we proceeded to teach jointly over many decades. At the same time, I attracted a small and highly capable core of researchers: Sue Miller, now professor at the University of California, San Francisco, and Natalie Ahn, now professor at the University of Colorado, Boulder, my first two graduate students; Monica Palcic, the first postdoc to join my Berkeley laboratory (from Canada); and Matt Krueger, an unassuming and highly efficient laboratory technician. The early research projects were, in part, an adaptation of the approach I had initiated at the ICR on the dehydrogenases, with Sue using the combined interrogation of isotope effects and structure reactivity relationships to show that DβM functions via a hydrogen atom abstraction mechanism (8) and Natalie combining isotope effects with pH and the DβM effector fumarate to understand the roles of acid base catalysis and anion activation (9). Natalie bravely took on a second project that examined the activity of DβM within its *in vivo* subcellular compartment, the norepinephrine storage vesicles of the adrenal gland that we had spent many years throwing away! Though this project attracted very little attention at the time, it was an important effort at extending the

in vitro characterization of an enzyme to its more biologically relevant contexts (10). During this time, Monica initiated our first forays into the kinetic and chemical mechanism of the CAO from bovine serum (11), bringing our focus more and more in the direction of metallobiochemistry. The growing theme of linking bioinorganic, bioorganic, and biophysical approaches to enzyme studies grew more robust with the next wave of graduate students and postdocs, leading to many mechanistic insights into the copper-containing enzymes DβM and the CAOs. An exciting collaboration between Dale Edmondson at Emory University and Mitch Brenner in my laboratory led to the important discovery that DβM requires two uncoupled copper ions per active site to function (12); this finding was followed by single-turnover, rapid mixing experiments to show that both of the (prereduced) copper centers undergo rapid reoxidation in a single phase that correlates with the time constant for the formation of norepinephrine from dopamine (13). The presence of two uncoupled copper centers was later confirmed by X-ray crystallography (14), introducing the new challenge of understanding how spatially distant copper centers separated by solvent water molecules are capable of supporting a highly controlled oxygen insertion into dopamine (15, 16), in the absence of any of the uncoupling reactions (17) commonly seen in monooxygenases. Our most recent proposal of a long-range proton-coupled electron transfer (PCET) (16) is relevant to many biological processes and has emerged as a highly active research area at the interface of theory and experiment.

SECOND DECADE AT UC BERKELEY AND THE DISCOVERY OF QUINO-ENZYMES

An unexpected and remarkable intersection of metallobiochemistry and cofactor chemistry emerged in the mid- to late 1980s, with the appearance of reports from the Netherlands that a significant number of eukaryotic proteins required a low-molecular-weight quinone cofactor, pyrroloquinoline quinone (PQQ), for function (18). This claim captured our interest for several reasons. To start with, PQQ had been assigned the role of a freely dissociable cofactor in a range of bacterial enzymes and had, until that time, no associated role in eukaryotes. Further, two of the enzyme systems concluded to require PQQ were DβM and the CAOs, and these enzymes had already been subjected to extensive study in my laboratory. In no instance had we observed a requirement for low-molecular-weight, dissociable cofactors other than copper ion, and importantly, the available kinetic and mechanistic properties seemed to be well rationalized in the absence of such factors. That said, there remained the continuing enigma of the non-dissociable cryptic cofactor in the CAOs, which had been ascribed to an aldehyde that either resembled or was derived from pyridoxal phosphate (19).

The major deterrent to identification of the implied organic cofactor in the CAOs was its inability to be isolated in a reproducible form following protein hydrolysis into peptides. Working on the premise that the cofactor contained a carbonyl functional group, but in a form that was chemically reactive, we proceeded to derivatize enzyme with the carbonyl reagent phenylhydrazine (20). This was intended both to produce a stabilized structure from the native carbonyl and to impart a UV/Vis absorbance band that could be easily detected outside of the background absorbance from the remaining protein-derived peptides. These studies were performed prior to the availability of a recombinant form of CAO and the enzyme used, bovine serum amine oxidase (BSAO), was obtained the old-fashioned way from bovine blood collected at a slaughterhouse! I was fortunate to have a truly gifted graduate student, Susan Janes, who excelled at everything the project required—from protein purification, derivatization, and hydrolysis, to peptide isolation in yields that were high enough to enable both mass spectrometric and NMR analyses of the resulting peptide product. In this manner, the structure of the CAO cofactor was identified

as 6-hydroxydopa (termed topa quinone or TPQ), previously known to be present *in vivo* as a hydrolysis product of dopa with demonstrated toxic properties (20). We were quite amazed that nature would have evolved an enzymatic cofactor that, when sequestered within the confines of an enzyme active site, could be used to promote the selective oxidative deamination of a range of biological amines.

The identification of TPQ set off a flurry of research activity around the world, with many new protein-derived quinone cofactors emerging over the following decade that included our own discovery of lysine tyrosylquinone (LTQ) in bovine aorta lysyl oxidase (21). These cofactors are all structured around an aromatic side chain (Tyr or Trp) that subsequently undergoes hydrolysis (TPQ) or covalent attachment to a second amino acid such as Lys (LTQ), Trp [tryptophan tryptophanylquinone (TTQ)], and Cys [cysteine tryptophanylquinone (CTQ)]. In all instances, the enzymes containing these cofactors function via two sequential steps, with the substrate amine first undergoing oxidation and hydrolysis to release ammonia, an aldehyde product, and the reduced, quinol form of the cofactor. A mechanistic divergence occurs in the recycling of the reduced cofactor, and this occurs either via reduction of O_2 (for the oxidases that contain TPQ and LTQ) or via electron transfer to another protein (for the dehydrogenases associated with TTQ and CTQ). The four cofactors also segregate with regard to the mechanisms of production of the protein-bound cofactors (22). In subsequent years, a major focus in my laboratory was on the eukaryotic-derived oxidases that contain TPQ and LTQ, with experiments demonstrating that biogenesis occurs as an autocatalytic process that depends solely on active site Cu^{2+} and molecular oxygen (23, 24). Once the mechanistic aspects of this process were uncovered, we could step back and marvel at how a single enzyme active site could have evolved to perform both O_2-dependent hydroxylation (as required for biogenesis) and O_2-dependent oxidation (as occurs during amine oxidation) (25). Many talented students and postdoctoral associates contributed to these studies, including Minae Mure, Sophie Wang, Jen DuBois, Ben Schwartz, Joanne Dove, Danying Cai, David Mu, Julie Plastino, Joanie Hevel, and Steve Mills. A network of collaborators including structural biologists (Scott Matthews and Carey Wilmot) and spectroscopists (Joann Sanders-Loehr) also aided immensely (both professionally and personally) to progress in this area. In parallel studies, other laboratories elaborated the quite different strategies for the production of TTQ and CTQ, prokaryotic cofactors whose formation is dependent on a suite of additional enzyme activities encoded within specifically tailored operons (26, 27). Although the mechanistic aspects of catalysis and biogenesis in this field are fairly well established, much remains unknown regarding the biological role of the TPQ-containing enzymes in eukaryotes, which are found ectopically in the vasculature and in adipocytes. In the former case, evidence of a role for the enzyme in immune cell recruitment has been found (28), whereas adipocytes may also use the TPQ-dependent CAO to initiate an inflammatory response (29). This aspect of the quino-enzyme field is wide open and ripe for new discoveries that relate to amine-based peroxide signaling. Additionally, the implication of a redox role for the bacterially produced PQQ in mammalian physiology is growing (30, 31).

THE DISCOVERY OF ROOM TEMPERATURE QUANTUM TUNNELING IN ENZYMATIC C-H CLEAVAGE REACTIONS

The Gordon Research Conference on Isotopes, initiated after World War II and continued until 2014, played a key role in a second, major research direction within my laboratory at UC Berkeley. This biennial meeting brought together a seemingly disparate group of chemists, geologists, environmental and atmospheric scientists, and biologists with a shared interest in using isotopes to study their individual disciplines. At the core of this recurring ingathering was a curiosity and

openness to new ideas and disciplines. For a long time, the meeting was reigned over by Jacob Bigeleisen, who had worked at the Manhattan Project on the extraction of uranium-235 from uranium ore and later coauthored (with Maria Goeppert-Mayer) the classic article on the use of statistical mechanics to formalize the impact of isotopic substitution on rate and equilibrium processes (32). I began to attend this conference with regularity in the 1970s and was thrilled (and terrified) when I was asked to talk about our early development of isotope effects for the study of enzyme mechanism. With time, the name of this Gordon Conference evolved from "Isotopes in Chemistry and Physics" to "Isotopes in the Biological and Chemical Sciences," to represent the growing presence of biochemists contributing to the meetings.

Although the use of statistical mechanics and transition state theory continued to dominate the theoretical framework of isotope effects for many years, new perspectives were welcomed [such as variational transition state theory developed by Don Truhlar (33)] as were other observations outside of the mainstream [such as mass-independent isotope effects in atmospheric chemistry demonstrated by Mark Thiemens at the University of California, San Diego (UCSD) (34) and the role of nuclear quantum effects in biological hydrogen transfer reactions implied from the early work of Mo Cleland at the University of Wisconsin and ourselves (35, 36)]. Although hydrogen tunneling had been recognized earlier at the isotopes conference, it had never been fully embraced for fear that the experimental designs and observations were flawed, especially in the context of free radical reactions, which can be notoriously difficult to study in solution. Enzymology was to play an important role in changing opinions about the importance of tunneling, in particular because of the much more controlled reaction pathways within the confines of enzyme active sites.

My interest in the possibility of detecting hydrogen tunneling in enzyme reactions had been brewing for quite a while, leading in the mid-1980s to a summer sabbatical in the laboratory of Pierre Douzou, an experimental physical chemist at the Institut Pierre and Marie Curie who had developed cryo-solvents to study enzyme behavior at low temperature (37). During this time, my focus was on low temperature studies, as I thought this would be the only way to detect significant tunneling in enzyme systems. Pierre Douzou and his associates were charming and gracious hosts; however, my attention was split between the laboratory and the allure of Paris. The latter was shared with my mother as well as my older son and some of his friends, who descended on (or rather ascended to) a 5th floor walk-up apartment near the Arc de Triomphe.

As it turned out, studies at low temperature for the observation of tunneling proved unnecessary, as emerging data and discussions revealed that hydrogen tunneling could be inferred at room temperature from deviations in the magnitude of secondary KIEs measured with substrates that contained specific patterns of protium, deuterium, and tritium. Following the laborious work of synthesizing the appropriately labeled substrates, together with the development of appropriate enzyme assay methodologies, we succeeded in publishing our first study implicating room temperature tunneling in the hydride transfer catalyzed by yeast alcohol dehydrogenase in 1989 (38). Over the next decade, the available toolbox for detecting tunneling expanded to include the size and temperature dependence of primary KIEs using assays that allowed kinetic isolation and analysis of the hydrogen transfer step (e.g., 39–41). As this field developed, it initially encountered considerable resistance with claims stating that our data must be wrong and, when that was shown not to be true, that the findings were likely irrelevant to biology. Now, from the vantage point of 2019, and as a consequence of tremendous research efforts from many different laboratories throughout the world over a period of more than three decades, the role of quantum tunneling for all major classes of enzymatic C-H cleavage reaction is accepted, and the implications for catalysis have changed the way we think about enzyme function (discussed below).

NEW WAYS OF LOOKING AT ENZYME CATALYSIS

A number of developments occurred in my laboratory during the 1990s that would propel studies of tunneling into new domains of relevance: first, to physical models for the origins of enzyme catalysis and, second, to the development of theories for PCET processes at room temperature. One experimental advance was the use of site-directed mutagenesis to examine the contribution of bulky hydrophobic residues to catalytic efficiency in enzymes. This advance meant moving away from a dominant focus of mutagenesis on charged and hydrogen-bonding protein side chains implicated in acid/base and electrostatic catalysis and toward the participation of large hydrophobic side chains in "tuning" active site reactivity. An early collaboration between Jodie Chin, a graduate student in my laboratory, and two X-ray crystallographers, first Barry Goldstein and later Brian Bahnson, would lead to the demonstration that reduction in the size of a single active site hydrophobic side chain could alter, concomitantly, the degree of deviation of secondary KIEs from their semiclassical limits (our earliest experimental reporter of hydrogen tunneling) and the distance between the reactive carbon centers of the bound cofactor and a substrate (42). This first structural intimation of the importance of "active site close packing" in enzyme catalysis had its genesis in a mini-sabbatical to the University of Rochester. I had arrived to work with Bill Saunders, an expert in computer-based analysis of secondary isotope effects and an early proponent of the contribution of tunneling effects in room temperature solution reactions (43). Byron Rubin, a good friend from earlier days at the ICR and now affiliated with the University of Rochester, offered me a room in his home and later introduced me to Barry Goldstein and his wife Andrea Barrett (who was immersed in writing her novel *Ship Fever*, which would go on to win the National Book Award). Within this mixture of deep friendship, exciting cultural tendrils, and cold enough weather to send us out ice skating together, Barry and I initiated our structure/function collaboration. So began the recognition of the importance of donor-acceptor distances in the manifestation of the properties of active site hydrogen tunneling and, ultimately, in enzyme reactions in general.

Serendipity has always played a role in the emergence of new research directions in my laboratory and that was certainly the case when Michael Glickman took up the subject of hydrogen transfer in the soybean lipoxygenase (SLO) for his PhD thesis. Given the ability of KIEs to sort out detailed enzyme kinetics, Michael began a series of careful studies comparing the reactivity of protio- and perdeuterio-labeled linoleic acid substrates. When his first measurements produced a KIE of 80, I sent him back to the laboratory. (Up to that point, the upper limit for k_H/k_D near room temperature was predicted to be approximately 7, though values as high as 10–12 had been seen and rationalized by the contribution of a small tunneling correction). Several years and reams of controls later, Michael stood fast (44, 45), and I reported his results at a Gordon Conference to an audience that was skeptical but also intrigued. After my talk, Dexter Northrup commented that he remembered a graduate student from the University of Wisconsin who had seen a huge KIE with lipoxygenase, but no one had believed his finding and it laid buried in an unread PhD thesis. Quickly, theoreticians entered the fray in an effort to explain the experimental observations, with an important new perspective put forth in 1999; this emanated from a group of Russian electrochemists who had been postulating for quite a few years that quantum effects would dominate hydrogen transfer near room temperature (46). Over the next two decades, and with the help of many talented graduate students and postdoctoral researchers (Thor Jonsson, Michael Knapp, Matt Meyer, Keith Rickert, Sudhir Sharma, Adam Offenbacher, and Shenshen Hu) and expert collaborators [Sharon Hammes-Schiffer in theory (e.g., 47, 48) and Brian Hoffman in spectroscopy (e.g., 49)], lipoxygenase emerged as a workhorse and prototype of enzymatic hydrogen tunneling. Ironically, the originally controversial KIE of 80 is now seen to be small in relation to the room temperature kinetic isotope of 661 ± 27 observed for a variant of

SLO in which two bulky hydrophobic side chains have been pared down to alanine (48). These studies emphasized the need for revision of earlier dominant theories regarding the origin of primary KIEs as arising solely from changes in vibrational frequencies (50).

Perhaps most importantly, the inherent properties of quantum mechanical tunneling in lipoxygenase along with many other systems have necessitated a reexamination of the origins of enzyme catalysis, showing how any description of enzymatic reaction coordinates must take into account a temporal and spatial hierarchy of protein motions (e.g., 51, 52). In recent experiments, designed to obtain a spatial resolution of such motions in SLO, Adam Offenbacher and Shenshen Hu employed time-, temperature-, and mutation-dependent hydrogen–deuterium exchange (HDX) to identify a protein dynamical network that communicates between the protein–solvent interface and the active site, 15–30 Å away (53). The role of protein dynamics and their link to specific protein networks has emerged as an important new direction in enzymology, not only for understanding function but also as a basis for the future design of new catalysts that may be able to approach the enormous rate accelerations characteristic of enzymes.

The 1990s also led to a fortuitous meeting with Amnon Kohen while attending a chemistry conference in the United Kingdom. Amnon was about to complete his PhD studies at the Technion in Haifa and was looking for a laboratory where he could expand his skills in enzyme kinetics. After hearing me talk on the role of hydrogen tunneling in enzyme catalysis, his enthusiasm seemed unbridled and a year later he arrived at UC Berkeley for postdoctoral training. During this time, researchers were addressing how families of prokaryotic proteins are able to sustain their function across a wide temperature range, especially given that the 3D structures of proteins that function at high and low temperature often appear to be essentially identical. My interest in this area was further stimulated by a fascinating sabbatical visit to the University of Waikato in New Zealand to work with Roy Daniel, one of the world experts in the isolation and characterization of proteins from thermal vents. A thermophilic homolog of the yeast alcohol dehydrogenase had just been isolated and partially characterized by researchers in Italy (54), and Amnon and I entered into a collaboration with Simonetta Bartolucci and her coworkers with the goal of relating protein activity to thermal stability. Remarkably, a kinetic break in behavior was seen at 30°C with this thermophilic protein (designated ht-ADH) when measuring either the enthalpy of activation controlling hydride transfer or the isotope effect on this step (55). The behavior of the latter was initially very puzzling to us, indicating a temperature *independence* of the KIE in the physiologically relevant temperature range of ht-ADH that transitions to temperature-dependent behavior below the break point. This change in behavior of the KIE with temperature for ht-ADH—both fascinating and difficult to fully understand until several years later—is now understood in the context of multidimensional hydrogen tunneling models (55a). Important for our understanding of optimal enzyme function, the findings with ht-ADH demonstrated the critical link between protein flexibility and the creation of tightly packed active site structures (56–58).

Many researchers in my laboratory carried these findings on prokaryotic ADH proteins in important new directions. A new postdoctoral associate, Zhao-Xun Liang, in collaboration with Natalie Ahn at the University of Colorado, carried out the first hydrogen–deuterium exchange linked to mass spectrometry (HDX-MS) in our laboratory, showing a temperature break for HDX that correlated with the temperature break observed earlier for catalysis (59). The changes in HDX detected for ht-ADH (and subsequently in a psychrophilic variant, ps-ADH) occur only in select regions of protein (59, 60). Zac Nagel later introduced mutations at bulky hydrophobic side chains within the active site of ht-ADH, showing how site-specific mutagenesis further exacerbated the impairment of ht-ADH at low temperature (61, 62). By way of comparison of ht-ADH with ps-ADH, Zac also uncovered a pathway for dynamical communication between a subunit interface in ht-ADH and the bound substrate over a distance of approximately 17 Å (63).

Following up on these studies, Corey Meadows introduced the measurement of fluorescence lifetimes and Stokes shifts using engineered forms of ht-ADH with single tryptophans (64, 65). Fluorescence studies were then extended by Jianyu Zhang to include T-jump Förster resonance energy transfer (FRET) measurements on ht-ADH (in collaboration with coworkers in the laboratories of Tom Spiro and Brian Dyer). In an exciting turn of events, a microsecond FRET has been detected above the 30°C break point in catalysis that displays an activation energy that is almost identical to the previously measured enthalpic barrier for catalysis. These studies directly link regional protein motions to the enthalpic barrier for active site tunneling (66). With feasibility established, it should be possible to systematically extend such studies to a wide range of enzyme reactions, ultimately testing the generality that spatially resolved protein motions, leading from solvent to an enzyme active site, are an essential and evolved aspect of biological catalysis.

Although this type of approach to experimental biology—the placement, one by one, of many "solid bricks of data"—requires time and patience, with luck and persistence a robust structure for understanding nature can emerge. This is exactly what has happened as a result of studies of SLO and ht-ADH, which, when combined with decades of studies of hydrogen tunneling in many other enzyme systems, have given way to a new framework for our understanding of the physical origins of catalysis (52). It is of note that recent studies of enzymatic methyl transferases (67–69) further implicate active site compaction as a key element in the ability of enzymes to achieve enormously high rate enhancements relative to their solution counterparts.

OXYGEN ACTIVATION

From an evolutionary point of view, the complexity of current life on Earth is, simply put, remarkable—and directly attributable to the emergence of aerobic life and the use of molecular oxygen (O_2) as the terminal electron acceptor in respiration. A "dark side" to life in the presence of atmospheric O_2 also exists—a result of the series of one-electron steps whereby O_2 normally undergoes reduction to water, generating free radical intermediates capable of carrying out damaging oxidative side reactions.

Looking back, the focus on O_2 activation in my laboratory was a natural outcome of our focus on C-H activation, given the large number of enzyme systems that directly couple O_2 reduction to subsequent C-H cleavage. Some of the mechanistic questions that captured our attention over the years included the following: (*a*) Can O_2 bind to an enzyme prior to the formation of the enzyme–substrate complex (6)? (*b*) Is the interaction of O_2 with a protein-bound transition metal a prerequisite for O_2 binding? (*c*) Or are discrete binding pockets, generated by hydrophobic side chains, capable of sequestering active site O_2 for subsequent reaction (70, 71)? (*d*) Can proteins create efficient networks to guide the movement of O_2 from solvent toward the reactive carbon(s) of a bound substrate (72–74)? Studies relating to these questions continue to attract the attention of researchers in a wide range of enzyme systems—recently, for example, in the nitrogenase reaction where it is critically important to distinguish the binding and reaction of substrate N_2 from the inhibitory O_2 (75).

Additionally, there is the historical and continuing focus on the nature of O_2-derived intermediates that give rise to substrate oxidation (expected to be critically dependent on the energetic demands of the reaction being catalyzed as well as the availability of enzyme active site features for stabilization and sequestration of reactive intermediates). As with hydrogen and its three isotopes (H, D, and T), oxygen also offers three isotopes (^{16}O, ^{17}O, and ^{18}O) as mechanistic probes. Although our primary focus was, over many years, the comparison of ^{16}O with ^{18}O, an early study made use of all three oxygen isotopes in the context of looking for alternative explanations to the huge (nonclassical) KIEs first seen in the lipoxygenase reaction.

In 1994, a proposal was put forth by Charles Grissom at the University of Utah that the inflated hydrogen effects in lipoxygenase might be the result of a differential magnetic coupling between the high-spin active site iron center of lipoxygenases and protio- versus deuterio-labeled substrates (76). To test this hypothesis, he and his coworkers applied an external magnetic field to the lipoxygenase reaction mixture and measured its impact on the size of the experimental k_H/k_D. Although no impact was detected, it remained possible that an internal, active site iron-induced magnetic field could be operative. Michael Glickman, the student who initiated hydrogen isotope effect studies on lipoxygenase, reasoned that such a magnetic effect might become apparent in the rate of combination of the enzyme-bound, substrate-derived radical intermediate with molecular oxygen to yield the lipid hydroperoxide product. Setting up a collaboration with Mark Thiemens at UCSD, they proceeded to compare $k_{O\text{-}16}/k_{O\text{-}18}$ with $k_{O\text{-}17}/k_{O\text{-}18}$ for the second partial reaction of lipoxygenase, in which O_2 traps the intermediate linoleyl radical (77). Alas, no deviation from mass-dependent effects was seen with molecular oxygen, clearing the path for a full quantum mechanical interpretation of the nonclassical deuterium isotope effects in SLO.

Our ability to compare the relative reactivity of ^{16}O, ^{17}O, and ^{18}O was a result of Thiemens's developed methodology for the direct detection of the isotopic composition of unreacted O_2. More generally, oxygen isotope effect measurements have followed a protocol developed by Joe Berry at the Carnegie Institute in Palo Alto that involves separation of unreacted O_2 from N_2, combustion of the O_2 to CO_2, and analysis of the resulting CO_2 by mass spectrometry (78). Use of this latter technique in my laboratory was initiated in 1992 by Gaochao Tian and proved to be quite adequate for several decades of measurement of $k_{O\text{-}16}/k_{O\text{-}18}$ in a range of enzyme systems.

Gaochao was a brilliant postdoc, newly arrived from mainland China, who made the decision to first measure the magnitude of equilibrium ^{18}O isotope effects in well-understood Fe and Cu systems that act as reversible O_2 carriers (79), thereby allowing the creation of a "ruler" for subsequent analyses of kinetic measurements by Gaochao (80) and, later, many others (e.g., 81–84). Although interpretation of such kinetic measurements can be complicated by the presence of multiple rate-determining steps as well as compensatory transition-state fractionations, the ensuing work was able to distinguish patterns that are correlated with the type of chemistry catalyzed. Perhaps most important was the use of a double isotope effect method, introduced by Mo Cleland at the University of Wisconsin in the context of other types of enzyme reactions (85). In the case of both Fe- and Cu-dependent oxygenases, we were able to show that a measured ^{18}O isotope effect depends on deuterium labeling of substrate at the reactive (cleaved) hydrogen (80, 84). This behavior arises when the reactive oxidizing species is formed in a partially rate-determining and *reversible step* prior to C-H activation. The detection of such "double isotope effects" in O_2-dependent reactions has played an important part in demonstrating a role for metal-superoxo species in C-H activation reactions, overturning the long-held expectation that reactive, O_2-derived species would need to be more reduced than the level of superoxide in order to carry out their catalytic functions [e.g., the canonical ferryl species central to cytochrome P-450 chemistry (86)].

A description of our multidecade pursuit of O_2 chemistry in biology would not be complete without a short summary of the work by Justine Roth on the flavo-enzyme glucose oxidase. Justine broke new ground at many levels in a seemingly straightforward and well-understood enzyme system. Her work showed that glucose oxidase reduces O_2 via an outer-sphere electron tunneling mechanism that is dependent on the proximity of a protonated, active site histidine whose function is to reduce the size of the environmental reorganization barrier (87). Perhaps even more revealing, a subsequent study of ^{18}O isotope effects as a function of reaction driving force led to the unanticipated but powerful conclusion that the heavy atoms of O_2 must also behave quantum mechanically, undergoing tunneling in the process of preparing the reaction coordinate for electron transfer (88). Looking to the future, it will be fascinating to see the extent to which

the now well-accepted quantum effects in biological electron and hydrogen transfer will be extended to include the movement of larger masses (cf. 89). We have come a long way from the early assumptions that the phenomenon of tunneling would be unimportant under the warm conditions of biology.

BACK TO THE BEGINNING (OF QUINO-COFACTORS): UNRAVELING THE PATHWAY FOR PYRROLOQUINOLONE QUINONE PRODUCTION

As I write this article, I am approaching a targeted retirement date. One nagging and unfinished aspect of our research trajectory has been a complete description of the multistep biosynthetic pathway for production of the bacterial cofactor PQQ. Conserved prokaryotic operons for PQQ production have been identified in approximately 15% of aerobic bacteria, a number of which are opportunistic pathogens (90). These operons indicate a role for five (six in some cases) open reading frames with gene products of unique properties and mechanisms, and we have been able to draw on our years of experience of working with O_2 and C-H activation in unraveling the pathway. Nevertheless, there have been many surprises, including (*a*) the role for a radical SAM enzyme as the first catalyst in the modification of a ribosomally produced peptide (91–93) that (*b*) requires a previously unknown peptide chaperone (94, 95), and (*c*) a final catalyst in the pathway that carries out an eight-electron, eight-proton oxidation within a single enzyme active site (96–100). Once again, an amazing cohort of graduate students and postdocs have contributed to these findings (Olafur Magnusson, Steve Wecksler, Jordan RoseFigura, Florence Bonnot, John Latham, and Ian Barr) as well as numerous good-natured and talented collaborators with essential skills and interest in this tough project (Kimmen Sjolander, Carrie Wilmot, Steve Almo, and David Britt). Until recently, the central portion of the biosynthetic pathway seemed to present an intractable enigma. It is exciting to see the recent hard work of Eric Koehn and Ana Martins, who have respectively identified a novel, iron-dependent oxygenase (100a) and a zinc-dependent, two-component protease, closing the gap toward a full resolution of all of the steps in PQQ production. With few precedents in eukaryotes, it is possible that one or more of the identified reactions in the PQQ pathway will offer up new directions for future antibiotic development, an area of urgent need with the increasing development of antibiotic resistance.

ON BEING A WOMAN IN SCIENCE

When I began my studies and later research in science, I never expected to be a professor, let alone a professor at UC Berkeley. In fact, I never expected to be paid for my work. This may (and should) seem incomprehensible to most women scientists today, but in the 1960s, I was simply grateful to have the opportunity to follow my strong interests and curiosity and to have an acceptable outlet for creativity (that was distinct from my mother's pursuits as an artist)! Luck has also played an important role in my career. The women's movement emerged as a powerful force during the time of my early career, giving many of us a community and context from which to advocate for a footing equal to our male colleagues. The Fox Chase Cancer Research Institute was, in retrospect, a virtual hotbed for women activists, and during the 10 years I was there we advocated for and received equal pay and succeeded in overseeing the creation of a childcare center. Many of the mainstays of women in science grew out of activities at Fox Chase, including an early chapter of the Association for Women in Science (AWIS) organization and a training ground for future leaders in higher education (e.g., Shirley Tilghman, who would go on to become president of Princeton University). I was very fortunate during this time to also have associates at U Penn, including Phoebe Leboy, an early and informed voice for women's rights, and Mildred Cohen, a brilliant and strong scientific colleague who would go on to become a mentor and friend.

With this background of forward thinking and colleagueship in Philadelphia, I was quite shocked at the extent to which UC Berkeley was a bastion of male privilege! When I first arrived at the University of California, I was asked to join a group of senior women who had been fighting their own battles, which included climbing through the windows of the campus faculty club because they were welcome as wives but not as independent faculty members (hence, their strong support of a second club, the Women's Faculty Club). One of my main responses to the position I found myself in upon arriving at UC Berkeley in 1978 was to hunker down and focus on science and family. The main effect of this behavior was considerable isolation and loneliness. Once again, the times produced what I very much needed—a group of women peers who gathered together to hear and help each other in their professional and personal lives. Since 1980, I have gathered with them twice a month, and in the interim we have celebrated our successes, cried together over our losses, and watched each other grow old. A book has been written about our group, *Every Other Thursday*, published by Yale University Press in 2006 and authored by Ellen Daniell (101), one of the founding members. Since the book was published, many young women (and men) have thanked us for offering a blueprint for navigating the challenging waters of scientific careers.

Beginning in 1990, the National Academy of Sciences (NAS), which had also been a long-time male bastion, was beginning to elect an increasing number of women to its ranks. Some of the first women to be honored in this cohort were Christine Guthrie and Carol Gross (both members of our women's group), in the next year myself, and a few years after Mimi Koehl. Although I had barely heard of the NAS up to that point, within a short period of time half of the members of our women's group were members. My own election led to an outpouring of congratulations from friends and colleagues, a true highlight in my long journey through science, as well as an offer to move from the Department of Chemistry at UC Berkeley to the Massachusetts Institute of Technology (MIT). This was a wonderful opportunity that I took very seriously, though the process unearthed a fact that was painful to confront: When asked to submit my current salary at UC Berkeley to MIT, so that a formal offer could be completed, I learned that my salary was much too low, in fact at or near the bottom of the entire chemistry faculty. Here was an important issue I had neglected after my move to California, which was to ensure that I was being compensated in an appropriate and fair way.

In the end, after much soul-searching I made the decision to remain at UC Berkeley, going on to become the first woman chair of a major chemistry department in 2000. Although I only served as chair for three years, I am very proud of the progress we made during that period, including the creation of an undergraduate major in chemical biology and the hiring of eight outstanding new faculty members who span experimental physical chemistry, organic chemistry, and biochemistry. My main regret is that none of the faculty hired during my tenure were women, something I lament to this day. However, the gauntlet was successfully taken on by several of the subsequent chairs, one of whom, Michael Marletta, had been hired during my tenure.

As I continue to travel to scientific meetings, both in the United States and internationally, it is common for women to come to me and to talk about their fears and the difficulties they encounter. Though so much progress has been made here and abroad, many barriers still exist, not the least of which is how to manage family creation while receiving tenure in an academic department. I try not to be angry that it is still so hard, and to be grateful for the progress in evidence as I write this article.

NOTES TO MY FAMILY

I feel very fortunate that my life has led to an expansive family—with lines between scientific (**Figure 3**) and personal (**Figure 4**) family often blurred. Many of the students and postdocs who



Figure 3

Photograph of a laboratory reunion in Berkeley, on the occasion of my 70th birthday, April 16–17, 2011. My assistant, Mae Tulfo, 4th from the right in the first row (*yellow arrow*), contributed immeasurably to this enterprise over a long period of time that extended from the 1990s to her retirement in 2014.

have graced my laboratory are mentioned above, with apologies to those who are not specifically named. Perhaps the most important consequence of my willingness to take on the role of "first woman in physical sciences here at UC Berkeley" has been the chance to work with so many gifted, brilliant, and courageous young people.

At the beginning of graduate school, my entire focus was on training for a career in science. My early first marriage and relocation from NYC back to U Penn seemed consistent with this trajectory, but this would change quickly when I discovered I was pregnant during my second year of graduate school. I had read *The Good Earth* by Pearl Buck and thought if the women in



Figure 4

Receipt of the National Medal of Science in 2014 from President Barack Obama. Mordechai and I are flanked by our four children. Photograph courtesy of the National Science Foundation.

China can give birth and go right back to their work in the fields, I could (somehow) combine my graduate school training with being a young mother. This cavalier philosophy enabled me to work up to a week before my first son (Andrew) was born and to return to work shortly after. Two-and-a-half years after the birth of my first son, while pursuing postdoctoral studies at the Weizmann Institute, Andrew's brother Douglas was born in Israel, and once again, I returned quickly to work. Alas, from the perspective of being in my 70s, I ask myself, "What was I thinking?" The one major regret in my life is that I did not take enough time after my sons were born to enjoy the incredible sweetness and challenges of the early baby years. Of course, this is easier to say now than to accomplish in 1965–1967. There were no contingencies for women in graduate school, and the strong bias was that it was better not to train women as they would leave the work force as soon as they became pregnant! I had something to prove, but at a cost. In her book *Do Babies Matter? Gender and Family in the Ivory Tower* (102), Mary Ann Mason makes it clear how hard academic women with families must work and how often this does interfere with getting tenure. Published in 2013, this seminal work highlights the central importance of making science adaptable enough to accommodate women who choose to combine family formation with the pursuit of a high-powered research-focused career.

Had divorce not intervened in 1978, my family formation would have been complete, I would likely have lived the rest of my life in Philadelphia, and much of the narrative in this article would not have taken place. The actual trajectory of my life tells a different story, showing that second (and third) chances really do happen in life. In 1980, I met Mordechai Mitnick, a grassroots organizer who later established a much sought-after psychotherapy practice in Oakland. After a fair degree of struggle and uncertainty, he and I made the leap to raise four children together (Alexandra, Joshua, Andrew, and Douglas), each of whom has gone on to create meaningful lives and families of their own. This proliferation of family (currently eight grandchildren), at many times overwhelming, has been a bulwark supporting me throughout my 40 years of research and teaching at UC Berkeley. Some of my fondest memories from those early years include the lineup of lunch bags in the mornings, lovingly prepared by Mordechai for each of the kids and me, as well as the evenings around the kitchen table in which delicious vegetarian food was the backdrop for our family's recounting of the challenges, successes, and failures that we encountered each day. This "second-chance life" that Mordechai and I were able to stitch together has also been enriched by our long-term commitment to the integration of meditative practices into our daily lives and a shared perspective on politics, literature, and the arts.

LOOKING BACK, LOOKING FORWARD

From the vantage point of more than half a century since receiving my PhD, I am struck by how privileged the last 50 years have been for basic scientists. The end of World War II, combined with the spur to US science that came from the Soviet Union's launch of Sputnik, brought forth an extraordinary era of well-funded, curiosity-driven research. I am grateful to have benefited from this largesse and an unbroken stream of federal funding for my scientific explorations. The 21st century's growing emphasis on applied science and engineering is a natural outcome of growing societal needs linked to rapid climate change and loss of species diversification. However, my early support of curiosity-driven research, although cavalier and, in part, self-serving, has only grown stronger with the years: There really is no predicting where the next important shifts in understanding that will alter the course and well-being of planet Earth will occur. My own contributions to shifts in scientific paradigms, although acknowledged and awarded in my lifetime (103) (**Figure 4**), will likely be modified and reexamined by the next generation of scientists. But, for this to happen, there must be a societal commitment to continued scientific discovery. The

full participation of women in science is a vital part of such forward thinking. In this area, I am actually encouraged by the emergence of a cadre of self-confident and full-voiced women who are already changing both the face and content of science.

DISCLOSURE STATEMENT

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

ACKNOWLEDGMENTS

The scientific endeavors carried out in my laboratory were made possible by funding from the National Science Foundation and the National Institutes of Health.

LITERATURE CITED

1. Klinman NR. 1969. Antibody with homogeneous antigen binding produced by splenic foci in organ culture. *Immunochemistry* 6:757–59
2. Klinman JP, Rose IA. 1971. Mechanism of the aconitate isomerase reaction. *Biochemistry* 10:2259–66
3. Klinman JP, Rose IA. 1971. Stereochemistry of the interconversions of citrate and acetate catalyzed by citrate synthase, adenosine triphosphate citrate lyase, and citrate lyase. *Biochemistry* 10:2267–72
4. Klinman JP. 1972. The mechanism of enzyme-catalyzed reduced nicotinamide adenine dinucleotide-dependent reductions. Substituent and isotope effects in the yeast alcohol dehydrogenase reaction. *J. Biol. Chem.* 247:7977–87
5. Klinman JP. 1976. Isotope effects and structure-reactivity correlations in the yeast alcohol dehydrogenase reaction. A study of the enzyme-catalyzed oxidation of aromatic alcohols. *Biochemistry* 15:2018–26
6. Klinman JP, Humphries H, Voet JG. 1980. Deduction of kinetic mechanism in multisubstrate enzyme reactions from tritium isotope effects. *J. Biol. Chem.* 255:11648–51
7. Summers MC, Markovic R, Klinman JP. 1979. Stereochemistry and kinetic isotope effects in the bovine plasma amine oxidase catalyzed oxidation of dopamine. *Biochemistry* 18:1969–79
8. Miller SM, Klinman JP. 1985. Secondary isotope effects and structure-reactivity correlations in the dopamine β-monooxygenase reaction: evidence for a chemical mechanism. *Biochemistry* 24:2114–27
9. Ahn N, Klinman JP. 1983. Mechanism of modulation of dopamine β-monooxygenase by pH and fumarate as deduced from initial rate and primary isotope effect studies. *Biochemistry* 22:3096–106
10. Ahn NG, Klinman JP. 1987. Activation of dopamine β-monooxygenase by external and internal electron donors in resealed chromaffin granule ghosts. *J. Biol. Chem.* 262:1485–92
11. Palcic MM, Klinman JP. 1983. Isotopic probes yield microscopic constants: separation of binding energy from catalytic efficiency in the bovine plasma amine oxidase reaction. *Biochemistry* 22:5957–66
12. Klinman JP, Krueger M, Brenner M, Edmondson DE. 1984. Evidence for two copper atoms/subunit in dopamine β-monooxygenase catalysis. *J. Biol. Chem.* 259:3399–402
13. Brenner MC, Murray CJ, Klinman JP. 1989. Rapid-freeze and chemical-quench studies of dopamine β-monooxygenase: comparison of pre-steady-state and steady-state parameters. *Biochemistry* 28:4656–64
14. Prigge ST, Kolhekar AS, Eipper BA, Mains RE, Amzel LM. 1997. Amidation of bioactive peptides: the structure of peptidylglycine α-hydroxylating monooxygenase. *Science* 278:1300–5
15. Klinman JP. 2006. The copper-enzyme family of dopamine β-monooxygenase and peptidylglycine α-hydroxylating monooxygenase: resolving the chemical pathway for substrate hydroxylation. *J. Biol. Chem.* 281:3013–16
16. Zhu H, Sommerhalter M, Nguy AKL, Klinman JP. 2015. Solvent and temperature probes of the long-range electron-transfer step in tyramine β-monooxygenase: demonstration of a long-range proton-coupled electron-transfer mechanism. *J. Am. Chem. Soc.* 137:5720–29

17. Evans JP, Ahn K, Klinman JP. 2003. Evidence that dioxygen and substrate activation are tightly coupled in dopamine β-monooxygenase: implications for the reactive oxygen species. *J. Biol. Chem.* 278:49691–98
18. Duine JA, Jongejan JA. 1989. Pyrroloquinoline quinone: a novel cofactor. *Vitam. Horm.* 45:223–62
19. Blaschko H, Buffoni F. 1965. Pyridoxal phosphate as a constituent of histaminase (benzylamine oxidase) of pig plasma. *Proc. R. Soc. B* 163:45–60
20. Janes SM, Mu D, Wemmer D, Smith AJ, Kaur S, et al. 1990. A new redox cofactor in eukaryotic enzymes: 6-hydroxydopa at the active site of bovine serum amine oxidase. *Science* 248:981–87
21. Wang SX, Mure M, Medzihradszky KF, Burlingame AL, Brown DE, et al. 1996. A crosslinked cofactor in lysyl oxidase: redox function for amino acid side chains. *Science* 273:1078–84
22. Klinman JP, Bonnot F. 2014. Intrigues and intricacies of the biosynthetic pathways for the enzymatic quinocofactors: PQQ, TTQ, CTQ, TPQ, and LTQ. *Chem. Rev.* 114:4343–65
23. Cai D, Klinman JP. 1994. Evidence for a self-catalytic mechanism of 2,4,5-trihydroxyphenylalanine quinone biogenesis in yeast copper amine oxidase. *J. Biol. Chem.* 269:32039–42
24. Tang C, Klinman JP. 2001. The catalytic function of bovine lysyl oxidase in the absence of copper. *J. Biol. Chem.* 276:30575–78
25. DuBois JL, Klinman JP. 2005. Mechanism of post-translational quinone formation in copper amine oxidases and its relationship to the catalytic turnover. *Arch. Biochem. Biophys.* 433:255–65
26. Davidson VL, Wilmot CM. 2013. Posttranslational biosynthesis of the protein-derived cofactor tryptophan tryptophylquinone. *Annu. Rev. Biochem.* 82:531–50
27. Datta S, Mori Y, Takagi K, Kawaguchi K, Chen ZW, et al. 2001. Structure of a quinohemoprotein amine dehydrogenase with an uncommon redox cofactor and highly unusual crosslinking. *PNAS* 98:14268–73
28. Jalkanen S, Salmi M. 2001. Cell surface monoamine oxidases: enzymes in search of a function. *EMBO J.* 20:3893–901
29. Shen SH, Wertz DL, Klinman JP. 2012. Implication for functions of the ectopic adipocyte copper amine oxidase (AOC3) from purified enzyme and cell-based kinetic studies. *PLOS ONE* 7:e29270
30. Rucker R, Chowanadisai W, Nakano M. 2009. Potential physiological importance of pyrroloquinoline quinone. *Altern. Med. Rev.* 14:268–77
31. Akagawa M, Minematsu K, Shibata T, Kondo T, Ishii T, Uchida K. 2016. Identification of lactate dehydrogenase as a mammalian pyrroloquinoline quinone (PQQ)-binding protein. *Sci. Rep.* 6:26723
32. Bigeleisen JM, Goeppert-Mayer M. 1947. Calculation of equilibrium constants for isotopic exchange reactions. *J. Chem. Phys.* 15:261–67
33. Truhlar DG, Garrett BC, Hipes PG, Kuppermann A. 1984. Test of variational transition-state theory against accurate quantal results for a reaction with very large reaction-path curvature and a low barrier. *J. Chem. Phys.* 81:3542–45
34. Thiemens MH. 1999. Mass-independent isotope effects in planetary atmospheres and the early solar system. *Science* 283:341–45
35. Hermes JD, Cleland WW. 1984. Evidence from multiple isotope effect determinations for coupled hydrogen motion and tunneling in the reaction catalyzed by glucose-6-phosphate-dehydrogenase. *J. Am. Chem. Soc.* 106:7263–64
36. Welsh KM, Creighton DJ, Klinman JP. 1980. Transition-state structure in the yeast alcohol dehydrogenase reaction: the magnitude of solvent and α-secondary hydrogen isotope effects. *Biochemistry* 19:2005–16
37. Douzou P. 1982. Developments in low-temperature biochemistry and biology. *Proc. R. Soc. B* 217:1–28
38. Cha Y, Murray CJ, Klinman JP. 1989. Hydrogen tunneling in enzyme reactions. *Science* 243:1325–30
39. Grant KL, Klinman JP. 1989. Evidence that both protium and deuterium undergo significant tunneling in the reaction catalyzed by bovine serum amine oxidase. *Biochemistry* 28:6597–605
40. Jonsson T, Edmondson DE, Klinman JP. 1994. Hydrogen tunneling in the flavoenzyme monoamine oxidase B. *Biochemistry* 33:14871–78
41. Jonsson T, Glickman MH, Sun SJ, Klinman JP. 1996. Experimental evidence for extensive tunneling of hydrogen in the lipoxygenase reaction: implications for enzyme catalysis. *J. Am. Chem. Soc.* 118:10319–20

42. Bahnson BJ, Colby TD, Chin JK, Goldstein BM, Klinman JP. 1997. A link between protein structure and enzyme catalyzed hydrogen tunneling. *PNAS* 94:12797–802
43. Saunders WHJ. 1985. Calculations of isotope effects in elimination reactions. New experimental criteria for tunneling in slow proton transfers. *J. Am. Chem. Soc.* 107:164–69
44. Glickman MH, Wiseman JS, Klinman JP. 1994. Extremely large isotope effects in the soybean lipoxygenase-linoleic acid reaction. *J. Am. Chem. Soc.* 116:793–94
45. Glickman M, Klinman JP. 1995. Nature of rate-limiting steps in the soybean lipoxygenase-1 reaction. *Biochemistry* 34:14077–92
46. Dogonadze RR, Kuznetsov AM, Levich VG. 1968. Theory of hydrogen-ion discharge on metals: case of high overvoltages. *Electrochim. Acta* 13:1025–44
47. Hu S, Sharma SC, Scouras AD, Soudackov AV, Carr CA, et al. 2014. Extremely elevated room-temperature kinetic isotope effects quantify the critical role of barrier width in enzymatic C–H activation. *J. Am. Chem. Soc.* 136:8157–60
48. Hu S, Soudackov AV, Hammes-Schiffer S, Klinman JP. 2017. Enhanced rigidification within a double mutant of soybean lipoxygenase provides experimental support for vibronically nonadiabatic proton-coupled electron transfer models. *ACS Catal.* 7:3569–74
49. Horitani M, Offenbacher AR, Carr CA, Yu T, Hoeke V, et al. 2017. ^{13}C ENDOR spectroscopy of lipoxygenase–substrate complexes reveals the structural basis for C–H activation by tunneling. *J. Am. Chem. Soc.* 139:1984–97
50. Westheimer FH. 1961. The magnitude of the primary kinetic isotope effect for components of hydrogen and deuterium. *Chem. Rev.* 61:265–73
51. Knapp MJ, Rickert K, Klinman JP. 2002. Temperature-dependent isotope effects in soybean lipoxygenase-1: Correlating hydrogen tunneling with protein dynamics. *J. Am. Chem. Soc.* 124:3865–74
52. Klinman JP, Offenbacher AR, Hu S. 2017. Origins of enzyme catalysis: experimental findings for C–H activation, new models, and their relevance to prevailing theoretical constructs. *J. Am. Chem. Soc.* 139:18409–27
53. Offenbacher AR, Hu S, Poss EM, Carr CAM, Scouras AD, et al. 2017. Hydrogen–deuterium exchange of lipoxygenase uncovers a relationship between distal, solvent exposed protein motions and the thermal activation barrier for catalytic proton-coupled electron tunneling. *ACS Cent. Sci.* 3:570–79
54. Guagliardi A, Martino M, Iaccarino I, DeRosa M, Rossi M, Bartolucci S. 1996. Purification and characterization of the alcohol dehydrogenase from a novel strain of *Bacillus stearothermophilus* growing at 70°C. *Int. J. Biochem. Cell Biol.* 28:239–46
55. Kohen A, Cannio R, Bartolucci S, Klinman JP. 1999. Enzyme dynamics and hydrogen tunnelling in a thermophilic alcohol dehydrogenase. *Nature* 399:496–99
55a. Klinman JP, Offenbacher AR. 2018. Understanding biological hydrogen transfer through the lens of temperature dependent kinetic isotope effects. *Acc. Chem. Res.* 51:1966–74
56. Klinman JP. 2009. An integrated model for enzyme catalysis emerges from studies of hydrogen tunneling. *Chem. Phys. Lett.* 471:179–93
57. Nagel ZD, Klinman JP. 2009. A 21st century revisionist's view at a turning point in enzymology. *Nat. Chem. Biol.* 5:543–50
58. Klinman JP, Kohen A. 2013. Hydrogen tunneling links protein dynamics to enzyme catalysis. *Annu. Rev. Biochem.* 82:471–96
59. Liang Z-X, Lee T, Resing KA, Ahn NG, Klinman JP. 2004. Thermal-activated protein mobility and its correlation with catalysis in thermophilic alcohol dehydrogenase. *PNAS* 101:9556–61
60. Liang Z-X, Tsigos I, Bouriotis V, Klinman JP. 2004. Impact of protein flexibility on hydride-transfer parameters in thermophilic and psychrophilic alcohol dehydrogenases. *J. Am. Chem. Soc.* 126:9500–1
61. Nagel ZD, Dong M, Bahnson BJ, Klinman JP. 2011. Impaired protein conformational landscapes as revealed in anomalous Arrhenius prefactors. *PNAS* 108:10520–25
62. Nagel ZD, Meadows CW, Dong M, Bahnson BJ, Klinman JP. 2012. Active site hydrophobic residues impact hydrogen tunneling differently in a thermophilic alcohol dehydrogenase at optimal versus nonoptimal temperatures. *Biochemistry* 51:4147–56
63. Nagel ZD, Cun S, Klinman JP. 2013. Identification of a long-range protein network that modulates active site dynamics in extremophilic alcohol dehydrogenases. *J. Biol. Chem.* 288:14087–97

64. Meadows CW, Ou R, Klinman JP. 2014. Picosecond-resolved fluorescent probes at functionally distinct tryptophans within a thermophilic alcohol dehydrogenase: relationship of temperature-dependent changes in fluorescence to catalysis. *J. Phys. Chem. B* 118:6049–61
65. Meadows CW, Balakrishnan G, Kier BL, Spiro TG, Klinman JP. 2015. Temperature-jump fluorescence provides evidence for fully reversible microsecond dynamics in a thermophilic alcohol dehydrogenase. *J. Am. Chem. Soc.* 137:10060–63
66. Vaughn MB, Zhang J, Spiro TG, Dyer RB, Klinman JP. 2018. Activity-related microsecond dynamics revealed by temperature-jump Förster resonance energy transfer measurements on thermophilic alcohol dehydrogenase. *J. Am. Chem. Soc.* 140:900–3
67. Zhang J, Klinman JP. 2011. Enzymatic methyl transfer: role of an active site residue in generating active site compaction that correlates with catalytic efficiency. *J. Am. Chem. Soc.* 133:17134–37
68. Zhang J, Kulik HJ, Martinez TJ, Klinman JP. 2015. Mediation of donor-acceptor distance in an enzymatic methyl transfer reaction. *PNAS* 112:7954–59
69. Zhang J, Klinman JP. 2016. Convergent mechanistic features between the structurally diverse *N*- and *O*-methyltransferases, glycine *N*-methyltransferase and catechol *O*-methyltransferase. *J. Am. Chem. Soc.* 138:9158–65
70. Goto Y, Klinman JP. 2002. Binding of dioxygen to non-metal sites in proteins: exploration of the importance of binding site size versus hydrophobicity in the copper amine oxidase from *Hansenula polymorpha*. *Biochemistry* 41:13637–43
71. Mure M, Mills SA, Klinman JP. 2002. Catalytic mechanism of the topa quinone containing copper amine oxidases. *Biochemistry* 41:9269–78
72. Knapp MJ, Klinman JP. 2003. Kinetic studies of oxygen reactivity in soybean lipoxygenase-1. *Biochemistry* 42:11466–75
73. Johnson BJ, Cohen J, Welford RW, Pearson AR, Schulten K, et al. 2007. Exploring molecular oxygen pathways in *Hansenula polymorpha* copper-containing amine oxidase. *J. Biol. Chem.* 282:17767–76
74. Collazo L, Klinman JP. 2016. Control of the position of oxygen delivery in soybean lipoxygenase-1 by amino acid side chains within a gas migration channel. *J. Biol. Chem.* 291:9052–59
75. Kalms J, Schmidt A, Frielingsdorf S, Utesch T, Gotthard G, et al. 2018. Tracking the route of molecular oxygen in O_2-tolerant membrane-bound [NiFe] hydrogenase. *PNAS* 115:E2229–37
76. Hwang CC, Grissom CB. 1994. Unusually large deuterium isotope effects in soybean lipoxygenase are not caused by a magnetic isotope effect. *J. Am. Chem. Soc.* 116:795–96
77. Glickman MH, Cliff S, Thiemens M, Klinman JP. 1997. Comparative study of ^{17}O and ^{18}O isotope effects as a probe for dioxygen activation: application to the soybean lipoxygenase reaction. *J. Am. Chem. Soc.* 119:11357–61
78. Guy RD, Fogel ML, Berry JA. 1993. Photosynthetic fractionation of the stable isotopes of oxygen and carbon. *Plant Physiol.* 101:37–47
79. Tian G, Klinman JP. 1993. Discrimination between ^{16}O and ^{18}O in oxygen binding to the reversible oxygen carriers hemoglobin, myoglobin, hemerythrin, and hemocyanin: a new probe for oxygen binding and reductive activation by proteins. *J. Am. Chem. Soc.* 115:8891–97
80. Tian G, Berry JA, Klinman JP. 1994. Oxygen-18 kinetic isotope effects in the dopamine β-monooxygenase reaction: evidence for a new chemical mechanism in non-heme metallomonooxygenases. *Biochemistry* 33:226–34
81. Purdy MM, Koo LS, Ortiz de Montellano PR, Klinman JP. 2006. Mechanism of O_2 activation by cytochrome P450cam studied by isotope effects and transient state kinetics. *Biochemistry* 45:15793–806
82. Stahl SS, Francisco WA, Merkx M, Klinman JP, Lippard SJ. 2001. Oxygen kinetic isotope effects in soluble methane monooxygenase. *J. Biol. Chem.* 276:4549–53
83. Mirica LM, McCusker KP, Munos JW, Liu HW, Klinman JP. 2008. ^{18}O kinetic isotope effects in non-heme iron enzymes: probing the nature of Fe/O_2 intermediates. *J. Am. Chem. Soc.* 130:8122–23
84. Zhu H, Peck SC, Bonnot F, van der Donk WA, Klinman JP. 2015. Oxygen-18 kinetic isotope effects of nonheme iron enzymes HEPD and MPnS support iron(III) superoxide as the hydrogen abstraction species. *J. Am. Chem. Soc.* 137:10448–51

85. Hermes JD, Roeske CA, O'Leary MH, Cleland WW. 1982. Use of multiple isotope effects to determine enzyme mechanisms and intrinsic isotope effects. Malic enzyme and glucose 6-phosphate dehydrogenase. *Biochemistry* 21:5106–14
86. Groves JT. 2006. High-valent iron in chemical and biological oxidations. *J. Inorg. Biochem.* 100:434–47
87. Roth JP, Klinman JP. 2003. Catalysis of electron transfer during activation of O_2 by the flavoprotein glucose oxidase. *PNAS* 100:62–67
88. Roth JP, Wincek R, Nodet G, Edmondson DE, McIntire WS, Klinman JP. 2004. Oxygen isotope effects on electron transfer to O_2 probed using chemically modified flavins bound to glucose oxidase. *J. Am. Chem. Soc.* 126:15120–31
89. Meyers MP, Klinman JP. 2011. Investigating inner-sphere reorganization via secondary kinetic isotope effects in the C-H cleavage reaction catalyzed by soybean lipoxygenase: tunneling in the substrate backbone as well as the transferred hydrogen. *J. Am. Chem. Soc.* 133:430–39
90. Shen YQ, Bonnot F, Imsand EM, RoseFigura JM, Sjolander K, Klinman JP. 2012. Distribution and properties of the genes encoding the biosynthesis of the bacterial cofactor, pyrroloquinoline quinone. *Biochemistry* 51:2265–75
91. Wecksler SR, Stoll S, Iavarone AT, Imsand EM, Tran H, et al. 2010. Interaction of PqqE and PqqD in the pyrroloquinoline quinone (PQQ) biosynthetic pathway links PqqD to the radical SAM superfamily. *Chem. Commun.* 46:7031–33
92. Barr I, Latham JA, Iavarone AT, Chantarojsiri T, Hwang JD, Klinman JP. 2016. Demonstration that the radical *S*-adenosylmethionine (SAM) enzyme PqqE catalyzes de novo carbon-carbon cross-linking within a peptide substrate PqqA in the presence of the peptide chaperone PqqD. *J. Biol. Chem.* 291:8877–84
93. Latham JA, Barr I, Klinman JP. 2017. At the confluence of ribosomally synthesized peptide modification and radical *S*-adenosylmethionine (SAM) enzymology. *J. Biol. Chem.* 292:16397–405
94. Latham JA, Iavarone AT, Barr I, Juthani PV, Klinman JP. 2015. PqqD is a novel peptide chaperone that forms a ternary complex with the radical *S*-adenosylmethionine protein PqqE in the pyrroloquinoline quinone biosynthetic pathway. *J. Biol. Chem.* 290:12908–18
95. Evans RL, Latham JA, Xia YL, Klinman JP, Wilmot CM. 2017. Nuclear magnetic resonance structure and binding studies of PqqD, a chaperone required in the biosynthesis of the bacterial dehydrogenase cofactor pyrroloquinoline quinone. *Biochemistry* 56:2735–46
96. Magnusson OT, Toyama H, Saeki M, Schwarzenbacher R, Klinman JP. 2004. The structure of a biosynthetic intermediate of pyrroloquinoline quinone (PQQ) and elucidation of the final step of PQQ biosynthesis. *J. Am. Chem. Soc.* 126:5342–43
97. Magnusson OT, Toyama H, Saeki M, Rojas A, Reed JC, et al. 2004. Quinone biogenesis: structure and mechanism of PqqC, the final catalyst in the production of pyrroloquinoline quinone. *PNAS* 101:7913–18
98. Magnusson OT, RoseFigura JM, Toyama H, Schwarzenbacher R, Klinman JP. 2007. Pyrroloquinoline quinone biogenesis: characterization of PqqC and its H84N and H84A active site variants. *Biochemistry* 46:7174–86
99. RoseFigura JM, Puehringer S, Schwarzenbacher R, Toyama H, Klinman JP. 2011. Characterization of a protein-generated O_2 binding pocket in PqqC, a cofactorless oxidase catalyzing the final step in PQQ production. *Biochemistry* 50:1556–66
100. Bonnot F, Iavarone AT, Klinman JP. 2013. Multistep, eight-electron oxidation catalyzed by the cofactorless oxidase, PqqC: identification of chemical intermediates and their dependence on molecular oxygen. *Biochemistry* 52:4667–75
100a. Koehn EM, Latham JA, Armand T, Evans RL III, Tu X, et al. 2019. Discovery of hydroxylase activity for PqqB provides a missing link in the pyrroloquinoline quinone biosynthetic pathway. *J. Am. Chem. Soc.* 141:4398–4405
101. Daniell E. 2006. *Every Other Thursday: Stories and Strategies from Successful Women Scientists*. New Haven, CT: Yale Univ. Press
102. Mason MA, Wolfinger NH, Goulden M. 2013. *Do Babies Matter? Gender and Family in the Ivory Tower*. New Brunswick, NJ: Rutgers Univ. Press
103. Wikipedia. 2018. Judith Klinman. *Wikipedi*a. **https://en.wikipedia.org/wiki/Judith_Klinman**