NO TRUTH EXCEPT IN THE DETAILS

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MARTIN KLEIN

NO TRUTH EXCEPT IN THE DETAILS

Essays in Honor of Martin J. Klein

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TABLE OF CONTENTS

DANIEL SIEGEL and A.J. KOX / Introduction	ix
ASGER ABOE / Martin Klein at Yale	ii

I. FOUNDATIONS OF PHYSICS, EXPERIMENTAL AND MATHEMATICAL

RUSSELL McCORMMACH / The Last Experiment of Henry Cavendish	1
ELIZABETH GARBER / Reading Mathematics, Constructing Physics: Fourier and His Readers, 1822–1850	31

II. ELECTRICITY AND MAGNETISM

OLE KNUDSEN / Electromagnetic Energy and the Early History of the Energy Principle	55
PETER M. HARMAN / Through the Looking-Glass, and What Maxwell Found There	79
ERWIN N. HIEBERT / Electric Discharge in Rarefied Gases: The Dominion of Experiment. Faraday. Plücker. Hittorf.	95

III. THERMODYNAMICS AND MATTER THEORY, PHYSICAL AND BIOLOGICAL

ROBERT J. DELTETE / Gibbs and the Energeticists	135
JED Z. BUCHWALD / Heinrich Hertz's Attempt to Generate a Novel Account of Evaporation	171
FREDERIC L. HOLMES / Crystals and Carriers: The Chemical and Physiological Identification of Hemoglobin	191

IV. EINSTEIN

A. J. KOX / Einstein, Specific Heats, and Residual Rays: The History of a Retracted Paper	245
ROBERT SCHULMANN / From Periphery to Center: Einstein's Path from Bern to Berlin (1902–1914)	259
GERALD HOLTON / Einstein and Books	273

V. FURTHER PERSPECTIVES

DANIEL M. SIEGEL / Text and Context in Maxwell's Electromagnetic Theory	281
STEPHEN G. BRUSH / Prediction and Theory Evaluation in Physics and Astronomy	299
ABRAHAM PAIS / The Power of the Word	319
ROGER H. STUEWER / The Seventh Solvay Conference: Nuclear Physics at the Crossroads	333
Appendix. List of Publications of Martin J. Klein	363
Index of Names	369

INTRODUCTION

Plus de détails, plus de détails, disait-il à son fils, il n'y a d'originalité et de vérité que dans les détails . . .

Stendhal, Lucien Leuwen

I

This book of essays in honor of Martin Klein represents an effort on the part of his students and colleagues to pay tribute to a lifetime of dedication to physics and the history of physics. The papers contained in this volume mirror Martin Klein's contributions to the history of physics, reflecting his influence as scholar, teacher, and colleague. The title of this book stems from a quotation used by Martin as an epigraph in one of his papers. It characterizes Martin's work and the aspirations of all of us in its call for meticulous attention to historical detail.

Of the themes of this book, more later; it behooves us, at the outset, to try to give some representation of the career to which this tribute refers. This effort labors under a major adversity: A significant portion of the impact of Martin Klein's work comes from the sheer pleasure of reading it, so that any representation of it by another hand necessarily falls short. The pleasure in the reading flows, in part, from the skill with which the words and equations are deployed, so as to convey the message both clearly and vividly. Standing behind this skill, however, controlling and commanding it, is something more basic and more important: an attitude of respect toward both the subject and the reader.

The subject, in Martin Klein's historical writing, is multiple: it is the science; it is the scientist; it is the history. The science is respected through the meticulous care with which it is treated; it is treated not as some epiphenomenon of social history or mere impediment to the smooth flow of historical narrative, but rather as a thing of beauty and significance, important in its own right. Similarly, the scientist is not treated as mere feed for the historian's mill, but rather as a person, with whom the historian interacts at a distance, but who must still

be respected as a person. Respect for persons is integral, embracing the direct experience of present individuals as well as the vicarious experience of historical figures. The third subject, the history, is treated with respect as well, through scrupulous honesty, careful construction of narrative, and avoidance of ideological slant.

Beyond this, the reader's situation is kept in mind, to is needs are perceived and attended to. Definitions of words, concepts, symbols are always given. Issues that will be puzzling to the reader, questions that will arise, are anticipated and dealt with; what this requires on the part of the writer is sensitivity and insight into the situation of the reader, as well as a commitment to minister to his needs. In a word, it is respect for the reader, as a person, as a fellow intellect, as a fellow student of the history of science, that drives this process. Beyond all of this, however – to return to the theme with which we began – it is the supreme skill and artistry with which Martin Klein's work is presented that provides the vehicle through which his positive sentiments toward his subjects and his readers are realized in historical narratives that give the highest kind of pleasure and insight.

Let us begin at the beginning, with Martin Klein's work in physics. This work was in the area of statistical mechanics, and in this field he made important theoretical contributions pertaining to the magnetization of thin films. Taking an interest also in questions of the foundations of statistical mechanics and the work of Paul Ehrenfest on this subject, Klein's concern then began to turn toward the history of Ehrenfest's contributions and their broader context in both the history of statistical mechanics and Ehrenfest's own life history. This phase of Klein's career resulted in two important books: the definitive edition of Ehrenfest's collected scientific papers; and a biographically framed study of Ehrenfest and the history of statistical mechanics – including quantum statistical mechanics – that constituted a milestone contribution both to the broader history and to our understanding of Ehrenfest himself.

Martin Klein's work in the history of statistical mechanics then branched out in two directions. First, in the direction of classical thermodynamics and statistical mechanics, as exemplified in the work especially of Rudolf Clausius, James Clerk Maxwell, and Josiah Willard Gibbs. Threading through Klein's work in this area and unifying it is the question of the nature and foundation of the second law of thermodynamics; this work promises to stand as definitive on the history of the second law in the second half of the nineteenth century. To single

out just one aspect, Klein's account of the role of Maxwell's demon in his reasoning on this subject has shed clear light on one of the most perplexed issues in the history and foundations of thermodynamics and statistical mechanics.

The other branch in Klein's work led on to the history of quantum statistical mechanics and quantum theory in general. His studies of the origin of the quantum hypothesis in the work of Max Planck were pioneering, showing that analysis in depth can shed light on even the most singular and puzzling events in the history of science; strong continuity with Klein's other work was maintained, as Planck's thinking on the foundations of the second law played a crucial role in his framing of the quantum hypothesis. Most important for Klein's continuing work on the history of the quantum theory, however, was his growing interest in Albert Einstein's contributions in that area. It was Einstein, as Klein has shown, who clarified the nature of Planck's quantum hypothesis, applied it to light itself, and first saw through to the depths of the changes of our view of physical reality that would be required to come to grips with the conundrums generated by quantum theory, especially the wave-particle duality. Martin Klein's intense involvement with the legacy of Albert Einstein has continued in his Senior Editorship of *The Collected Papers of Albert Einstein*, where his direction of that major enterprise is ensuring a product worthy of its subject.

Martin Klein's contributions to the history of physics and the history of science are attributable no less to his personal and institutional activities than to his writing. As Professor of Physics at Case Institute of Technology to 1967, and as Professor of the History of Physics at Yale University thereafter (becoming Eugene Higgins Professor in 1974 and Bass Professor in 1991), Klein has trained a generation of students to go forth and practice the discipline with the clarity, integrity, and skill that characterize his own work. Beyond this, owing to the respect in which he is held in both the physics and the history of science communities, Martin Klein has been an effective facilitator of positive interaction between those communities in the history of physics enterprise. Perhaps the greatest problem of the modern university – and the greatest problem of the intellectual enterprise that is housed in the university – is the fractionalization of learning, the breaking down into departmental and disciplinary feudal domains. Martin Klein is one of those few broad and gifted individuals who is able to bridge the disciplinary gap, producing scholarship that will travel well across disciplinary boundary

lines and convince those on each side that the other side has something of value to offer. Especially in bringing history of physics to the physics community, Klein has been an indefatigable ambassador. In numerous lectures and articles directed especially toward physicists, Klein has presented the history of physics as a discipline for which the physicist can have respect. As Chair of the Division of the History of Physics of the American Physical Society, in two separate tours of duty, Klein has done much to further the relationship between the history of physics discipline and the physics community. In a long series of distinguished lectureships, including the George Sarton Memorial Lecture to the American Association for the Advancement of Science, the Morris Loeb Lectures at Harvard, and others, Klein has brought his message to a broad audience of scientists, science teachers, and a variety of other constituencies. Not least in his work with the Einstein Papers, Klein has made, and is making, a major contribution to the cooperation of these communities. Serving him in this endeavor have been the respect in which his scholarly contributions are held, as well as the respect in which he is held as a person.

The honors that have come Martin's way reflect the catholicity of his intellectual approach and activity: He is a Fellow of the American Physical Society as well as a full member of the Académie Internationale d'Histoire des Sciences and of the American Academy of Arts and Sciences; his election to the National Academy of Science in 1977 was noteworthy as designating one of a very small number of memberships allocated to individuals who are not primarily scientists. Martin's activity in bringing communities together is manifested also in international ties. There has been a strong connection with the Netherlands, first in his studies of Ehrenfest, which involved his working in Leiden (where Ehrenfest had succeeded H. A. Lorentz), and then through his appointments at the University of Amsterdam, first as Van der Waals Visiting Professor and later as the first Pieter Zeeman Visiting Professor. Martin Klein thus has been, throughout, a unifier of knowledge and of the disciplinary and national communities that generate knowledge. Honors, acknowledgment, and respect for his work have in turn come from many quarters and in profusion.

II

Martin Klein's work in the history of physics has been primarily focused on the late nineteenth and early twentieth centuries, but his influence, through students and colleagues, has touched on the history of physics and related disciplines since Newton – that is, from the seventeenth through the twentieth centuries. The present volume, in like manner, has its primary focus in the late nineteenth and early twentieth centuries, with extensions both earlier and later.

Part I presents a pair of studies dealing respectively with the mathematical and experimental foundations of the physics discipline, as they developed in the late eighteenth and early nineteenth centuries. Elizabeth Garber begins with the French mathematical tradition, embodied in the rational mechanics of the eighteenth century and its generalization in the early nineteenth century, as exemplified especially in work of Joseph Fourier; she then goes on to discuss the transformation of this tradition, especially by the British followers of Fourier and his Continental colleagues, into what may be properly designated mathematical physics. Garber's paper not only advances our understanding of the emergence of a flourishing physics discipline in the course of the nineteenth century, her essay also furnishes background for an understanding of continuing issues, through the nineteenth and twentieth centuries, of the demarcation of physics from mathematics and of the interaction of the two at their borderline. The themes broached in Garber's paper continue into Part II of this volume, in which the work of William Thomson and James Clerk Maxwell on the mathematization of electromagnetic theory are discussed.

The companion paper to Garber's in Part I is by Russell McCormach, on Henry Cavendish's weighing of the earth. The tradition of precision experiment is the other leg on which the developing nineteenth-century physics discipline stood, and no better example of the roots of this than Cavendish's experiment may be found. In recent decades, among historians of science, experiment has been given short shrift: either experiment has been regarded as unproblematic, and hence unworthy of probing historical analysis; or experiment has been deemed to be so theory-laden, so derivative from theory, as to have no independent significance worthy of study. Most recently, however, there has been a dawning realization among historians that the malleability of experiment – the tendency of its results to be influenced by theoretical expectations –

is accompanied also by a certain recalcitrance of experiment, a tendency to resist manipulation and confound theoretical expectation in certain instances – as working scientists have known all along. Questions of the independence of experiment and the interaction of theory and experiment are important in several of the subsequent articles, especially those by Hiebert, Buchwald, Holmes, Kox, and Brush. McCormmach's article also provides acute insight into aspects of Cavendish's personal life and how it interacted with his science – a theme that continues most conspicuously in Part V of this volume, dealing with Albert Einstein.

Following the discussion, in Part I, of the eighteenth- and early nineteenth-century foundations of the physics discipline, the following two sections of the volume then go on to consider the blossoming of the physics discipline in the nineteenth century, on the basis of those mathematical and experimental foundations. Parts II and III consider respectively the two broad areas of investigation that constituted the central foci of the development of the physics discipline in the nineteenth century: electricity and magnetism; and thermodynamics and matter theory. To separate these two areas is in fact a bit artificial, as there were very important interactions between them, both conceptually and with respect to individual scientists who participated in both. In fact, most of the central figures to be treated in Parts II and III, including Hermann von Helmholtz, William Thomson, James Clerk Maxwell, Heinrich Hertz, and J. Willard Gibbs, made central contributions in both areas.

The first article in Part II, by Ole Knudsen, shows the depth of the connections between energy physics and electromagnetic theory from the outset, that is, from the years around 1850, when the energy law was being formulated. William Thomson started out in electromagnetic theory as described by Elizabeth Garber in her article, namely, as a follower of Fourier's mathematics and methodology. However, as has been shown in recent work, especially by Knudsen himself and by Norton Wise, energy concerns very soon began to play a central role in Thomson's thinking about the electromagnetic field. Knudsen here argues, in particular, that the concept of potential energy in electromagnetic theory was won with much more difficulty than has been realized hitherto, as shown by the fact that both Helmholtz and Maxwell made fundamental errors in their treatments of the relationship between electromagnetic induction and the potential energies of magnetically interacting circuits. It was only Thomson who was able to get this straight, and that, Knudsen

argues, was owing to his familiarity with the concept of internal energy from his work with that concept in connection with the energy law and thermodynamics.

Following on directly from Thomson, both in the history of electromagnetic theory and in the current volume, is the work of Maxwell. It was Maxwell who established, in enduring form, the relationship between electromagnetism and optics, in what emerged as the electromagnetic theory of light. Peter Harman, in his article, presents a synthetic overview of the development of Maxwell's thinking on this subject, drawing upon his own previous work in this area as well as that of his colleagues in the Maxwell industry; added to this in Harman's article is the particular insight he brings on the basis of his expert knowledge of archival Maxwell materials, as brought to light in the course of his continuing work on a three-volume edition of Maxwell's scientific letters and papers.

Last in Part II, and bringing the story up toward the end of the nineteenth century, is Erwin Hiebert's article on the history of electrical discharge in rarefied gases, from Faraday onward, to the period just before J. J. Thomson's work in that area, leading to the discovery of the electron. Taken in conjunction with the previous two articles, Hiebert's article completes the treatment, in the present volume, of the Faraday–Thomson–Maxwell triumvirate in the history of field theory. The article itself, however, is rather more concerned with the experimental aspect of the work of Faraday and his successors in this field of research, and in this sense follows on most directly from McCormmach's article in Part I. Hiebert argues for the primacy of experiment in directing research with electrical discharge tubes in the course of the nineteenth century. Those carrying out the research admitted that they knew little about the physical processes taking place inside the tube, and they did not undertake experiments in order to confirm or refute fundamental theories of the phenomenon. Instead, they relied on largely unarticulated feelings, hunches, and low-level generalizations concerning the observed phenomena to direct their attempts to manipulate situations and generate new, and perhaps more revealing phenomena. Technical advances in dealing with the tubes – as in sealing electrodes, pumping to high vacuum, introducing various gases, shaping the tubes, and introducing various objects into the tubes – had more to do with the development of the research than any theory. Part II thus ends on a note of considerable current historiographical interest.

Part III, dealing with thermodynamics and matter theory, parallels Part II chronologically in dealing with nineteenth-century developments, and, as we have seen, there was in fact much interaction between thermodynamics and electricity and magnetism during that period. It is in Part III that we begin to move directly into the part of the history of physics that Martin Klein has made his own. In his article on “Gibbs and the Energeticists,” Robert Deltete employs a historical gambit that is familiar to readers of Klein’s work: using what one historical figure or set of figures had to say about another figure or set of figures, in order to illuminate the world views and presuppositions of both. In illuminating the energeticist movement and its views concerning the history, nature, and significance of thermodynamics, Deltete’s work is important for our understanding both of the beginnings of the energy law and thermodynamics – especially in the work of Robert Mayer, seen by the energeticists as the founder of their movement – and of the role of thermodynamics in the decline of the mechanical worldview toward the end of the nineteenth century. The philosophical orientation implied in the energeticist movement, especially in the form developed and transmitted by Ernst Mach, was to be of great importance for Einstein, whose work is discussed in Part IV below.

Jed Buchwald’s paper on Heinrich Hertz deals with a scientist perhaps best known for his experimental work in electricity and magnetism, namely, in the generation and detection of electromagnetic waves. It is Hertz’s experimental work relevant to issues in thermodynamics and kinetic theory, however, that Buchwald explores here. In thematic content, Buchwald’s article continues the emphasis on the integrity of the experimental tradition that was broached in McCormmach’s article on Cavendish and further developed in Hiebert’s article on gas discharge studies. Buchwald sees Hertz’s experiments on electromagnetic waves and his experiments on evaporation as both stemming from the same scientific impulse, the same methodology: the attempt to produce new phenomena in the laboratory, using hints and suggestions stemming in part directly from experience in the laboratory and partly from theoretical ideas. Even when there was theoretical input, however, the primary and overriding object was not to verify or falsify the theory, but rather to use the theory for what it was worth in trying to produce new phenomena. Hertz’s work on evaporation was hardly as successful as the work on electromagnetic waves, but that circumstance is perhaps all for the best in helping to illuminate Hertz’s experimental methodology.

Last in Part III is an article by Frederic L. Holmes, which begins with a problem posed by Antoine Lavoisier in the later eighteenth century, relating to the site of oxidation reactions in the body and the attendant production of heat; the story ends with the answer furnished by the work of Felix Seyler-Hoppe in the 1860s and 1870s, in terms of the transport of oxygen through the bloodstream by hemoglobin, to support oxidation in the individual cells of the body tissues. From Lavoisier's work onward, and especially as manifested in his collaboration with Pierre Simon de Laplace on the subject, the question of animal heat was closely bound up with the physical sciences, and this matter was central for both Mayer and Helmholtz in their pioneering work on the conservation of energy. More directly relevant thematically for the present volume, however, is Holmes's treatment of the interaction between theory and experiment. On the one hand, Holmes shows how the posing of the question of oxidation in the body influenced both the program of experimentation that was undertaken and the conceptualization of the results that were obtained; this tends to diminish the independence of experiment. On the other hand, Holmes's account of the influence of developments in experimental technique on the experimental outcomes and their interpretation, and his detailing of how these in turn brought about changes in the phrasing of the questions that were asked of experiment, tends to enhance the element of independence in the role of experiment. It is, above all, a balanced account of the interaction of theory and experiment that Holmes presents.

Moving on to the early twentieth century is Part IV, on Einstein, a topic central to Klein's oeuvre, which has become even more central since he assumed the Senior Editorship of the Einstein Edition. It is thus not surprising that two of the contributors in this section are involved in the editing of Einstein's papers. Robert Schulmann's paper draws heavily on new archival material, to a large extent unearthed by himself, that sheds new light on Einstein's Swiss years. Schulmann argues convincingly that the old myth of Einstein as an outsider, who suddenly, through his revolutionary work on relativity, statistical physics, and quantum theory, was called back into academia from his exile in the Bern patent office, is indeed a myth. Throughout the patent office years Einstein kept in touch with some of his academic colleagues, and his return to academic life was carefully prepared and executed. The article by Kox shows, through the example of an episode in Einstein's life, the importance to historians of having an edition such as the Einstein edition available.

The article concerns the writing, submission, and eventual retraction, in 1911–1912, of a paper by Einstein on the theory of residual rays. By putting together the various pieces of evidence that can be culled from Einstein's correspondence of that period, now conveniently collected in Volume 5 of the Einstein Papers, the background to the paper, most of its contents, and the reason for its retraction can be reconstructed. As in earlier articles in this volume, in Kox's article, too, experiment and its interaction with theory take a central place, as it was Einstein's disbelief in certain experimental results that prompted his paper, and it was his eventual acceptance of those same results that made him retract it. In the meantime he had argued for a different interpretation of the data, an interpretation based on theoretical considerations. In the article by the renowned Einstein scholar Gerald Holton, finally, we encounter another aspect of Einstein's personality: his love of books. Holton traces Einstein's intellectual development through the books he read and outlines an important future research project, namely, to study the influence of various books on Einstein's thinking through an analysis of the detailed inventory of Einstein's library that is currently being prepared.

In a variety of ways, Einstein's work is a culminating point in the history of science; so also in the composite narrative that emerges from Martin Klein's contributions to the history of physics, and so also in the present volume, where the threads collected in their bearing on Einstein diverge in various directions in Part V, *Further Perspectives*. Stephen Brush's paper on prediction and theory evaluation continues the theme of the interaction of theory and experiment. It has been widely believed that predictive power furnishes an especially acute test of theory, so that a theory which makes successful predictions – especially if the phenomena predicted run counter to existing belief – will command assent. What Brush demonstrates, in brief, is that this is not true historically. Other issues, such as whether the predicted experimental result is seen as of core relevance to the theory, and whether the theory itself has the kind of internal coherence and structure that will make it generally acceptable, are more important, and will outweigh a given incident of successful prediction in molding the opinion of the scientific community. Experimental tests of the general theory of relativity furnish one set of Brush's examples, and there is, it might be suggested, another Einstein connection as well, having to do with his delineation of two aspects of theory evaluation, respectively the "internal" and the "external." The

internal criterion has to do with the internal coherence, harmony, etc. of the theory; the external criterion refers to the fit with experiment. As Brush's investigation shows, especially in the case of Hannes Alfvén's astrophysical and cosmological theories, if the internal criterion is not satisfied, no amount of successful prediction will be convincing.

Two of the articles in Part V, by Daniel Siegel and Abraham Pais respectively, address historiographical issues bearing on the location of the history of physics enterprise at the borderline between physics and history of science, where technical material characteristic of the physics discipline is treated, but with the methods and perspectives of the historian. Martin Klein's work has been at this borderline, furnishing paradigmatic examples of what can be accomplished in this area that will command the respect of both historians and physicists. The history of science discipline was institutionalized in the universities in the period after World War II, partly in response to the feeling that, in the aftermath of the development and use of nuclear weapons in that war, it was necessary for society and, in particular, the intellectual community, to come to grips with the interactions between science, technology, and society, in part through studying the history, philosophy, sociology, and politics of science. The history of science enterprise was envisioned, in this context, as a highly interdisciplinary kind of undertaking, having relationships with science, history, philosophy, sociology, political science, etc. In recent decades, however, history of science has become more of a subdiscipline of history than a truly interdisciplinary undertaking, and relationships with the sciences have been de-emphasized as a result. Pais and Siegel, in their respective articles, voice some discontents with this state of affairs.

Pais's concern in this is primarily with the writing of history, while Siegel is concerned with the reading of historical documents. Pais discusses the primacy of narrative in historical writing, from the journalistic to the scholarly: it is the telling of the tale that matters, that bears the magic, as Pais illustrates with a Chassidic legend. And when the tale is at the borderline between physics and history of science, the teller should be one who understands the physics – otherwise the magic disappears. Siegel, in a parallel argument, suggests that the reader of a historical text that has technical content must be one who is prepared to read that technical material as it was meant to be read, that is, with pen or pencil in hand, poised to fill in the missing steps in the argument, as the reader

of mathematical material is almost always called upon to do. Siegel as well makes use of a literary example: a poem by Maxwell that must be read as it was meant to be – out loud – in order to get its message. The concern with reading, writing, and narrative brings us full circle: back to the perspective of the historian, no matter how technical the historical materials may be.

Finally, in the paper by Roger Stuewer on the seventh Solvay Conference, held in Brussels in 1933, we come to the chronological end of the volume. Martin Klein has made good use of conferences, including the Solvay Conferences, as microcosms for the study of historical situations in science; Stuewer here applies the approach to a later time and a different topic, namely, nuclear physics. The seventh Solvay Conference represented both a coming to an end and a new beginning. With the intellectual migration of that period (illustrated by Stuewer as relating to the participants in the conference), the old order characteristic of the period covered by this volume, in which the history of physics was a story of European, especially British, French, and German science, was breaking down, thus beginning the transition to a Post-World-War II situation in which science and technology in the Americas and in Asia play central roles. Also, in physics itself, there was what appeared, briefly, to be an ending, which then very quickly gave way to a new beginning: The quest for the ultimate building-blocks of the universe had been taken down to the molecular level in nineteenth-century kinetic theory, then down to the atomic and subatomic levels in the decades after 1890, and finally to the nuclear level in the second and third decades of the twentieth century. For a moment in the 1920s the quest appeared to have ended with the identification of the proton and the electron as the elementary positive and negative charges and the ultimate elementary particles. However, with James Chadwick and the discovery of the neutron; P. A. M. Dirac, Carl Anderson, and the positron; and Hideki Yukawa and the meson (as variously discussed in the articles of both Stuewer and Brush), this paradise turned out to be, if not exactly a fool's paradise, then perhaps an Eden lost.

With Roger Stuewer's article, then, we write *finis* to the period in the history of physics characterized by Cavendish, Fourier, Faraday, William Thomson, Maxwell, Gibbs, Hertz, Einstein, and Rutherford, and anticipate the nuclear zoo and other novelties of the end of the twentieth century. Our survey of the period from Cavendish to Ruther-

ford, especially by way of Maxwell, Gibbs, and Einstein, constitutes our tribute to Martin Klein on his seventieth birthday. May he flourish!

A. J. Kox, Daniel Siegel
June 25, 1994

MARTIN KLEIN AT YALE

I first met Martin Klein to talk to in 1964 and in curious circumstances: a few miles above the North Atlantic aboard a KLM airplane bound for Amsterdam. We were, I seem to recall, only three passengers: the third was Derek Price. We talked of many things, and the flight seemed short, at least to me. It may have seemed short to Martin, too, and certainly was when compared to another flight of his, also connected with Amsterdam, where he had stopped to buy cigars on his way home from Denmark. His plane out was hijacked and flown via Beirut to Cairo where it was blown up while he was running away from it through the desert in someone else's shoes. Since then his travel luck has been better, though not of the best.

Martin stayed in Amsterdam, while Derek and I flew on to Hamburg and began plotting ways and means of inducing him to join us at Yale – it took three years, but we succeeded.

In the interval he came to New Haven to give several memorable colloquia which caused the formation of the nucleus of a growing band of faithful followers who do not willingly miss a lecture of his. We were not surprised that he became a popular teacher among Yale's undergraduates and that he achieves his popularity sacrificing neither his subject's dignity nor his own.

His excellence as a lecturer and expositor rests, of course, first on his intimate and detailed knowledge of the warp and weft in the fabric of physics of the late nineteenth and early twentieth century: he can in an instant trace the threads that connect any two related events. His style at the lectern is as far removed from the histrionic as one can imagine, though not without art. He plays his cards quietly and cunningly from a carefully arranged deck and addresses his audience directly, without the intervention of a manuscript, always keenly aware of his listeners' limitations and needs.

I saw a striking example of this last in a lecture to the Connecticut Academy of Arts and Sciences whose chief claim to glory is the publication in 1875–1878 of Gibbs's "On the Equilibrium of Heterogeneous Substances" in its *Transactions*. A celebration of this event's centenary seemed in order, and Martin was the obvious choice for celebrant. He

had prepared himself with the promised presence of most of Yale's physicists in mind. However, at the very last moment we learned that a departmental emergency prevented them from attending, so he faced an audience void of expertise in physics (though not of intelligence). Without so much as a blink – at least as far as this observer could see – he proceeded to give one of the best lectures I have heard, elegant, with few technicalities, yet rich in substance, obviously not the one he had planned, but perfectly attuned to the unforeseen situation.

As said, Martin came to Yale in 1967. Because of an administrative tangle, he was given a presidential appointment, and it became my pleasant task to announce his arrival to Yale's Board of Permanent Officers. To prepare myself I looked carefully at his curriculum vitae and was startled to learn that he had been a child prodigy, for he exhibited none of the conventional tell-tale signs. Indeed, he had graduated from Columbia University in his native New York City with a bachelor's degree in physics at age eighteen, and obtained his master's degree there two years later. He was all of 24 years old when he became a doctor at MIT, but he had by then spent two years in war research for the US Navy – it had to do with the propagation and detection of sound under water (he is an excellent swimmer).

Martin began his tenure at Yale on leave so he could finish, and see through the press, his book on Paul Ehrenfest. I remember well, and with pleasure, an expedition toward New York City in search of an early copy of the *New Yorker* magazine that had in it Jeremy Bernstein's review of it – an informed and enthusiastic long essay. We all basked in his glory. It is, of course, a remarkable book, beautifully written as is all his published work, with great clarity and insight, and ample, but not too many technical details. It can be, and has been read with profit and pleasure by professional and lay alike. I have talked with several scientists who had known Ehrenfest, and all thought it astonishing that Martin, who had not, was still able to capture his essence to perfection.

Martin's principal field of work and mine are separated in time by some two millennia, yet we joined forces in a seminar on Newton's *Principia* and slogged our way through it proposition by proposition. It was heavy going – Newton did not feel kindly toward his reader – but it was great fun and I, for one, learned much.

We held the seminar at the end of the sixties, the golden decade of academic life in America. Never before or since have academic endeavours been so eagerly and generously encouraged and supported, and

never before or since and, for that matter, nowhere else, has academic life been so exciting and flourishing. They were heady days, and we were fortunate to have excellent graduate students and visitors, also from abroad, many attracted to Yale by Martin's presence. They all left with his imprint on their standards and style of work.

It behooves old men to count their blessings, but all too many must, alas, find it all too simple. I am not among them for I have been very fortunate and my blessings are many and great. Not least among them is having had a profession and been in circumstances that brought me Martin Klein as a colleague and friend.

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