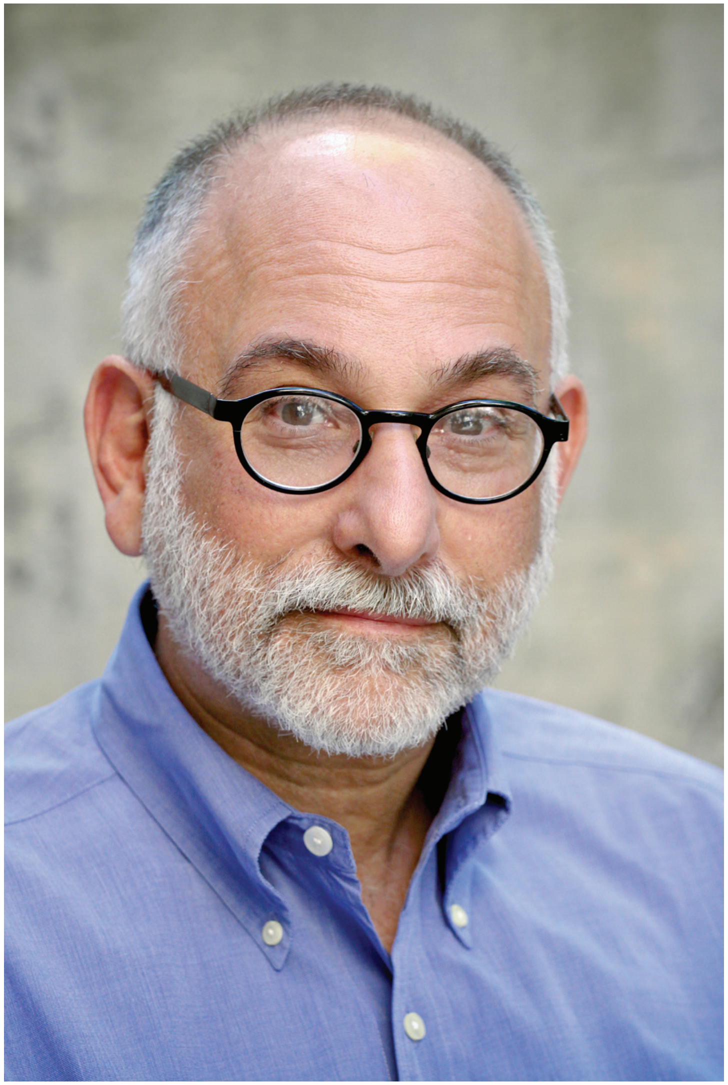
LOOKING BACK AS WE MOVE FORWARD



Looking Back as We Move Forward

The Past, Present, and Future of the History of Science

Liber amicorum
for Jed Z. Buchwald on his 70th birthday

April 2019

ISBN 978-0-578-45417-7

Contents

Foreword

THIS COLLECTIVE HOMAGE brings together forty-one scholars hailing from fifteen countries who endeavor to wrestle with the main intellectual and institutional changes in the history and philosophy of science and technology, of mathematics, astronomy, physics, biology, and economics over the past four decades. Three generations of practitioners reflect on the intersection between their own education, careers, and research and what in their estimate are the most significant contributions to be found in the more than 100 articles, books, and reviews that Jed Z. Buchwald has written or co-authored. Many essays contain personal reminiscences, others are quite scholarly, but all are affectionate and humorous.

The volume has been prepared in advance of an international conference entitled "*Looking Back as We Move Forward: The Past, Present, and Future of the History of Science*," to be held at the California Institute of Technology in Pasadena on 25–27 April 2019. A summary of findings and conclusions of this conference will be published in a separate report, but certain arguments already emerge in this volume.

John Heilbron states starkly, and amusingly, the dilemmas of the historian, the overarching theme of this volume: "It is the great fault of our discipline that we do not know what we can safely ignore. We are like Bacon writing about color without knowing whether the key to its character resided in a rainbow or a peacock's tail." In reply, both Heilbron and Martinez point out that Buchwald cultivated the notion that, in fact, historians ought to proceed as professional agnostics when attempting to ascertain how past scientists studied nature, without presupposing what allegedly must have been the case. It was one of Buchwald's important contributions that he drew attention to the unarticulated, often unconscious presuppositions of scientists' principles, ones that can be uncovered by the historian through the reworking of scientific practice, whether on paper or in the laboratory, without venturing into easily refutable rational reconstructions (Darrigol).

Many authors recount how, until the 1970s, understanding change in theory was generally of paramount concern to scholars, while the careful and detailed examination of calculation and measurement in the practice of physical science, or in biology, was largely ignored, although it had been adopted in the history of astronomy and mathematics (Gray, Shapiro, Taub). Buchwald was a pioneer in this field, as he was also in the analysis of discrepant measurements and data, a topic relevant for today's crisis in the replication of scientific results (Franklin, Shapiro).

To the question: How can one make reliable historical statements?, many answer that the trend of juxtaposing "bits of culture and science that were occurring simultaneously, and which might appear to be analogous, is a necessary yet woefully insufficient historiography to demonstrate causality" (Jackson). Buchwald's analysis of the physics laboratory as an engine of discovery of new *physical effects* provided him also a road map and novel way to create *historical* effects (Olesko). This approach would entail adopting "the *fractal model for history* since it replicates the same pattern at every scale of complexity. Close attention to the broad sweep of contemporary (to us, not to the historical subjects) scientific developments can serve as a resource for understanding the craft of writing." (Gordin) "The aim, and ability of combining great sensitivity to minute technical details—call it the *particular*—and the

optimal approach, which offers the view from above, the general, the approach that brings a myriad details into a coherent and convincing whole," is an ideal to be aspired to in historical work (Hon). Making something "real" in historical scholarship is very similar to making something "real" in physics (Olesko).

Buchwald early on insisted that attention be paid to the "technologies" of science, to craftsmanship and artisanal skills, never ignoring that "scientific work is a creative intellectual process. This idea, which may seem obvious, is missing from a lot of the secondary literature today that has been influenced by the sociology of science" (Kingsland). Many authors remind us that historians, and even philosophers, benefit from delving not only into published accounts, but into archives, correspondence, and diaries (Lützen), and that the mining of texts for nuances, for the origin of significant phrases and their connection to philosophical and even theological contexts, and careful attention to details of translation, can illuminate knowledge taken for granted and never queried (Feingold).

Jed has also encouraged the "dialogue between historians of science (including social constructionists among them, a group hitherto outside his purview), philosophers of science, and members of the STS community" (Olesko). The challenge of producing "finely-grained, detailed studies that engage deeply with questions relating to human knowledge, and research that is driven by intellectual curiosity and engagement with philosophy" are the kind of narrative-driven work that many see as the hallmark of Jed's contributions to the field and that they would like to see being emulated (Taub, Chang, Hoynigen-Huene)

The unity of knowledge *about* science, of bringing the histories of science and technology into closer contact with the philosophy of science and creating something similar to the "single conception of the probing character of human knowledge [that] bound together a Newtonian triad of history, theology, and science," as Jed and Moti Feingold argued, would also help to overcome the fragmentation of knowledge. In this respect, some authors encourage us to ask big questions—and attempt unification rather than fragmentation—an attitude that goes against "trends in history and philosophy of science that might content themselves with ever more contextualized, micro histories." (Laubichler & Renn, Chemla, Taub). That such a deterministic, evolutionary principle ought to be sought is formulated quite forcefully in the last essay: "Historians traditionally view their subject as unfolding in an essentially random way, contingent upon the violent, retributive whims of a citizenry and the political machinations of a handful of influential individuals. But history is more accurately seen through a more deterministic lens in which it obeys its own internal logic. Most of history, including the history of science and technology, is preprogrammed" (Ausubel).

The essays emphasize that Jed also has been a prolific supporter, promoter, and organizer of the profession, and that his work as editor of book and journal series since 1994 has been a massive undertaking. He has made possible the publication of some 250 single book volumes or journal issues at a rate of almost one volume per month for the past 25 years. Many of these volumes were the result of his personal interactions with their authors, to whom Jed strove to give constructive guidance and encouragement.

In collaboration with Jeremy Gray, Jed has brought out some 15,000 pages in the *Archive for History of Exact Sciences* alone. There are now 60 volumes in print in the *Archimedes: New Studies in the History and Philosophy of Science and Technology* book series. Several authors refer to the beautiful books produced by MIT Press in the *Dibner Institute Studies in the History of Science and Technology* series and in *Transformations: Studies in the History of Science and Technology*, for which Jed curated, as one says today, some 45 path breaking monographs. Undoubtedly proud to have been invited to serve for more than 30 years as a successor of Clifford Truesdell, editor of *Sources and Studies in the History of Mathematics and Physical Sciences*, Jed has overseen the publication of 52 volumes in the series. And, more recently, he has ventured so far as to found and co-edit a series called *Mathematics, Culture, and the Arts*. Twelve colleagues who have served on editorial boards to select promising works for these various series have contributed essays to the present volume. The breadth of topics covered in these many edited books and articles testifies to Jed's ecumenical and merit-based encouragement of scholarship truly meant to contribute to the advancement of knowledge (Swerdlow).

Chen-Pang Yeang, Jed's last graduate student, sums up what others write, too:

> I do think that I have learned a thing or two from the 'Buchwald School,' if there is such a thing. That is not necessarily only a wealth of knowledge in the history of physics and mathematics, a preoccupation with technicality, a preference for the 'internal' approach … or the writing of papers and books filled with equations, diagrams, and descriptions of instruments and procedures. Rather, the 'Buchwald School' to me is the embodiment of an attitude toward historical scholarship—the attitude of paying supreme attention to details; of conducting research with extreme caution but bold hypotheses; of being driven and intrigued by the burning curiosity about what exactly happened, how it happened, and why it happened in this, and not that, way; and of letting facts and evidence speak for themselves but also insist on finding reasonable interpretations.

The essays in this volume are presented roughly in the chronological order in which the authors became acquainted with Jed, and cover the years: 1974–1992 at the Institute for the History and Philosophy of Science in Toronto; 1992–2001 at the Dibner Institute at MIT in Cambridge, Massachusetts; and 2001 to the present at the California Institute of Technology in Pasadena. The last word in the book is accorded to Jesse Ausubel, whom Jed has known since early childhood.

Many thanks to Sini Elvington and Tom Whitridge for assisting in the editorial and typesetting process, and to the following institutions and foundations for the kind support of the conference, the production of this volume, and the associated audio-visual and digital materials: the Division of the Humanities & Social Sciences and the Provost's Office at Caltech; the Sloan Foundation Program in Public Understanding of Science & Technology; the William & Myrtle Harris Endowment for Science & Civilization at Caltech; the Francis Bacon Foundation; and the Richard Lounsbery Foundation.

DIANA KORMOS BUCHWALD

Trevor Levere

JedFest

When jed was a stripling, a mere 50 years old, Diana secretly invited several of his friends to join him in Palm Springs during the meeting of participants, also old friends, to wrap up the Sloan workshop. Jed came in to lunch, and had to be prodded by Diana before he realized that the group had grown beyond the official participants. For me, two of the highlights of that meeting were Alan Shapiro's surprise in discovering that road runners existed outside of Wile E. Coyote cartoons, and my taking a photo of Jed and Diana in a dry desert swimming pool. Since that 50th birthday, Jed's antennae have become more sensitive, and he would have known that something was afoot even had he not been told about this JedFest.

Jed's first academic appointment was in the fledgling Institute for the History and Philosophy of Science and Technology at the University of Toronto, where Polly Winsor and I were the other youngsters, younger indeed than some among the small cohort of graduate students. It took him a while to get used to Canada; one day in early April, he returned from a brief trip, to find the campus blanketed in snow, and was downright indignant. He adjusted to the climate, without ever quite forgiving it. On one outing to a frozen waterfall, followed by Irish whiskey at a nearby inn, he rolled up the cuff of his jeans to reveal a plaid lining—made in the USA, but Canadian by inspiration. His tenure hearing went smoothly, of course, and we celebrated in the Copenhagen restaurant, sampling almost every brand of aquavit in their well-stocked bar. That may have had some bearing on his decision to spend his first sabbatical leave in Aarhus.

He submitted the typescript of his first monograph to the University of Toronto Press, and as far as I know never heard back from them. After six months, I suggested that he should find a more responsive publisher, and in due course present a copy of the book to the U of T Press. Chicago published *From Maxwell to Microphysics*, and for all I know Toronto still has its copy of the typescript at the back of some cupboard.

For several years, Jed served as Graduate Secretary at the Toronto IHPST, in charge of the graduate programs. He welcomed one incoming cohort of students with a smile that showed his teeth, telling them: "I want you to look on me as Torquemada." He told another cohort that, since he decided to write his doctoral dissertation on a topic in 19th century physics, he began by reading every book and paper published in physics throughout the century, in the principal European languages. (That was overstatement. How could he have ignored Dutch publications?) For all that this intensive and total immersion sounded intimidating (and one former student told me that Jed taught by intimidation), and for all his intellectual rigor, he was liked as well as admired by the students. Jed is going to endure a lot of praise during this gathering, so he might bear in mind that, as another friend said at a similar meeting, beatification in Judaism generally comes while the one beatified still lives.

Jed, having named his black lab after himself (although no one I knew ever called Jed "Bucky"), moved comfortably up through the ranks in academia, became director of his and my Institute, and then segued into the role of founding director of the Dibner Institute. Students, colleagues, and friends became

visiting junior and senior fellows there, until it seemed that that Institute had welcomed just about everyone who pursued the rigorous scholarly enquiry that Jed demanded of himself and his students. Like-minded collegiality was the order of the day. I found that visiting historians and philosophers provided stimulating and challenging company, the most valuable often from fields other than the history of chemistry. Jed asked Larry Holmes and me to organize a Dibner workshop on experimentation and apparatus in the history of chemistry, an invitation that I welcomed because of the opportunity to work with Larry, and because this was a topic in the history of chemistry that had been overly neglected and that even today continues to intrigue me. An additional bonus was the opportunity to get to know historians of chemistry who became and remain friends.

I have never worked with Jed on a research topic, in spite of occasional visits to Caltech, Diana, and Jed, but he can still take some blame or credit for the directions I have followed in the history of chemistry. Thank you Jed.

Alan E. Shapiro

Jed's 'Experimental Way'

One day in the spring of 1978 Tom Kuhn came into my office at the Institute for Advanced Study, where I was spending my first sabbatical, to ask if I could help one of his former students with a paper. Forty years later I cannot be expected to recall exactly what Tom said, but it was something like: He was a really good student when he left Princeton for that other place, but they seem to spend more time there talking than writing, and now he needs a little bit of help. The former student, of course, was Jed. Jed's paper was on Huygens and double refraction, and Tom knew that I worked on the history of optics and had a few years earlier published a large paper on Huygens. In retrospect, I am pretty sure Tom was engaged in some match-making, while unloading the paper on me—he certainly had enough on his plate then—because the paper wasn't bad at all. Indeed, it was pretty good. Jed came down to the Institute, and we spent a good part of the day on his paper. There were a few problems that we resolved by working together. As I remember we learned from each other and had fun doing it. When published in the *Archive for the History of Exact Sciences* (which he now edits), it was an important paper. This would not be the first time we would work together on a problem.[1]

1. *Buchwald 1980a.*

In the 1980s history of science was beginning to change significantly. What was then called "internal" history of science, notably, a concern for the development of scientific concepts and theories, was subject to a variety of critiques. By the 1990s this had become a full-scale attack and, in the perception of many, an attack on science itself. This was the era of the "science wars." To those of us who did the sort of history of science that was increasingly rejected and discredited, Jed was an increasingly important practitioner and supporter. Not that Jed was digging in his heels and taking retrograde steps. He led a group of us (the "Sloan Rangers," as we became known) in putting forward a proposal to the Sloan Foundation on "The Nature and limitations of historical knowledge about scientific objects and their investigators." The aim was to investigate the possibility of gaining reliable historical knowledge about scientific entities and scientists' convictions about them. After three years of our exciting quarterly workshops, I had a much deeper understanding of the complexity of developing historical knowledge. One feature that has long characterized Jed's work was assigning a prominent role to experimental practice, which was a theme of a number of our Sloan workshops. This removed the study of scientific concepts solely from the realm of history of ideas and, as Jed wrote in another context, placed "a workbench-like emphasis on the concrete sources of past scientific experience, whether embedded in objects, mediated by techniques, or displayed in words and images."

In the summer of 1998, Jed and the Dibner Institute held a one-week workshop for postdoctoral junior scholars on Cape Cod to examine historical and philosophical issues concerning such entities as the various 18th and 19th century fluids, chemical structures, and short-range forces. The focus was on the ways in which such entities came to life on paper and in the laboratory. I led one day's session on Newton, the spectrum, and its subsequent history through the early 19th century. On the second day we set up a lab to repeat a number of Newton's optical experiments. The preceding spring Jed and I had spent a week in Cambridge repeating and working up the lab experiments. In developing the lab we learned much both from the experiments and each other. When I looked over the documents from that workshop to prepare this essay, I was astounded at the perspicacity of the selectors (Jed was among them, I was not). Twenty years later, I recognize at least six of the ten participants as leaders of the field: Theodore Arabatzis, Hasok Chang, Ofer Gal, Myles Jackson, Jessica Riskin, and Friedrich Steinle.

I want to return to the paper on double refraction that first brought Jed and me together, for it was notable for its methodology. The paper was concerned with the calculation of numerical results and techniques of measurement in experiments and observations of double refraction in Iceland crystal in the long century between Huygens's proposal of a law and Malus's confirmation of it. Jed meticulously examined both calculation and measurement and largely ignored theory. This approach was not then common in the history of the physical sciences, although it had been adopted in the history of astronomy. A concern for numerical results and measurements subsequently became more widespread, as it helped us to understand how scientists actually practiced experimental work and accepted and rejected theories.

A hallmark of Jed's work—in his publications, and in the conferences and workshops that he organized—which has influenced the broad community, has been his concern for experiment, calculation, and scientific practice. In my opinion his long-time concern culminated in his paper "Discrepant measurements and experimental knowledge in

the early modern era" in the *Archive for the History of Exact Sciences* in 2006. He there takes up the crucial question—only occasionally touched upon by others—of how scientists deal with and report multiple measurements that necessarily will always differ from one another.

At the last meeting of the Sloan workshops in Palm Springs, we celebrated Jed's fiftieth birthday. I am, to be sure, delighted that he is still with us twenty years later (not to mention my equal delight at my own persistence). After a winter in Minneapolis, my wife Linda, who knew Jed and many of the other participants, joined us in Palm Springs. For the occasion she wrote a doggerel birthday tribute for Jed. Since it was of near epic length, I cannot reproduce it all here. The final stanza, though, is still apt on his seventieth birthday.

Though Jed's a man of tastes sophisticated,
I think he'd be quite miffed and would berate us
Did we not recognize that present laughter
And simple joys of life are all he's after.
For instance, he's been known to get quite frisky
On English chintz, and drinking Irish whiskey
Although he's not averse to Brandy French,
Or any drink, as long as there's a mensch
To share it with. So here we are to thank you
And toast you, and most certainly to rank you
Among the best and brightest that we know—
Here's cheers and chin-chin, mazel tov, and skoal!

John L. Heilbron

Of Anachronisms

Dear Jed,

Your attainment of the biblical limit of normal human development naturally raises the question how well you have matured during this allotted span. It is with great pleasure and in warm friendship that I offer a few hints at an answer to your eventual biographer.

You will certainly recall our conversation in London, at what I remember was our second meeting. It occurred at a watering hole off Pall Mall to which we had fled from a dry session at the Royal Society. You then proposed to list in order of merit all the historians of science at the meeting and as many more as I could name. I have preserved that precious list. Many who will attend the symposium in April 2019 will be surprised to know how far down on it their names appear.

Our earliest substantive discussion also took place in England, at Grasmere in March 1984, at a posh hotel where we reviewed the doings of old Cambridge wranglers. You were then rapidly consolidating your position as the greatest living 19th century physicist and I was beginning the thankless task of editor of *Historical Studies in the Physical Sciences*. Naturally I wanted to secure a real wrangler for the journal. Our negotiations came to grief over the important question how faithful the historian had to be to the nomenclature used by historical actors. Your position was that exact transcription was obligatory. Since your teacher Laplace wrote out everything

in three dimensions and frequently changed his impenetrable notation, the journal had not enough paper or its readers enough patience to meet your counsel of perfection. I suspect that at the time you were deferring to our master Tom Kuhn, who taught a form of necromancy by which the adept could crawl around in the heads of dead physicists and learn to speak the argot of their paradigms. Or rather argots: for few dead men speak the same language, or speak at all, and it is unusually challenging to master enough argots to say why their living native speakers had not been able to understand one another.

You will remember that this matter came up again when we participated in the Kuhnfest at MIT in 1990. I had concluded that since scientists could progress without speaking the same language, they must use some sort of average argot, or pidgin, which in our time is couched in approximate English. It seemed to me that in practice historians who wanted to communicate what they thought they had discovered about the past had to do so in terms of the average argot. Tom would not hear of this betrayal and pointed to your work as exemplary, which indeed it is. But not because you had remained faithful to exact transcription: By then you had compromised your historical accounts by introducing vectors and other anachronisms, and had entirely abandoned direction cosines. You spoke of schools of thought with complicated names like “Helmholtzian” without worrying that its members could not understand one another; you compared schools without worrying that their paradigms were incommensurable; and you approached the greatest Helmholtzian, Heinrich Hertz, in the style not of a necromancer, but of a biographer. To quote you exactly, you looked, and are still looking, into Hertz’s “mental world.”

Your recognition that the wider life experiences of scientists might be relevant to their scientific work was an important milestone in your intellectual journey. That still left some way to go, however, for you dropped character when you entered physics, much as the authors of Soviet texts ignored their Marxist prefaces when they reached the safety of science. You explored Hertz's acts as a Helmholtzian with the most admirable penetration; but you said nothing about the art he saw or the music he heard or the breakfast he ate or the girl he lost before going to the laboratory. What authorized these omissions? It is not your fault that you have no answer. It is the great fault of our discipline that we do not know what we can safely ignore. We are like Bacon writing about color without knowing whether the key to its character resided in a rainbow or a peacock's tail. What we think important in our reconstructions will not satisfy our successors, indeed, does not satisfy them, for, as you know at your advanced age, they are already on the scene; and the more we omit, the greater room we leave them. I trust that your second volume on Hertz will give few footholds to revisionists.

I noticed another important historiographical breakthrough in your volume on Hertz apart from the truly ingenious interweaving of theory and experiment that is the heart of the book. You allowed yourself the casual remark that historians must be agnostics. By this you did not mean that we should disavow knowing anything at all, although that was the position toward which you unknowingly were moving, but that we should not take sides in the battle between rationalists and positivists. This is a hard doctrine: for however faithful to it we may strive to be, we frequently relapse into writing about the world and its physical constituents as if they existed. And I think that you too have not always adhered to your doctrine. In your paper for the Kuhnfest, for example, you frequently used phrases like "it could not be that way at all." It

is as hard to remain agnostic as to avoid whigism, a truth that the great scourge of whig history, Herbert Butterfield, amply demonstrated in his writings on the history of science.

You came to know all this and much more owing in large part, I suppose, to your experiences as director of the Dibner Institute and editor of several journals and many books. As director you had to listen to lectures on all respectable subjects; as editor you have published volumes that, taken as a whole, would challenge the erudition of a university; and as both you learned to adhere scrupulously to the agnosticism you previously had only preached. In this year of your coming of age, you have capitalized on your liberation from the jejune prejudice of knowing what you are talking or writing about to complete an account of the decipherment of hieroglyphics that breaks new ground and is chock full of discoveries. This implausible accomplishment makes a perfect measure of the distance you have come in the years I have known you. Whereas in your salad days you knew the exact merit of every one of us and the entire meaning of each of the many symbols you wrote out, now you discourse confidently about a language in which you cannot tell a hymn for the dead from a menu for lunch. Bravo Jed!!!

Lately I have had the pleasure of observing on site the mellowing of your special human qualities. Here I can say that I have followed your lead and am the better for it. Formerly when we dined together, each of us would consume a steak while we ran down our colleagues; now at your suggestion we divide a single steak, and criticize only the deserving; and soon, as an agnostic vegetarian, you will find no faults in any of us worth mentioning. Even without taking this last step, however, you command the admiration and affection of everyone who, like me, can claim the privilege of being your friend.

Noel M. Swerdlow

Regiomontanus's Prospectus and Defense of Scientific Publishing

Among Jed Buchwald's accomplishments, and a way of introducing the subject of this essay, is encouraging and supervising the publication of more books in the history of science than, I believe, anyone else. I cannot count the number of books, although I can visualize the shelves they occupy in Jed's office, but I do know that within the series *Sources and Studies in the History of Mathematics and Physical Sciences* and *Archimedes*, both published by Springer, and the volumes published by MIT Press, the number is quite large and continues to increase. And in addition is his editing of the *Archive for History of Exact Sciences*, also published by Springer. Of course, of greater importance than the number of books and articles is their quality, seriousness, and technical proficiency in an age when much if not most publication in the history of science barely attains an elementary scientific level. Indeed, were it not for these series, there would be little, or surely far less, scientifically competent publication in the history of science at all. I could go on and on about the importance of these publications, but enough has been said and their importance speaks for itself.

All of this is intended to introduce something similar and even more remarkable, more than five hundred years earlier, the very beginning of scientific publication by Johannes Müller of Königsberg, known as Regiomontanus,

the foremost mathematician and astronomer of his age. To pass rapidly through his life, he was born in the small Franconian town of Königsberg in 1436, studied with Georg Peurbach at the University of Vienna in the 1450's, went to Italy with Cardinal Johannes Bessarion in 1461, then to the newly founded University of Pressburg in Hungary in 1467, and finally to Nuremberg in 1471. Among his many works of importance were the *Epitome of the Almagest*, *On Triangles of Every Kind*, *Tables of Directions and Profections*, *Tables of the First Motion*, and the first seven-place sine tables to 1' of arc. His intention was to restore to the mathematical sciences all they had achieved in the past and then build upon that foundation to advance or even perfect them. An essential part of this plan was that, scarcely twenty years after the invention of practical typography, he founded in Nuremberg the first printing firm devoted to scientific publication, particularly the mathematical sciences, when not a single scientific work, let alone in the mathematical sciences, had yet been printed. As he wrote in July 1471 to one master Christian of Erfurt, who has been identified, although not with certainty, with Christian Roder, Rector of the University of Erfurt, "I am undertaking to compose with a letter press all worthwhile books of mathematics so that henceforth faulty copies will not annoy readers, no matter how discriminating." For, he explained, manuscript codices are commonly subject to error, and none more than mathematical, where unique elements are used for signifying each thing, and therefore greatly subject to corruption, not to mention astronomical tables where a single character omitted or transposed or corrupted in any way necessarily ruins an entire page.

Printing may have been established in Nuremberg as early as 1470 by Anton Koberger (although his first dated publication was 1472), so Regiomontanus was, if not the first, the second printer there, and he had to start from scratch. In addition to acquiring type and the equipment necessary for printing, between March and September 1472 he may have travelled to Italy for manuscripts. His first two publications, it appears by 1473, were Peurbach's *New Theories of the Planets* with numerous hand-colored woodcut diagrams, which became the standard instructional text on solar, lunar and planetary theory for the next century and a half, through more than fifty editions, some with lengthy commentaries; and Manilius's *Astronomica*, an astrological poem of the first century. He then began printing his own *Kalendarium*, in Latin and German editions, containing the ecclesiastical calendar with tables for conjunctions and oppositions of the Sun and Moon for 1475–1532, diagrams of eclipses of the Sun and Moon with times and magnitudes for the same period, and other useful tables and instruments on paper, and the most ambitious of all, *Ephemerides*, daily longitudes of the Sun, Moon, and planets with their astrological aspects for thirty-two years, 1475–1506, which he computed himself, running to some 900 pages.

While these were in progress, probably in 1474, he issued a prospectus of what he intended to publish, divided into works "by others," almost all of the then known and important mathematical, astronomical, and astrological works of antiquity, the Greek works in Latin translation, some in new translations or translated for the first time, which he intended to do himself, with a few more recent works, and "the attempts of the artisan," works of his own, about fifty publications in all. He also made some rather sharp, and amusing, comments about work of which he did not approve, concerning which more below. His entire prospectus is translated, with some additions in parentheses for clarity, as follows:

These works will be produced in the city of Nuremberg in Germany under the supervision of Johannes of Königsberg.

WORKS BY OTHERS.

- *New theories of the planets* by the celebrated astronomer Georg Peurbach, with appropriate figures.
- *Astronomica* by Marcus Manilius.

These two are completed.

- *Geography* by Ptolemy in a new translation, for the old one by Jacopo d'Angelo of Florence, which is in common use, is faulty since the translator himself (no offense intended) has sufficient knowledge of neither the Greek language nor of mathematics. In this verdict it will be proper to trust the best judges, Theodore of Gaza, an illustrious gentleman very learned in both Greek and Latin, and Paolo (Toscanelli) of Florence, by no means unacquainted with (the language) of the Greeks and extremely distinguished in mathematics.
- *Great Treatise* by Ptolemy, which is commonly called the *Almagest*, in a new translation.
- *Elements* by Euclid with the *Anaphorica* by Hypsicles in the edition of Campanus, but with the greatest part of the errors removed, as will also be shown in a special short treatise.
- *Commentaries on the Almagest* by Theon of Alexandria, the illustrious mathematician.
- *Astronomical hypotheses* by Proclus.
- *Tetrabiblos* and *Centiloquium* by Ptolemy in a new translation.
- As much as is found of Julius Firmicus (Maternus).
- Leopold of Austria, and if any other astrological forecasters seem worthy in reputation, for instance, whatever

fragments there are of Antonio de Monte Ulmi will also be expounded in their numerous uses.

- The works of the most acute geometer Archimedes: *On the sphere and the cylinder, On the measurement of the circle, On conoids and spheroids, On spiral lines, On the equilibrium* (*of planes*), *On the quadrature of the parabola, On the numbering of sand*. With the commentaries of Eutocius of Ascalon on three of the works mentioned, namely, *On the sphere and the cylinder, On the measurement of the circle, On the equilibrium* (*of planes*). The translation is that of Jacobus Cremonensis, but in some places corrected.
- *Optics* by Witelo, an excellent and renowned work.
- *Optics* by Ptolemy.
- *Harmonics* by Ptolemy with Porphyry's exposition.
- *Spherics* by Menelaus in a new edition.
- *Spherics*, *On habitations*, *On days and nights* by Theodosius in a new translation.
- *Conics* by Apollonius. Likewise *Cylindrics* by Serenus.
- *Pneumatic inventions* by Heron, a mechanical work of great delight.
- *Elements of arithmetic* and *On given numbers* by Jordanus.
- *Four-part treatise on numbers* (probably of Johannes de Muris), a work rich in diverse subtleties.
- *Mechanical problems* by Aristotle.
- *Astronomia* by Hyginus, with a delineation of the figures in the heavens.
- Moreover, a tree of Ciceronian rhetoric has been made in a splendid likeness.
- And a map (*descriptio*) of the entire known habitable world will be made, commonly called a Map of the World (*Mappam mundi*), besides a separate depiction of Germany, also of Italy, Spain, all of France, and Greece. But it has also been decided to summarize briefly the descriptions (*historias*) of each one drawn from very many authorities, namely, such as seem to pertain to mountains, seas, lakes and rivers, and other specified places.

THE ATTEMPTS OF THE ARTISAN.

Although natural modesty and the republic of letters have long debated whether they should be published or not, reason has held them worth the risk.

- *New Calendar* by which the true conjunctions and oppositions of the luminaries are provided, and likewise figures of their eclipses, true daily positions of the luminaries, differences between equinoctial and seasonal hours at any habitations you wish by means of a two-fold instrument, and many other things very agreeable to know.
- *Ephemerides*, which are commonly called an *Almanac*, for thirty-two years to come, in which you will look upon the true daily motions of all the planets and the head of the dragon (the ascending node) of the moon, together with the aspects of the moon to the sun and planets, with even the hours of their aspects noted, which is not without value, and with the aspects of the planets to each other also not omitted. In the front matter of the pages, lists of latitudes (of places) are set down. And finally if any eclipses of the luminaries are to take place, they are drawn in their proper positions.

These two works are now nearly completed.

- Large commentaries on Ptolemy's *Geography*, in which is explained the manufacture and use of the meteoroscopic instrument by which Ptolemy himself derived nearly all the numbers of his entire work. For one would believe erroneously that the numbers of so many longitudes and latitudes were discovered through observations of the heavens. Further, a description of the armillary sphere together with the entire habitable world in a plane is made so clear that most people will be able to learn everything, which no one has hitherto comprehended in Latin, since he has been hindered through the fault of the translator.

- A special short treatise, which will be sent to the judges, against the translation of Jacopo d'Angelo of Florence.
- *Defense of Theon of Alexandria against George Trebizond* in six books, from which anyone will clearly understand that his commentaries on the *Almagest* are worthless, and that his translation of the work of Ptolemy is not without error.
- A short treatise by which it is clearly shown that the opinions of Campanus ought to be removed from his edition of the *Elements of geometry*.
- *On the five equilateral bodies*, which are commonly called regular, specifically, which of them fill up a corporeal space and which do not, against Averroës, the Commentator of Aristotle.
- Commentaries on those books of Archimedes that are lacking an exposition by Eutocius.
- *On the quadrature of the circle*, against Nicholas of Cusa.
- *On the motion of the eighth sphere*, against Thābit and his followers.
- *On the restoration of the calendar of the Church*.
- *Epitome of the Almagest*.
- *Five books on triangles of every kind*.
- *Astronomical problems pertaining to the entire Almagest*.
- *On the size and distance of a comet from the earth, on its true position*, etc.
- *Geometrical problems of every kind*, a work of profitable delight.
- *Pannonian game*, which at another time was suitably called the *Tables of directions*.

- *Great table of the first movable*, with numerous uses and certain computations.
- *Sighting rods of many kinds* with their uses.
- *On weights and aqueducts*, with figures of instruments necessary to these things.
- *On burning mirrors* and other mirrors of many kinds and of astonishing use.
- In the workshop an *astrarium* (astronomical clock) is constantly under construction, an achievement certainly worthy to be viewed as a wonder.
- Some astronomical instruments for observations of the heavens are also being made, and likewise other instruments for common daily use, to recite the names of which would be tedious.
- Last of all, for the sake of enduring written memorials, it has been resolved to commission that wonderful art, a foundry of letters (typography), whereby when completed, God willing, even if the artisan soon falls asleep, death will not be bitter, since he will have left so great a gift in inheritance to posterity that they will forever be able to deliver themselves from want of books.

As impressive as Regiomontanus's prospectus is, it is not to be wondered that its critical, if not dismissive, remarks about a number of respected authors, works, and translations were found sufficiently offensive to incite opposition to this arrogant young man's entire project. Not a word of this opposition survives; presumably it was in conversations and letters, it appears from Italy, but enough reached Regiomontanus that he decided to answer. He did so in the preface to a work not listed in the prospectus, a dialogue on errors in the old *Theorica planetarum* attributed (incorrectly) to Gerard of Cremona, known from later printings as the *Disputations against the*

Cremonese Nonsense on the Theory of the Planets. It may have appeared in 1475, after the printing of the *Kalendarium* and *Ephemerides*. The dialogue takes place in Rome in 1464 between Regiomontanus, called *Vienensis* from the University of Vienna, and his friend Martin Ilkusch, called *Cracoviensis*, from the University of Cracow; in one manuscript that probably belonged to Ilkusch, the two are called Joannes and Martinus. The criticism of the old *Theorica* is sufficiently detailed and technical that one may wonder how many people could follow it or cared enough to do so, although since it was reprinted several times, it must have had some interested readers, and it is amusing. But the preface pretty much speaks for itself, even after more than five hundred years, and needs no further introduction.

> Preface to *Disputations against the Cremonese Nonsense*
> To all those devoted to the good arts, Johannes Regiomontanus sends many greetings.
>
> Soon after we published the list of works which we shall deliver to our publishers to be printed, we learned that some people, more inflamed with zeal to harm than to help, regarded it as worthy of censure that we endeavor to alter the works of some authors, but throw out the works of others entirely, that is, by introducing new translations, then that we criticize many approved and ancient authorities, and especially that we do not fear to consign to oblivion the commentaries of certain more recent writers, even mentioning names, which they consider rude. However, since I desire to study clear and correct exemplars, rather than either write new ones or copy faulty ones, I must by no means deny that I did this willingly and deliberately, not to disparage the authority of another, but that the study of mathematics—already debased in many places for ages and

> forsaken by almost everyone—be illuminated by removing every blemish, as far as possible, which surely requires both altering a great deal and translating anew.
>
> Moreover, advised by the example of almost all those who have ever written something new, we consider it the duty of a man of fair and honorable disposition to criticize writers, however ancient, if they have anywhere erred, as men do. Finally, it should scarcely be thought that we have not spared the names of the writers without reason, since some poor wretches, bound by excessive credulity, impute so much value to the celebrated titles of books and the antiquity of their authors that when about to dispute on any subject, they think the highest and strongest ground of demonstration must always be borrowed from an authority, evidently trusting more in any kind of assertion of another than in the most certain reasoning. The death of men who wrote something in life brings about some kind of special indulgence such that the works of those who we perhaps disregarded while they were still alive, we religiously venerate now that they are dead, either because one ought not to oppose their opinions—lest it be thought to arise from envy or pride—or because we refuse to shake off the opinions of others and distinguish more subtly, since this generally cannot be done without great labor. For this reason, therefore, I believe it has come about that many learned studies have been reduced to the semblance of some kind of dream or old wives' tale on account of extremely careless readings and obsequious interpretations.
>
> But truly, although this contagion is common to almost all liberal studies, it is nevertheless utterly shameful and intolerable in mathematics, inasmuch as mathematics, by the acknowledgment of all always displaying invariable certainty, has by the idleness of our age been boiled down to such dregs

that in astronomy (for it would be tedious to introduce all mathematics) we ignore nearly all authors except Gerard of Cremona and Johannes of Sacrobosco. And now we who have looked at their fabrications, namely, the *Theoricae planetarum* and the so-called *Sphaera materialis*, are praised as astronomers. But as soon as we have also touched upon some kind of rudiments of computations and predictions with tables, then at last we are supposed perfect in all respects!

Hence some of us are sent out to public lectures, doubtless for making students of the same sort as we ourselves are teachers. Others of us are summoned to the councils of princes; encouraged by their applause, we soon do not blush to pour forth our nonsense in public before a common throng. I am truly ashamed to reckon up how much injury will fall to our lot, for the most part from that cause, and indeed not undeservedly since, through stupid blindness, we are the publicists of our own folly. But since in fact these things are expiated by a punishment attached to them, they warrant a less severe censure than the fact that we rush indiscriminately to correct exemplars of the sciences however abstruse. For unless I am deceived in this, it is a sin to obscure the thoughts of renowned authors with the spurious contagions of our own ignorance and to infect future generations with corrupted copies of books. For who does not know that the wondrous art of printing, recently devised by our countrymen, is as harmful to men if faulty volumes of books are distributed as it is helpful when exemplars are properly corrected?

I cannot restrain myself from mentioning one example of a rather impertinent corrector who recently presented for printing by Roman publishers Strabo's *Geography* translated some time ago into Latin, although it may be more pleasing to laugh than to disclose to men in writing the impudence

of the smatterer. In book three where the duration of the longest day that occurs for those living between Rome and Naples is treated, he says the longest day is fifteen 'solstitional' hours, and there by the very frequent repetition of this epithet 'solstitional' shows his ignorance and barbarism, that is, with a single word he exposes his double ignorance to public shame. For when the Greek author says ὡρῶν ἰσημερινῶν—written by hand in Regiomontans's printing—which in Latin is 'equinoctial hours,' this fool has wondered how equinoctial hours can make up a solstitial day, since the equinox and the solstice are far apart from each other. And so, unaware in any case why hours, even those that are counted on the solstitial day, are called 'equinoctial', he named those hours from the solstice. To be sure, no one will ascribe so great an error to the translator Guarino, for earlier, not long after the beginning of book two, he discloses that the longest day among the Britons is nineteen equinoctial hours. He would not say 'solstitional' as would that grammaticaster, but would form 'solstitial' from 'solstice,' following Lucan who says 'and the solstitial head of the scorching lion' (6.337–38 *rapidique leonis solstitiale caput*).

Friend, would you entrust exemplars filled with problems and very difficult to correct to such a corrector, or rather corrupter? What will happen, I ask, if by the negligence of the translator the first exemplar is obscured by error? Or if it is falsely altered by some hungry publisher? Without question both of these are to be discerned in that work which nowadays circulates as the *Geography* of Claudius Ptolemy, where neither the literal text of the Greek author agrees with the meanings given by the perverting Jacopo d'Angelo of Florence, nor do the maps of individual regions preserve the form drawn up by Ptolemy, but they have been subjected to worthless alteration by a starving man. Therefore, anyone who believes he has Ptolemy's *Cosmography* will not even be able to exhibit a shadow of so great a work. And no one will doubt me when I say briefly that this work has not yet been translated into Latin, especially if he learns that on account of its difficulty it was also lost for a long time among the Greeks, and would have perished entirely except that it was rediscovered by the vigilance of a certain monk Maximus (Planudes). But these things will be treated more fully elsewhere.

Returning now from where I have wandered, lest in blaming the faults of others I seem to exclude myself from that ridiculous herd of astronomers, as though innocent and liable to no error, I now declare myself ready to endure fairly, indeed, ready to be immensely grateful to, nearly everyone who will examine and pass judgment upon my editions however critically. For although I am aware, through the warning of Horace and Quintilian, that these editions must not be hastened, nevertheless, I must attempt something in the prime of life lest I seem merely to gratify my stomach like cattle.

I suspect, however, that there will be some who will cast up at me the charge of arrogance, as I live in Germany, not to say like a barbarian, lacking books and distant from the concourse of learned men, and I dare to attack so many distinguished men. But, unless I am mistaken, they will grant indulgence if they consider the object of what is intended, not the person or circumstances of the writer. For in order that everyone may the more freely and thoroughly examine, judge, correct, and revise my attempts, behold, I set myself up in public of my own accord and by multifarious translating, not fearing to risk any fortune for the republic of letters. And let this present little study serve as a foretaste of all the learning that we shall pursue whatever God may bestow as the measure of our remaining life.

Finally, kind readers, we urge that each one examine our efforts according to his own capacity, not indeed without profit, unless someone prefer to despise the honorable mention of his name, which we surely promise we shall make where the opportunity arises in our works. Moreover, no small pleasure comes to the envious if they detect in error a man daring to undertake the unfamiliar!

But that we may not continue longer in this preface, let us begin to review the *Theoricae planetarum* published, it is said, by Gerard of Cremona, and for a long time now chosen to be taught in all universities, a poor work indeed, but credulously approved by many great and intelligent men. Everywhere you will run into many enthusiastic expounders of it attempting to reinforce its errors by geometrical demonstrations, who will realize how vainly they have stayed awake from seeing this dialogue—seized from our hands long ago—which we once wrote for amusement in the city of Rome. And now with it as our messenger, we greet all students of astronomy.

Unfortunately, little of Regiomontanus's plan for scientific publication came to pass. Not long after 28 July 1475, the date of his last recorded observation in Nuremberg, he journeyed to Rome, summoned by Sixtus IV, it is said, to assist in reform of the calendar. There he died at the age of forty, apparently on 6 or 8 July 1476, perhaps of plague, which was epidemic in Rome that year, although a rumor later circulated that he was poisoned by George Trebizond's sons for criticizing Trebizond's translation of the *Almagest* and its commentary, as he promised to publish in the prospectus. His death was a great loss to astronomy, to the mathematical sciences, and to scientific publishing. No one of his age understood these as well as he did, and no one could write with such proficiency and clarity. Considering his own capacity for work, including translation, and the level of productivity reached by printers in the 1470s and 1480s of two to three thousand page settings per year, there is reason to believe he could have published a substantial number of the works listed in his prospectus in the next twenty or so years. Many, including his own, were printed in the following century, some from his manuscripts; some of his own were not yet written or have been lost. And since some have not been printed to this day, his great project is still not completed.

References and Notes

The prospectus of books to be printed in Nuremberg and the preface to the *Disputations* are translated from Regiomontanus's printings. There is an edition and translation of the preface with commentary by O. Pedersen, 'The Decline and Fall of the Theorica Planetarum, Renaissance Astronomy and the Art of Printing,' *Science and History, Studies in Honor of Edward Rosen*. *Studia Copernicana* 16 (1978), 157–185, and a fine edition of both texts with analysis and detailed notes on the prospectus by Michela Malpangotto, *Regiomontano e il rinnovamento del sapere matematico e astronomico nel quattrocento*, Cacucci Editore, Bari, 2008. Only a few specific notes are added here: Regiomontanus did not complete the translation of Ptolemy's *Geography* and large commentaries, nor, it appears, his other intended translations, but extensive annotations on the errors of Jacopo d'Angelo, although only 'fragments' of his complete manuscript, were published by Willibald Pirkheimer with his own translation of the *Geography* in Strassburg in 1525. By confusion with the adjective *anaphorikos*, meaning 'relative' or 'related', Hypsicles's *Anaphorica*, 'On rising times', was thought to refer to Books 14 and 15 of Euclid's *Elements*. The *Defense of Theon of Alexandria against George Trebizond* survives in the autograph manuscript in St. Petersburg, which appears to be a draft with countless corrections, published in facsimile with a diplomatic transcription, including deletions and corrections, by Richard Kremer and

Michael Shank, on line through Dartmouth College Library Digital Publishing. Alas, Guarino of Verona, who translated Books 1–10 of Strabo's *Geography*, is probably responsible for the barbarism 'solstitional'; Gregorio Tifernate translated Books 11–17, and the first printing, edited by Giovanni Andrea Bussi, appeared in Rome about 1469. The maps to Ptolemy's *Geography* that Regiomontanus criticizes are probably those of Nicolaus Germanus, who worked in Florence, which are found in many manuscripts and soon formed the basis of early printed editions.

Kurt Møller Pedersen

A Beautiful Map Made of Driftwood

I GRADUATED from the Department of the History of the Exact Sciences at Aarhus University in 1966. Founded by Dr. Olaf Pedersen, the department had begun as a small section within the Department of Physics in the Science Faculty, and eventually comprised quite a few professors: Ole Knudsen, Bent Søren Jørgensen, Kristian Peder Moesgaard, Kurt Møller Pedersen, Kirsti Andersen, and Jesper Lützen, all of whom had studied science and mathematics. Most of the Science Faculty's and the department's graduates would become science teachers. The history of science played only a minor part in their education. Research and teaching focused on interpreting Ptolemy's *Almagest*, Copernicus's *De Revolutionibus*, Newton's *Principia*, Lagrange's *Mecanique Analitique*, and Fermat's and Roberval's writings. We invited many distinguished guest professors, and Jed Buchwald became a most inspiring, helpful, and intellectually stimulating colleague.

The department's main goal was to give our students a historical background to their science studies, and it was believed that this would be important for their careers. But the faculty members became more and more engaged in historical and philosophical aspects of science and society. One turning point was marked by Thomas Kuhn's *Structure of Scientific Revolutions*, and the subsequent discussions in the 1970s and 1980s. From that time on, philosophy and theory of science became an integral part of the department's activities.

We had a small collection of scientific instruments, and the nearby university hospital had a collection of medical instruments and a small garden with all kinds of fragrant herbs that for centuries were believed to have curative properties. All these collections were eventually housed in a new building, the Steno Museum, which opened in 1989 as part of the Department of the History of the Exact Sciences, later to be called Center for Science Studies.

Jed visited in 1979, bringing his infectious enthusiasm for the history of science. He was a most inspiring colleague and we all felt his passion for whatever he engaged in. History was not cold facts; it was conveyed to us all with his charm, humor, sense for detail, and an eagerness to discuss the subtleties of his projects. For Jed, history involved the researcher, one who was not hiding behind his writings. His articles and books showed the man behind the text. His papers and books are uniquely Jed's.

That has inspired me to write a story about US citizens on the west coast of Greenland in the 1920s; a story that brings us from Aarhus via Copenhagen to Godhavn, in the Disko Bay of Greenland, and to Washington D.C.

The Steno Museum owns a seal skin map, with pieces of driftwood sewn onto it, received from the Danish Geodetic Agency in Copenhagen.

I don't know whether Jed ever saw this impressive, 150 x 100 cm large object, since it was kept in the museum's basement and never exhibited. We were told that it was made in 1925 by Silas Sandgreen, an Eskimo hunter and member of the local municipal council in a small settlement near Godhavn, on the West Coast of Greenland. As a hunter, Sandgreen was familiar with the islands, hunting places, fjords, and mountains which he saw from his kayak. He was a respected and well known person in the community, able to roll his kayak, and how to carve wood with a knife, as we can see from his map of the islands in the Disko Bay.

How and why did that map come to Copenhagen? When searching for Silas Sandgreen, Google produced quite surprising information: It showed a map held at the Library of Congress's Division of Maps that was identical to the one at the Steno Museum and was described as follows:[1]

> Silas Sandgreen, an Eskimo hunter, was commissioned by the Library of Congress to prepare the map in 1925, through

> the good offices of the Secretary of the Navy, as well as Commander R. E. Byrd, Mr. Philip Rosendahl, Administrator of North Greenland, and Dr. M. P. Porsild, Chief of the Danish Arctic Station at Disco.
>
> The base of the map is made of skin. The representations of the islands are made of driftwood from Siberia, colored to indicate the extent of grassy and swampy ground and of grass covered with black lichens. Uncolored wood shows the area reached by the tide.
>
> The area mapped is approximately seventy square miles.

1. "Relief map of the Crown Prince Islands, Disco Bay, Greenland by Silas Sandgreen, 1925–26." *Noteworthy Maps No. 2, Accessions 1926–27.* Compiled by Lawrence Martin and Clara Egli, 1927, p. 24.

In the summer of 2017 the map lay before me on a huge desk in the Library of Congress.

Now, with two almost identical maps made by Silas Sandgreen, other players entered the scene: The Secretary of the U.S. Navy Curtis D. Wilbur and Lt. Commander Richard E. Byrd. The Archives of the University of Greenland contain the copy of a letter from Herbert Putman, Librarian of the Library of Congress, to Wilbur:[2]

> June 5, 1925
> Sir:
> In connection with the MacMillan Arctic Expedition carried on in cooperation of the United States Navy, the Chief of the Division of Maps of the Library of Congress is anxious that I should attempt, through you, to secure one or more maps made by the Eskimos.
>
> If the project interest you, would you be willing to transmit this letter to Commander R. E. Byrd, U.S.-Navy?
>
> It would be admirable if Commander Byrd might secure for the Library of Congress some old map made by an Eskimo, preferably a map on a skin.

The Macmillan Arctic Expedition was led by a veteran explorer, Commander Donald B. Macmillan, who had followed Robert E. Peary almost to the top of the Earth in 1909, and had since explored Greenland over many years. The 1925 expedition consisted of two U.S. Navy ships, the USS *Bowdoin* and the larger USS *Peary* which, with its 600-horsepower triple-expansion engines, nine-foot propeller, and triple-plated bow was indeed a good ice ship. It carried three airplanes

2. "To the Honorable the Secretary of the Navy. June 5, 1925." *Library of Congress Washington anmoder om Landkort tegnede af Eskimoer.* Greenland National Archives. Filed in: Library of Congress Washington anmoder om Landkort tegnede af Eskimoer.

NA1, 2, and 3 (for Navy Aircraft), with Byrd of the Bureau of Aeronautics as chief of the expedition's aviation section.

The expedition was sponsored by the National Geographic Society and its million members, and was meant to explore the unknown Arctic land and sea between Alaska and the North Pole. The plan was to settle for the summer in Etah, 11½ degrees from the North Pole. Byrd was hoping that, by establishing his base in Etah, he would become the first person to fly over the North Pole.

On the outbound journey it was planned that the expedition should find a coal supply for the USS *Peary*—a real coal burner—somewhere on the west coast of Greenland. On 16 July 1925 the *Peary* arrived at the port of Godhavn, where the Governor of Northern Greenland resided. The town was also the center for scientific research led by Morten Porsild, director of the Danish Arctic Station since its establishment in 1906. When the *Peary* arrived at Godhavn, Captain E. F. McDonald Jr. asked Governor Philip R. Rosendahl for a coal supply, but was told that no coal could be spared.[3] This could have spelled the end of the expedition, but McDonald persisted. Through wireless communication with Washington, however, the good offices of the Danish ambassador to the U.S., Constantin Brun, were enlisted, and by the time of the *Bowdoin*'s arrival a week later, plentiful coal had been promised at Umanak. Rosendahl volunteered to accompany the *Peary* to the coal depot, 180 miles north, and personally oversee the

3. "The MacMillan Arctic Expedition Returns. U.S.Navy Planes Make First Series of Overland Flights in the Arctic and National Geographic Society Staff Obtains Valuable Data and Specimens for Scientific Study." *The National Geographic Magazine.* 48 (5) November 1925: 477–518, p. 497.

loading. MacMillan described the event in sober and appealing prose, but Byrd was much more outspoken in his diary:[4]

> Thursday, July 16, 1925
> Arrived Godhavn, Disco this morning 5:30. The local and district Danish governor came aboard early and gave us the startling information that we can get not a single ton of coal here. We haven't enough coal to get up to Etah and back here. There seems to be no coal on the Greenland coast. It looks as if the expedition is ruined but we'll get that coal somehow. The governor admits that he has coal and is mining it at the other end of the island but when winter comes he will have just enough for the eskimos here and in surrounding villages.
>
> On top of this no one is allowed to go the village (about 150 eskimos) because the eskimos have an epidemic of whooping cough. The governor says he is afraid we will carry the disease north and give it to the Etah eskimos. I tried to get the governor to have some laundry done for me but he said it couldn't be done. There has been no evidence what so ever of any hospitality.

Photograph by
Maynard Owen Williams,
Etah 1925

4. Byrd, Richard Evelyn, *To the Pole. The Diary and Notebook of Richard E. Byrd, 1925–1927.* Edited by Raimund Goerler. Ohio State University, 1998. p. 8.

More than 30,000 words of news dispatches alone were sent by the Bowdoin to the National Geographic Society, which released them day and night.[5]

It was not until Monday, July 27 that the expedition got underway at 4:20 in the morning. During the 10 days at Godhavn, the crew was not allowed to visit the village. There were, however, some contacts between the ships and the authorities in the village, mainly about the coal supply. Byrd had a letter from the Librarian of the Library of Congress, Herbert Putnam, to the Secretary of the Navy, dated June 5, 1925, which he wished to show to local people in order to secure maps made by eskimos for the library.[6]

> Sir:
> In connection with the MacMillian Arctic Expedition, carried on in cooperation of the United States Navy, the Chief of the Division of Maps of the Library of Congress is anxious that I should attempt, through you, to secure one or more maps made by the Eskimos.
>
> If the project interests you, would you be willing to transmit this letter to Commander R. E. Byrd, U.S. Navy?
>
> It would be admirable if Commander Byrd might secure for the Library of Congress some old map made by an Eskimo, preferably a map on a skin. I fear, however, that there are none.
>
> Failing this it seems to me that it might be possible for him to arrange to have an Eskimo draw, freehand, in pencil, on a piece of white man's paper, his own representation of an island, a peninsula, or other body of land near the Eskimo's home or his hunting grounds, or near the base where the U.S.S. Peary lies for a time.

5. See note 3, p. 491.

6. See note 2.

> If the map showed the coast, a conspicuous mountain, the edge of an ice cap or glacier, a winter village site, a summer hunting ground, or a habitual route of sledge- or Kayak-travel, so much the better.
>
> Commander Byrd might, perhaps, learn the Eskimo names of a village, a mountain, a conspicuous cape, or bay, or an adjacent tribe and write them on the Eskimo's map, together with the name of the native who drew the map.
>
> Perhaps more than one Eskimo might be persuaded to draw maps of the same area.
>
> The Library of Congress would, naturally, prefer a map of part of Grinnell Land or some portion of Ellsmere Island, largely explored by Americans, or of the part of northern Greenland explored by Peary rather than a map of a portion of Axel Heiberg Land and the adjacent islands whose first white explorers were probably Sverdrup and his Norwegian companions. This, however, is not essential.
>
> Commander Byrd might even be willing, once the map was secured, and if his regular work were not interfered with, to have airplane photographs of the same area taken from low levels so that a more accurate map could be compiled, after the return to the United States, for comparison with one made by an Eskimo.
>
> This project, wholly or in part, appears to be feasible; I hope you will feel willing to suggest it to Commander Byrd.

Although upset with Rosendahl regarding the difficulties surrounding the coal supply, Byrd nevertheless forwarded this letter:[7]

7. Rosendahl received the letter on July 22. Byrd, Richard Evelyn. Letter from Commander Byrd to Governor Rosendah, July 22, 1925. In *Greenland National Archives.* Filed in: Library of Congress Washington anmoder om Landkort tegnede af Eskimoer.

S. S. PEARY
My dear Governor:
I am enclosing a copy of a letter from Librarian of our Congress which has been forwarded to me by the Secretary of the Navy.

Will you please do me the great favor to procure an Eskimo chart for Doctor Putnam. If you can do so. Cordially yours

R. E. Byrd.,
Lieut. Comdr., U. S. Navy.,
Commanding Naval Arctic Unit.

This letter was then given to the Director of the Danish Arctic Station, Morten Porsild, known for his research in the natural sciences but not at all knowledgeable in geographical surveying. It told him that the Library of Congress wanted an eskimo to produce the map, as Putnam who was very much interested in the cultural activities of people living in remote places. Such a map by an amateur surveyor would not be a significant tool for guiding the American Navy to remote places on Earth. Byrd was an expert in navigation with modern equipment, and MacMillan on the *Bowdoin* and MacDonald on the *Peary* knew the waters from earlier expeditions to North Greenland.

Porsild knew someone who could do the job, Silas Sandgreen. However, what Putnam had in mind was a map drawn with pencil or pen on a skin. But what Sandgreen produced was something completely different. Pencils were not common, but he could do almost the same thing with his knife and pieces of driftwood. Sandgreen spent the next year traveling around the islands in Disko Bay and his resulting map was

sent to Washington via Copenhagen, accompanied by a letter from the Governor:[8]

August 1, 1926
Library of Congress,
Washington, U.S.A.

Sirs
On account of a request from R. E. Byrd, Liut. Comdr. U.S. Commanding Navy Arctic Unit, the undersigned has let perform a map in wood and skin, such as it was wished by the Library of Congress by a letter of 1925 June 5' to the Secretary of the Navy.

The map has been made by the Greenlander Silas Sandgreen, aged 40 years, hunter and member of the municipal council and it represents the domicile Kronprinsens Ejland in the Bay of Disco. The man was recommended to me by the chief of "Den danske arktiske Station" on Disco Dr. M. P. Porsild, a man well suited to the performance of this work. The map is no doubt made with genius and with all the care, which a layman may develop. So he has by sledge and Kayak repeating times frequented such islands as are not visited in usual in that purpose also to get these localities ~~allright~~ exactly. So he has indeed laid 83 islands and 10 rocks on the map, while you in the best maps made by Europeans ~~only~~ not find ~~40 islands and 5 rocks~~ so many.

The black colour indicates that the country is covered with a shallow, while the yellow colour represents grass and swampy ground, the blue indicates lakes and uncoloured

8. Rosendahl, Philip R., Letter to The Library of Congress, Washington, USA July 8, 1926. In Greenland National Archives. Filed in: Library of Congress Washington anmoder om Landkort tegnede af Eskimoer.

places indicates that they are overflowed by the sea. The rocks are indicated by pencilmarks on the skin.

All the islands are made by driftwood, which comes from Siberia and by the sea has been thrown up on the coast of the man's domicile. All has been performed after the man's own idea without influence of any Europeans. Thereby the man has had no admittance to any map ~~else,~~ and in this way the map only depend on his own observations.

Of tools he has got a transferring paper for transferring that of himself made and used, but somewhat dirty preliminary drawing on a white clean paper, so that he on this should be able to write the names of every place, there ~~has such~~ has got a such.

The translator of the office has arranged the place names in alphabetic order on a special appendix and translated those, as they besides being the name of a locality also give a description of the place.

Hoping that the forwarded work may be of value for the library,

I am, Sirs, Yours truly

In his Annual Report, the librarian acknowledged receipt of the map.[9]

The Library also acquired a relief map of the Crown Prince Islands in Disko Bay on the west coast of Greenland, also made by Sandgreen. He was commissioned by the Library of Congress to prepare this map in 1925, through the good office of the Secretary of the Navy, as well as of Byrd, Rosendahl, and Porsild.

This aboriginal map is remarkable because Sandgreen had no opportunity of seeing any other maps, and received no

9. Library of Congress, *Annual Report of the Chief of the Division of Maps for the Fiscal Year ending June 30, 1927, submitted July 30, 1927.*

European assistance. Relying wholly upon his own observations near his home in the Crown Prince Islands, and after repeated visits by sledge or kayak for the purpose of precisely locating these remote islands, Sandgreen mapped 83 islands and ten reefs. The best Danish chart, entitled "Nordvestkysten af Grönland, fra 66° 30' til 74° 45′ N. brede, udgivet af det kongelige sökaart-Archiv," and published at Copenhagen in 1888, shows only 38 islands.

On this sealskin relief map, Sandgreen's models of individual islands are whittled out of Siberian driftwood. They are sewn on the sealskin with thongs, and then painted. The yellow color on the islands represents grassy and swampy ground; the blue indicates lakes; and the areas colored in black show the extent of land covered with black lichens. The area covered by the tides is left without color. Reefs are indicated by pencil marks on the skin. The area mapped is approximately 70 square miles. The scale of the map is 1 inch to about 1760 feet. These figures are based on rough computations from the 1925 edition of *British Admiralty chart No. 276*, which shows in detail several of the Crown Prince Islands.

With this relief map, through the courtesy of Constantin Brun, the Library of Congress also received an outline of the 83 islands charted by Sandgreen, and lists of their Eskimo names, with translations of the meaning of these geographical names into Danish and English. The report purports the American fascination of the primitive nature of Inuit lives.[10] Portable wooden maps, however, were used by Inuit for many years. Sandgreen's map represents a historical tradition of Inuit cartography. Such wooden maps were collected by the

10. Onion, Rebecca. "A Beautiful Driftwood-and-Sealskin Map. Carved by an Inuit Hunter in 1925." *Vault. Historical Treasures, Oddities and Delights,* 2014.

Danish explorer Gustav Holm during his expedition along the East Coast of Greenland in the 1880s.

Returning to MacMillan's and Byrd's expedition of 1925: the former found it very successful, the latter was somehow disappointed. Byrd was very much upset by the delays caused by difficulties in obtaining coal, by accidents with the air planes, fire on board the ships, and problems with forcing the ships through the ice. He had hoped to fly from Etah to the North Pole, which he succeeded in doing a year later by flying from Spitsbergen, Norway, with the explorer Robert Bartlett.

I am now working with my wife Jette Rygaard on a project that involves Americans in Greenland in the 1920s and 1930s. We wish to examine more closely Macmillan's and Byrd's achievements during their expedition. To this end, Jette follows in the footsteps of the painter and writer Rockwell Kent, who for several years in the 1930s lived on the West Coast and was acquainted with several other explorers, among them Knud Rasmussen, who travelled with his group from Greenland to Alaska on a sledge.

I hope Jed will find that interesting. Jed explored the double refraction of Iceland Spar that was used by the Viking as a navigating device, a Sun compass, when voyaging from Norway to Greenland. Byrd also used a Sun compass when exploring Greenland and crossing the North Pole.[11] Jette's and my projects will explore Americans in Greenland.

11. Byrd, Richard Evelyn. "Flying Over the Arctic." *The National Geographic Magazine.* 48 (5) November 1925: 519–532, p. 520.

Jesper Lützen

Contact-connections and Actions at a Distance

During the second half of the 19th century, physicists disagreed about the nature of the fundamental physical interactions. Many continental physicists agreed with Laplace that actions at a distance could explain all physical phenomena. Most British physicists, on the other hand, tried to avoid such explanations. In their opinion, apparent actions at a distance could be explained by contact actions in a field carried by a mechanic medium called the ether. Electromagnetic theory was the primary battlefield for the two competing views: Weber advanced a theory based on elementary forces acting at a distance between moving charged particles, whereas Faraday and Maxwell developed their field theories.

Despite being German, Heinrich Hertz sided with the British. Considering his experiments with electromagnetic waves as a verification of Maxwell's theory, he spent the last years of his short life composing a book on the principles of mechanics that he hoped would eventually function as the foundation of all physical theories, including electromagnetism. In his new image of mechanical systems, Hertz discarded forces acting at a distance as a fundamental concept. Instead, he tried to explain all interactions as a result of rigid connections between mass points, some of which being hidden in the sense that they are not directly observable by our senses.[1]

In social systems, as for example the scientific community or the community of historians of science, we may also distinguish between actions at a distance and contact actions. Interactions between scientists or scholars may happen through publications. They act at a distance. Interactions may also take place during personal encounters between two or more scholars or scientists (contact actions) or through correspondence (a middle form). Personal experience, discussions around the lunch table, and the reading of (auto)biographies have convinced me that social direct contact interactions are usually stronger than actions at a distance. To be sure scientists and historians do learn from reading each other's published works, but such distance actions are usually initiated by and combined with personal interactions in the laboratory, at the blackboard, in the coffee room, or at the least through correspondence.

This poses a problem for the historian. History is based on written sources, but the strong oral interactions between actors are only indirectly traceable in written form. Published works give an impression of the scientific knowledge at a particular time, but there are many examples where published ideas were not communicated to the scientific community at large. For example, Caspar Wessel published the geometric interpretation of complex numbers in 1798–1799, but the work was overlooked until French and German mathematicians published similar ideas about ten years later. Here the explanation for the neglect could be that Wessel's paper was published in an obscure language (Danish). Yet examples abound of results published in leading journals and in the primary scientific languages of the time that were nevertheless ignored: In a paper written in French in *Liouville's Journal* in 1837, Pierre Wantzel proved that the duplication of a cube and the trisection of an angle could not be constructed with ruler and compass. In modern accounts of the history of mathematics, we recognize this as the solution of a two millennia old problem, and yet Wantzel's paper remained virtually unknown for a century. Similarly, Nikolai Lobachevsky published his ideas on non-Euclidean geometry in French in *Crelle's Journal* (1837) and then in German in a separate booklet (1840), but the new ideas did not become general knowledge until 20 years later, when Jules Hoüel began corresponding with Italian geometers about the matter.

1. Jesper Lützen, *Mechanistic Images in Geometric Form. Heinrich Hertz's Principles of Mechanics.* Oxford University Press, 2005.

The last example illustrates the important role played by scientific correspondence. Being more personal than published works, correspondence will often contain more information about value judgments and heuristic ideas than publications aimed for a greater audience. More importantly for the historian, exchanges of letters will often reveal the process of discovery that is mostly hidden in published papers and books. If I am right claiming that personal encounters give rise to even stronger interactions than correspondence, we are left in a somewhat tricky situation. If scientists or scholars work together at the same place, their interactions are strong, but the historians have no way of knowing the details of the interaction. If, on the other hand, collaborators live far away from each other, the interactions are probably weaker, but the historian has a chance of knowing about it through their surviving correspondence.

For example, Archimedes might have had a stronger influence on his contemporaries had he lived in Alexandria, the Western world's scientific center at the time. And yet we would most likely have known less about his ideas than we do now, since we can read his letters to his Alexandrian colleagues. Something similar can be said of Pierre de Fermat, who had to communicate in writing with the Parisian mathematicians in order to establish that "the truth is the same in Toulouse and in Paris."

In former times there were fewer centers of science where many scientists could collaborate face to face about a specific theme. However, in revolutionary and post-revolutionary Paris we begin to see the phenomenon of joint papers where the collaboration between the authors is lost to the historian in oral communication. For example, we can only guess about the interactions between Jacques Sturm and Joseph Liouville that led to the first joint mathematics papers concerning the theory called after the two friends. Something similar can be said about the collaboration between the mathematician Hieronymus Zeuthen and the philologist Johan Heiberg on the history of Greek mathematics. A few letters between them have survived, but since they lived in the same town and met every second week at the Royal Danish Academy, there is little doubt that they collaborated more than the written record suggests.

When we get closer to our own time, the issue of oral interactions becomes even more of a problem for the historian. Recently I co-authored a paper about the significant role played by Harald Bohr in the dissemination of Laurent Schwartz's theory of distributions. The most important episodes in this history were two encounters: first, at a meeting on harmonic analysis in Nancy, where Bohr met Schwartz for the first time; and then, Schwartz's subsequent visit to Copenhagen. However, our knowledge of the story comes from letters written in the period between these two encounters. This skews our story and accords disproportionate importance to a period that was not as significant as the personal encounters. I suspect that many other historical analyses are similarly skewed by our lack of direct written evidence of oral interactions between scientists.

Let me illustrate the importance of personal oral interactions by returning to Hertz. How did he get convinced that the British field theory was superior to Wilhelm Weber's action at a distance theory? As pointed out by Jed Buchwald, it was Hertz's mentor Hermann von Helmholtz who got him interested in finding experimental evidence for or against the two theories. And where had Helmholtz learned about the British field theories? From his visit to Britain in 1853, where he befriended William Thomson, one of the most outspoken proponents of the mechanistic world view favoring contact actions. Thus, Hertz's preference for contact connections was itself a result of personal connections to the British physicists themselves.

Finally, how did I become interested in Heinrich Hertz's *Principles of Mechanics*? In connection with my work on the geometrization of mechanics, I studied the relevant aspects of Hertz's book. However, I would never have begun a thorough historical analysis of Hertz's novel image of mechanical systems had it not been for the direct oral interaction with Jed.

I first met Jed when he visited Aarhus while I was a PhD student. After I came to Copenhagen, I wanted to persuade the physicists that they ought to hire a historian of physics to teach the subject to their students. I wanted to show them how interesting such a course could be, so I turned to Jed. Fortunately he agreed to come for some months in 1988. Two years later (1990–1991), my wife Tinne Hoff Kjeldsen and I visited Jed in Toronto, where our first daughter was born. For the next 17 years we visited Jed about every four years: The spring of 1994 and the spring of 1998 at the Dibner Institute, and the academic years 2001–2002 and 2006–2007 at Caltech. These stays abroad have been the most memorable of our lives and they have had a profound influence on our work.

When we came to the Dibner Institute for the first time, Jed had just published his book on Hertz (*Buchwald 1994a*), and so we began to discuss how Hertz's geometric ideas fitted in with his other ideas on physics. These discussions convinced me that my next major research should deal with Hertz's book *Prinzipien der Mechanik* (1894). Philosophers had addressed the philosophical introduction to the book, and historians of physics had to a lesser degree written about the physical ideas. But no one had dealt with the mathematical geometric form in which Hertz chose to formulate his new image of mechanical physics despite the fact that, according to Hertz, this was the most important part of the work, and the part that took him most time to develop.

Of course, I might have learned of Jed's ideas on Hertz through his book, but such a distant action would not have caught my interest the way the direct discussions with Jed did. To me this nicely illustrates that, as in Hertz's mechanics, direct connections are more important in the academic exchange of ideas than actions at a distance. A discussion at the blackboard has a much greater impact on the participants than a study of published material; and in addition, it is usually a much nicer social experience.

I am grateful for the many occasions I have had to interact directly with Jed. A discussion or a chat with Jed is never boring whether the subject is history of science, American politics, or how to leave a restaurant in the most dignified way after our children have messed it up completely.

Jeremy Gray

Archive for History of Exact Sciences

The history of exact sciences, as a journal, explicitly includes mathematics as one of its key disciplines, as it must when it aims at illuminating the conceptual groundwork of the sciences and the course of rigorous quantitative thought, and its standards are to be those of the mathematical sciences. Truesdell and his trusted associates made sure that the history of mathematics was included from the start, and since then Henk Bos, and many others here in body or spirit, have done the same: Alexander Jones, Niccolò Guicciardini, Noel Swerlow, Jesper Lützen, Umberto Bottazzini, and Bernard Vitrac. And of course other editors and friends have contributed heavily mathematical essays, notably in the field of mathematical physics, and others past and present have also done so across a range of periods: Len Berggren, Craig Fraser, Menso Folkerts, Moti Feingold, Karine Chemla.

But it can seem that the history of exact sciences, as a topic, in many places in the United States and the United Kingdom, excludes the history of mathematics, and that this has been the case for some time. Moreover, the kinds of topics that ISIS, for example, includes that belong to the history of mathematics are heavily at the social historical end of the spectrum: institutions,

pedagogy, biography, etc. But it would be easy to exaggerate: the pages of *Science in Context*, for example, are much more hospitable to historical articles with a mathematical core. And it must be said that history of science is an immense field, and no one historian or single coherent group of historians can be expected to cover every relevant topic in every period in every place.

The divorce between history of mathematics and history of science is most acute and most obvious in the modern period, and on this occasion it might be appropriate to indicate how things look in the history of modern mathematics, and to reflect back over the period of the journal, which happily coincides with the working lives of those of us now old enough not to have trustworthy memories. So most of what I have to say for the next few pages will be based on re-consulting the archives.

The Archive for History of Exact Sciences, as we all know, was founded in 1966. *Historia Mathematica*, the house journal of the International Commission for the History of Mathematics, was first published in 1974, and the French *Revue d'Histoire de Mathématiques* (RHM) began in 1995. For a time, Truesdell relied on his trusted but elderly mathematicians, but with the arrival of Tom Whiteside, and then of Henk Bos, he had professional historians of mathematics to whom he could turn, and he did. The late 1970s saw another change, the emergence of a new generation in Europe who wanted to be historians of mathematics, and saw a chance to pursue their careers in the expanding university world of the time. Of course, this generation was numerically quite small, and it was unusually fortunate because mathematics departments frequently proved hospitable. The RHM marks another aspect of this shift: the formal involvement of national mathematical societies in publishing work on the history of the subject. The American Mathematical Society, jointly for a time with the London Mathematical Society, has since 1988 run a book series on the subject which also illustrates the same process.

The scholars that emerged in the late 1970s and 1980s were sometimes self-taught as historians, sometimes the product of the more sympathetic history of science departments, and they have travelled in their work, and indeed in person, to the lands of Islam, to China, and to other places. Those who settled on the modern period achieved a great deal. The long 19th century is the period when a great deal of modern mathematics was created: rigorous analysis, modern algebra and number theory, and the various forms of modern geometry; many of the names of mathematicians that students learn are those of 19th century people. One way or another many of those topics have received fresh, detailed accounts, many of the famous mathematicians now have rich biographies. A lot of this filled the pages of *Archive*, a great deal of it fits the prescription Truesdell laid down, but, rightly, not all.

Much of this work, inevitably, is arranged to appeal to the host body of its authors, which is the mathematical community, and while this has been very advantageous in numerous ways, it has not been entirely without some negative effects. It has been hard to work on the 18th century, the century of the Bernoullis, Euler, d'Alembert, and Lagrange, perhaps because mathematicians do not find it so interesting. It is notable that the recent surge of interest in Euler, which has led to many translations of his work and to a fine new biography of him, is the collective work of the Euler Archive, a largely American group of historically-minded established mathematics professors. And being mostly in the mathematics community has encouraged authors to address topics that may not seem accessible to people with little appreciation of mathematics—a group one can suspect contains some historians of science.

To speak personally, but I think that all those who work on the history of modern mathematics would agree, I was attracted to the 19th century because it was a coherent period. From the perspective of someone in, say, 1974, this long century ended only 60 years before, a length of time that is just long enough to act as a buffer and allow the historian rather than, say, the sociologist, to proceed.

But what if we look back 60 years from today? What do we see if we ask for a history of mathematics stretching from, perhaps, 1918 to 1958? We shouldn't insist on the precise choice of year, we might surely want to retreat a little to, say, 1900. But what are we to make of the history of mathematics in the first half of the 20th century? I think we should have to agree that it is a challenge, and that much is to be done.

I don't think the difficulty of the mathematics can be a defense. It would be impossible to stand in one of the centers of research on Einstein and expect to be heard making such a plea, whatever Einstein scholars might say still has to be done; nor does the comparison with the history of quantum mechanics offer comfort. These tasks have been taken up, intellectual difficulties and archival difficulties notwithstanding.

What you notice in the history of 20th century mathematics is the arrival of a recognizable form of modernism. Mathematics became autonomous in its foundations and its methods, much less naïve and intuitive, much more formal and abstract. Its relations with science had to be renegotiated, and at times were surprisingly distant. Historians have begun to grapple with this, but they have concentrated on the build-up. There is much less on the aftermath, and the sheer growth in the numbers of active mathematicians is daunting.

Then there is the geographical growth of this characteristically modern mathematics. What had been a predominantly European subject (German, French, and Italian in the main) became more international. There were major Japanese mathematicians, more Russians than before, the first American mathematicians of international standing, Poles, and even British mathematicians of note.

All this comes with significant institutional complications. The history of the Soviet Union complicates the picture; there are language issues; and archival issues. And of course there are the two world wars, and their many ramifications.

Much has been done. We have accounts of, for example, aeronautics and mathematics, of mathematics in the First World War and again in the Nazi time; we have biographies of Gödel, Robinson, and now of Hausdorff. The conceptual, rigorous quantitative side is less well analyzed. We have of an account of the emergence of a rigorous mathematical theory of probability, and forays into certain other topics, a major study of Emmy Noether's work is in the pipeline to go with a recent biography (by a different author). But if you took the syllabuses of good mathematics departments and sifted them for a guide to what 20th century mathematics consisted of, and then asked for the historical treatments of the resulting topics, you would come up with an agenda, not a reading list. Or perhaps you would come up with the sort of surveys of the field that distinguished, elderly mathematicians have written. There is much historical work to be done.

How might this come about? We could wait for fifty years, by which time the mathematics of the first half of the 20th century will be much more familiar. Those of us for whom this remedy is perhaps out of reach might hope for fresh methodological approaches that will cope with the problems of size. There have been moves to articulate a philosophy of mathematical practice—what it is that mathematicians do as they discover mathematics—that might well become a fruitful way to break any tendency to produce just another list of 'greatest hits.' It might also invigorate the philosophy of mathematics, currently, and rightly in my opinion, a topic of bewildered lack of interest among mathematicians, although worryingly its practitioners seem not to care. An initiative along the lines of the Euler project could be another way forward. But we must also hope that there will be individual scholars wanting to take on the tasks involved, and it must be said that the situation is strong in France, but only in France. The reason for this is the shifting currents of academic interest and our abilities to navigate it. It is likely that the work will have to be done in mathematics departments or similar centers.

For the present we can be glad that Truesdell defined the history of exact sciences so carefully and created such a durable vehicle for its dissemination, one that Jed has now so capably driven forward for many years. Jed has been one of the few historians of science to take the history of mathematics seriously, in his own work and in the work of others, as is exemplified not only by *Archive for History of Exact Sciences* under his leadership but also by the many other books and series with which he has been associated. Jed, you have built a community of historians around the *Archive*, we are all in your debt, and may I say that it is a particular pleasure for me to thank you, along with all of you.

Sharon Kingsland

Becoming a Historian of Science: Reminiscences of Graduate School in Toronto

On this happy occasion I would like to reflect briefly on the graduate education that I received at the University of Toronto in the 1970s, when Jed was an assistant professor and later associate professor at the Institute for the History and Philosophy of Science and Technology. I joined that program in 1975. As an undergraduate biology major, I had become interested in the subject after reading Thomas Kuhn and Herbert Butterfield. Otherwise I was relatively ignorant of the history of science, and of what it meant to be a historian. Jed, along with Trevor Levere and Polly Winsor, were the most important influences in helping me to understand what historical research was all about.

We all were expected to take courses in subjects outside our area of interest, which is how I ended up taking a course in the history of physics and astronomy in my first year. The first term was taught by Stillman Drake, whose discourses on Galileo were spell-binding. Jed taught the second term, starting with Newton and going to about the end of the 19th century. To me, in that first year, Jed's course was intimidating and even terrifying: there was a lot of physics, and I didn't know much at all about physics. And it was drilled into us that we really needed to understand the science.

Jed used to hold his classes in his office, and I remember that he had a piece of Icelandic spar on his desk, which he was using to work out the science behind the discovery and analysis of double refraction, or polarized light. For many weeks we struggled in class with difficult subjects of that type, but we were given to understand that this struggle was the essence of historical work. It was not about learning what some other historian had discovered and put together. It was about your brain struggling to understand a difficult text, in order to work out what some past scientist had done and thought.

Even a historian of biology was expected to know about the history of astronomy, physics, and chemistry, and in the end I appreciated having that background knowledge. When I finally got to my dissertation topic, which was on the history of population ecology, I found I really did need to know something about the Carnot cycle, thermodynamics, and late 19th century physical sciences, and the readings I did for that course turned out to be very helpful.

The common starting point for us, whether in history of physical sciences or history of biology, was Aristotle, so we all gained a broad understanding of the history of science from antiquity, and often just from reading primary sources. We did relatively little reading in the secondary literature, and were not encouraged to take up a theoretical position of any type. The lack of interest in theory at Toronto contrasted greatly with the situation I found at the University of Montreal, where I spent a term as an exchange student. At that time Foucault was the rage in Montreal, and one was made to feel deficient if one had not read at least one of Foucault's books.

But in Toronto there was none of that. We were placed in as close contact with primary sources as we could be. Nothing theoretical, and very little that was historiographical, came between us and the primary sources. The main obstacle was language, and we did rely on English translations of ancient texts, but otherwise we were expected to be capable of handling texts in German and French. The other obstacle was mathematics, but it was always possible to find a research topic that was not heavy on math, and in Jed's course I found that topic in 18th century theories of heat. We would generate research topics by finding a natural philosopher or scientist to write about, locate that person's texts, sit down with the texts and figure out what the arguments were about. And even though we lacked extensive knowledge of the secondary literature in our field, we were asked to come up with enough analysis to cover twenty or so pages, and at least try to offer our own interpretation of the subject.

At that time I took this approach for granted and did not realize what it meant for my development as a historian. It was only much later that I came to see how valuable it was to be taught to swim by being thrown into the deep end of the pool. Since our professors expected us to do research, we accepted the idea that we could, in fact, do research. We could sit down with a text we had never seen before, and after looking at it long enough, we could find something interesting to say about it. What we said might be naïve, for after all, we didn't know anything. But we gained a certain confidence in our ability to engage with and interpret a text and really come to grips with what the science was about.

After a few days in this kind of work, one began to notice things, make connections, and even make little discoveries, perhaps even finding that an important authority on this subject had made an error. One saw historical analysis as a process of discovery and of gaining new insights into old science. It was exciting. That sense of excitement in turn built up one's confidence. It made you feel that you *could* discover new things, if only you could figure out what was going on in that text. It made you feel that if you stayed with something obscure for long enough, that obscurity would give way to clarity. It made you feel that you could discover new things in almost any text you picked up, and that there were hundreds of potential topics out there waiting to be analyzed.

And it tended to make us understand that the object of a lot of science was to advance human understanding of the world, and that it was pretty exciting to figure out how scientists in their different ways made these advances. We never lost sight of the fact that scientific work is a creative intellectual process.

This idea, which may seem obvious, is missing from a lot of the secondary literature today that has been influenced by the sociology of science. In the 1970s we weren't exposed very much to the sociology of science, outside the works of Robert Merton. We paid less attention to the social relations of science, although they were not entirely absent. But starting around the early 1980s, sociologists of science tried hard to redirect our attention to scientific practices and social relations, often ignoring the intellectual process of science. They told us what the scientists were doing, but had less to say about what they were thinking. Studying scientific practices is important, but leaving out the brainwork of science leaves us with a strangely lopsided view. Many students these days are trained in programs with a heavy emphasis on sociology. The result is that they may find it hard, even impossible, to engage with scientific ideas. At Toronto there was no risk that we would neglect the scientific brainwork.

One belief that was common in our field in the 1970s, but which I never encountered in Toronto, was that it was not smart to delve into new and unknown topics. One feature of the Toronto program that I remember with great fondness, and which was communicated to us by the entire faculty, including Jed, was the idea that a student did not have to make her mark by studying only the "great men of science," which

in my field would have been Darwin. We were never told to steer clear of a topic because no one had ever written about it, or because it would be risky for us, as young scholars, to venture into a completely unknown field of research. We were never warned that we should only work on topics where there was an established literature, and where our role would be to mop up around the edges. We were never made to feel that we should seek the security of a large community of people working on the same topic, the people who would referee our articles, review our books, and supply our tenure letters. Students elsewhere received advice of this kind, but at Toronto no one ever said "don't waste your time on subjects that no one knows about." There was no pressure to make us conform and be like everyone else.

And so I wrote a dissertation on a topic that no one had heard of, but I was fairly sure that ecologists would be interested in what I wrote, and that an audience was out there. The topic was only unknown to historians of science. Still, I was a bit taken aback when I came to Johns Hopkins for my first job in 1981, and the chair of my department told me that it would be fine for me not to publish my dissertation, and that I should feel free to pick another topic for my first book. He was clearly nervous on my behalf, for he did not know anything about my subject. But by that time I had a book contract, and did publish it, and so I have felt confident in encouraging my own students to explore new topics. I am extremely grateful to the Toronto faculty for instilling in us the confidence to be adventurous.

Best wishes on your 70th birthday, and many happy returns.

Craig Fraser

The Equation Editor

When I arrived at the University of Toronto in the mid-1970s to do doctoral work on the history of mathematics, I enrolled in Jed's graduate seminar on the history of physics. Jed's seminar was known for the close study of major developments in past physics. In the year when I took it, a subject of study was Norton Wise's newly minted Princeton dissertation on the flow analogy in the origins of the theory of electromagnetism. This subject was difficult for two reasons: first, some effort was required to get a grasp of the phenomenon, and second, the mathematical underpinnings of the theory were not straightforward. I would like to quote the opening sentence of Wise's dissertation: "An interpretation of the origins of electromagnetic field theory is presented with special emphasis on change in the mathematical basis of theory construction and on the role of mathematical techniques in producing conceptual change."[1] Although this passage expresses a somewhat stronger focus on mathematics than was actually the case in Jed's seminar, it reflects what I found engaging in the various subjects in physics that we explored together. Of keen interest was the formation of new theories and methods and the creative and conceptual steps involved in the emergence of a novel part of mathematical science.

1. Matthew Norton Wise, "The Flow Analogy to Electricity and Magnetism: Kelvin and Maxwell." Princeton University PhD dissertation, ProQuest Dissertations and Theses, January 1977, p. ii.

A fascinating topic in Jed's seminar was Sadi Carnot's route to the second law of thermodynamics, one of the most remarkable intellectual constructions in the history of modern science. Another subject I remember very clearly involved a beautiful piece of physics, Christiaan Huygens's virtuoso derivation of the laws of refraction from his wave theory of light. Of particular interest was Huygens's account of double refraction for Icelandic crystals. Jed had on hand an actual piece of Iceland spar. As we moved forward through history we followed the dialectic between Newtonian particles and Huygensian waves, ending up with a close study of the mathematical optics of the French physicist Augustin Fresnel. In the late 1970s, the way in which computer work was done was via local terminals that connected to a central university mainframe. Jed spent hours at the terminal going over calculations with Fresnel diffraction integrals, an activity that seemed to exert a powerful fascination for him.

The work we did in Jed's seminar on topics in optics, mechanics, electromagnetism, and thermodynamics provided a model for how to explore past mathematical science, and I learned a great deal from it. What I admired in Jed's work then and still do today is its concern for details of mathematical theory, experiment, and observation, involving an historical engagement with the technical subject that is entirely absent in current social history and does not fall prey to the twin historical vices of professional scientists, namely, lack of seriousness and historical presentism. The ethos that infused Jed's seminar was expressed by him in a review he wrote of a book by the eminent historian of mechanics Clifford Truesdell: "It opened mechanics to true historical analysis... Truesdell's historical essays form a logical part of the insistence on proper understanding of the primary sources that became common in the history of science by the early 1950s. Nearly every page on his histories of mechanics bears witness to his insistence on carefully reading and explaining the original material, on not inserting into it concepts that were developed much later."[2] In this review Jed emphasized the challenges and rewards of understanding the intrinsic technical content of past mathematical science in its original historical setting.

On a personal level I should note that in those days there was not a large age gap between the graduate students and the younger faculty. Indeed, some of the graduate students were older than some of the faculty. The student-professor relationship possessed a somewhat different dynamic from today where the majority of the faculty is one or two generations removed from the students. Nevertheless, even back in the 1970s and 1980s there was at Toronto's Institute a representative of the older generation, Stillman Drake, someone older even than our own parents. Drake was a remarkable figure, an outsider among professional historians of science who possessed an all-abiding interest in the life and work of Galileo Galilei. Stillman and Jed would carry on long conversations in the common room about various aspects of early modern physics, often involving Galileo's science, and these conversations were a constant source of information and edification. Over the years the executive assistant at the Institute Connie Gardner and her husband Terry, a professor in mathematics, would host dinner parties for IHPST faculty at their Moore Park home. Stillman, Jed and various visitors to the Institute would discourse on all manner of subjects in the history of physical science.

I spent one Christmas at the University of Aarhus's Institute for the History of Science, and Jed was also there during part of my visit. The connection to Aarhus resulted from the

2. *Buchwald 1988,* p. 91.

common research interests of Jed and historians of physics in Denmark. At the time both he and I were doing research that included some German sources. The Institute's library kept only Danish-German and Danish-English dictionaries. If one encountered an unfamiliar German word it was necessary to translate it into Danish and from there translate to English. Jed's German was much better than mine, but sometimes he too needed to consult a dictionary. By chance, I had brought with me a German-English dictionary which bypassed this dual procedure and eased the task of working with the sources. Jed's connection to Denmark was a significant feature of his career during this period. This extended not just to joint academic work, but even to culinary matters, such as pickled herring and open-face sandwiches, as well as Gammel Gdansk, a distinctly flavored liqueur which the Danes seemed to consume in large quantities.

Jed was an early adopter of all forms of computer technology, first as I mentioned earlier with local terminals connected to the university framework, then to programmable calculators, and later on to PCs and peripherals. When programmable calculators first came out there was a rivalry between Hewlett-Packard calculators, based on reverse Polish notation, and more conventional devices marketed by Texas Instruments. At IHPST we were adherents of Hewlett-Packard technology.

With the advent of the personal computer in the 1980s Jed became our go-to authority on the new technology, always a defender of the PC against the Mac, although that would change decades later. One of the problems with composing on an MS-DOS machine was its inability to handle the typesetting of equations, formulas and symbols that appear in technical writing. This was before TeX or LaTex had become standard. Jed introduced me to the now long-forgotten software called

ChiWrite, which had a strange and antiquated Rube-Goldberg feel to it even when it was new. Imagine taking an old typewriter, adding elastics and springs and a second carriage, and then producing a software emulation of the whole contraption. Soon we moved onto WordPerfect which had an equation editor that was miles ahead of ChiWrite or anything in Microsoft Word.

Tom Archibald

Long Hair, Brisket, and Indicting Chicanery

My first glimpse of Jed Buchwald was on 22 March 1974 at the first Joint Atlantic Seminar on the History of the Physical Sciences, at the Université de Montréal. He was the other guy with long hair. The Institut d'histoire et sociopolitique des sciences at the U. de M. had just gotten going, and Lew Pyenson was the newly-appointed historian of physics. The talks were a mixed bag—I recall an appalling talk on Hilbert, a pretty interesting one by the late Joan Bromberg, and Martin Klein was also on the program. I was sitting behind the philosophers Mario Bunge and Raymond Klibanski, whom I knew from McGill, where I was working in the Rare Book Department of the fine library. Jed and I had a short chat standing under the pillars of the Mussolini-esque Edifice principale, which included discussion of whether there was anything unusual in the library at U. de M. I had gone at the suggestion of a friend, Raymond Fredette, the Galileo scholar; and I was not in any form of "the profession" at that time. I have never mentioned this to Jed and I expect it will surprise him. It did nothing to adumbrate future interaction. At the time I was approached by a member of the U. de M. faculty to consider studying history of science; he reassured me that I would not have to carry out anything like the impossible technical and linguistic efforts of Otto Neugebauer, but since this was exactly the kind of thing that interested me, I was not enticed.

I next encountered Jed in a grad course in the history of physics in 1979; the IHPST at Toronto had admitted me as an MA student, to my annoyance, since I already had a Master's degree (in mathematics). Jed was on a term appointment at that time, one which morphed into an ongoing one by stages. My aim at that time was to study the mathematical work of Leibniz, or something closely related. Jed's was one of two courses that really worked for me. The readings were very stimulating, and I guess I would say uncompromising in what they required of the reader. This was tough for some bewildered students who knew no physics at all—I think the course was sort of required, or at least hard to avoid. I remember tears in presentations about the Carnot cycle, and Jed was doubtless close to tears during more than one of the seminars. The object of study was a nice combination of major primary and good secondary work from antiquity to the late 19th century. I did a paper about which he was very encouraging, saying that it could be revised for publication, something that fed my ego, as I thought, appropriately.

The history of mathematics component of my studies did not go so well, and I soon found myself seeking a supervisor other than my initial choice, which at that stage implied a change of research direction (it seemed). Following a discussion with Tom Hawkins at the 1980 HSS meeting in Toronto, I acted on his advice and approached Jed, who suggested I could "sort out all that German stuff about action-at-a-distance electromagnetic theory." Gauss and Riemann were involved there, so I agreed, and the fact that I knew nothing much about physics was graciously overlooked. I did other coursework with Jed, notably a course on Ptolemaic astronomy.

When I arrived at IHPST, Jed was a young man about town, with John Major as unindicted co-conspirator and Herbert Odom as a kind of preppy foil. I remember Jed's BMW being stolen. He brought records to grad student parties: "Their Satanic Majesties Request," with its cute lenticular jacket image. He was thought of as hypercritical: the sign on his office door saying "it's hard to soar with eagles when you work with turkeys" could have contributed to that. I also remember him prowling the crowds at the 1979 HSS meeting in New York grumbling about the sad state of history of science. Not so long after, my wife and I were invited to his wedding, where I met Tom Kuhn, a big deal for me, and Jed's parents. Our wedding gift was stemware, goblets selected by Jan; could they have been registered at Ashley?

By that time, Jed had begun his semi-regular visits to the Institute for the History of Exact Sciences at Aarhus. His main "people" at that fine outfit were Ole Knudsen and Philip Lervig, who gave him the opportunity to discuss and work out the understanding of 19th century field theory that is evident in his first book. This affinity for Denmark led him, not much later, to Jesper Lützen, and in particular to their long interaction about Heinrich Hertz. These interactions were a general source of scientific and professional solace for him. He had never been comfortable with the tendencies evident in Isis or HSPS, and the mannerist connoisseurship of Truesdell didn't suit him so well either, though it was in AHES that his first large contributions saw the light, if I recall through the offices of Martin Klein. During that time he was increasingly close to, and influenced by, Stillman Drake, with whom I would say he shared a conviction that close and reflective reading of a scientist's work is what brings us nearest to an appreciation of the processes of discovery and invention that lead to innovation; that, and the notion that it is the work, after all, that is interesting and important. These values were central to his own research, and led in part to the Dibner appointment.

When I started working with Jed, he of course had no supervisory experience to speak of. He had inherited Craig Fraser, who was working on 18th century mechanics, on the death of Ken May, but Craig's interests were quite different from Jed's and he remained at some distance. He also had assisted with Eric Reitan's dissertation, as a successor to the retired Stillman Drake and jointly, I think, with James Weisheipl at the Pontifical Institute. I was not an ideal first full supervision, as one of those annoying graduate students who does not hand things in, wastes a lot of time on matters that are totally irrelevant to the thesis, and hides out a lot. I am grateful that he did not (to my face anyway) lose confidence in me, and was ultimately very positive about a reluctantly-produced and very boring dissertation.

The fact that I had picked up a job in a mathematics department made my long-term involvement with the history of physics hard to sustain, particularly once it became clear that I was highly unlikely to find employment in a history of science unit of some sort. He was generous with professional opportunities to get me started: book reviews, joint articles ("Centi anni di radio"), and a collaboration (also with Kurt Møller Pedersen) on a quirky project about Erasmus Bartholin. Our interactions along those lines petered out, largely due to me having to pursue more mathematical things to remain employed, and I have seen him only rarely since his move back to the US.

Jed's influence on my own work is a bit hard to characterize. I started with the idea that physics was essentially the mathematical part of the subject—experimental work was invisible to me, its role in theory formation mysterious. Jed understood that tension very well; his own lack of exposure to pure-mathematical research surely made him wonder at how dense

I was and how odd my preoccupations. Gradually, some of his understanding penetrated my mathematical prejudices, so that among historians of mathematics (and even philosophers, at times) I pass as someone who is semi-literate about physical theory. Certainly the fact that I have remained very much concerned with studies of published text, which was certainly my initial inclination, is due to his example, and my students have followed that tendency. In matters of taste, he was surely a big influence.

When I squirm at some trendy vocabulary, or read with pleasure indictments of various forms of intellectual chicanery, I feel at my most Jed-ish.

Personally I feel quite close to Jed. I knew New York somewhat and could share his nostalgia for the Madison Deli. He came to my house to eat comfort food on the evening of the day his son was born—brisket, I believe. One afternoon, playing hookey from IHPST, he sat next to me in Spielberg's *ET* and said, in a critical scene, "My God, he doesn't die in this thing, does he?" He shared with me his childhood confusion at the onscreen death of Pinkie Lee: "Pinkie don't look too good." Our dogs, Rosie (mine) and Bucky and Oliver (his), got on well.

As did we. It's part of life that people we get close to often drift away, and of course, despite a certain professional proximity, we operate in pretty different spheres now. I can only be proud of the association with him, and filled with gratitude for the professional support and reassurance that my own singularity had some interest and value. What I've been doing as an historian of mathematics over the past 35 years bears his imprint, and would not have happened without him.

Margaret Schabas

Reminiscences of Jed Buchwald

I was doctoral student at the IHPST in Toronto, starting in 1978. Now that I know Jed's age, I realize that he was not quite thirty years old at the time. Many of us in this room also became professors in our twenties; I started at age twenty-eight. But I wince when I think back to how little I knew or understood. The fact is, Jed probably doesn't or if he does, he wouldn't admit to it. He is different from most scholars in our field. Even in his twenties, he exuded an immense amount of confidence and displayed remarkable erudition. He had very strong opinions of what constituted good scholarship and a deeply rooted belief that he knew how the world was put together. He still does, of course; only presumably at present he is more likely to be right.

I am from Toronto and went to the University of Toronto initially to study music performance. I needed more stimulation, however, and began taking philosophy courses; for a couple of weeks, I also enrolled in the introductory course in HPS for which I would eventually serve as a TA for several years. It meant that I bought the textbook, Westfall and Thoren's *Steps in the Scientific Tradition*. Even though I dropped the course, I kept the book. A year later, having transferred to Indiana University because of their outstanding music school, I found myself drawn back to HPS, and that book told me that there were scholars at IU in the field. I ended up doing my degree in music performance and the philosophy of science. The

latter consisted of about two-thirds of a major in physics (so advanced physics courses) and two-thirds of a major in HPS. Ed Grant, my official undergraduate advisor, was willing to sign off on every course menu I brought to him for approval. I'm grateful for that flexibility. Because I shone in a couple of courses that were jointly on offer with graduate students in HPS, one in philosophy of science and the other the history of chemistry, the HPS department approached me with the offer of a full scholarship for the doctoral program. So in the last semester of my senior year, the week I turned twenty-one, I found myself enrolled in the PhD program at IU. But I really had no idea why I was there. It took a few more years, including a year studying the oboe in London thanks to another scholarship, before I realized that HPS was my true calling. I returned home to Toronto.

While in Bloomington, however, I was strongly encouraged to take a course from Scott Gordon on the History and Philosophy of the Social Sciences, and that proved seminal in steering me away from physics and toward economics, his own subject. When I arrived at the IHPST I announced that I wanted to do my thesis in the history of economics. This was not met with enthusiasm. For one, I was told I would never get a job. But they decided that I could pitch my case to Stillman Drake to see what he had to say. He decided that I could follow this path, (I believe because Galileo had also found economics interesting), and that was that.

I audited Jed's year-long seminar in the history of physics because I still loved the subject, and because it was one of the four subjects I would need for my comprehensive exams that I sat in May of that same year. His course proved excellent for someone at my stage. I had taken the survey courses at IU, Ed Grant on ancient and medieval science, Sam Westfall on early modern, plus his seminar on Newton's *Principia*, Noretta Koertge's on the history of atomism, Alberto Coffa on philosophy of physics, and Fred Churchill's survey of the history of biology. For the survey courses at least, we mostly read secondary literature. The beauty of Jed's course is that we read primary sources for the entire year, and I found that this prompted me to revise many of my beliefs on the subject. The class was also tiny, perhaps four or five students, mostly women, and I must say, there were some tears along the way. This was forty years ago, but I recollect Jed sparking an interest in Archimedes, as well as, months later, the history of thermodynamics. We read a few famous articles that offered a contextual account, "Newton and the Pipes of Pan," and the thesis by Paul Forman, but by and large only to discredit them.

To prepare for the comprehensive exam, I would drop by to chat with Jed from time to time to insure that I had an adequate grasp of the material. His initial reaction to me when I first arrived was that I knew nothing, but over time, I think he came to see that I had a general sense of the subject and some understanding of physics itself. The comprehensive exam went very well. I remember Jed trying to stump me more than once (as I had been warned he would do), but as he assured me in subsequent years, I was able to cough up the answer. I wasn't fazed by him, although he tended to intimidate other students.

Since the IHPST was a very close-knit community, there were frequent encounters between faculty and graduate students. This was also true at IU, but because most of the professors at Toronto were so young, I think it is fair to say there were more interactions. Jed brought Tom Kuhn to visit more than once, and I will forever value the exchanges I had with him at the time. Others who spoke to us, such as Frances Yates or Stephen Toulmin, also proved memorable.

I wrote my doctoral thesis on Jevons and the mathematization of economics, and Jed ended up as one of the examiners at the oral defense. Ian Hacking had just arrived at UofT and was also an examiner, and basically I spent the two hours entertained by everyone else trying to impress Ian. Fortunately, when I arrived at my viva, Ian was sitting outside the room, not knowing he was supposed to be inside. He told me he really liked my thesis so I knew everything would go smoothly. But I remember Jed taking on a philosophy professor (I won't give his name) and trying to expose the fact that he did not know what he was saying. Jed was good at this. I believe he has remained dubious of the value of philosophy, or academic philosophy, to this day, and I understand why and am grateful for the reminder.

Jed grew into his job at the IHPST, and when I returned to Toronto to be a professor at York University, it was clear he was in his element (not least because he was tenured). He had, in effect, taken Stillman Drake's position as the person who had an opinion on every subject, and it was usually a well-formed opinion and thus worth hearing. And he still tended to teach by intimidation, and set high standards in the classroom. He then moved to the Dibner, and I visited his Boston home, meeting his charming mother as well. In 1995, I went to the Dibner as fellow for the winter semester.

Jed is a remarkable scholar and a person of the utmost integrity. He may not be humble, but underneath his gruff manner there is a very generous soul. He is a good listener, and over the years, he has helped me considerably with my professional and personal life. He still thinks most economics is bullshit and is puzzled by the fact that I beaver away on the subject, albeit critically. But I think he came to see that there is some value in understanding it, historically if not philosophically, much as one might look into alchemy or witchcraft or something like that. And he has had me down to Caltech to give a talk in a workshop on early modern rationality. Our paths cross about once a year, at conferences mostly, although he recently came to speak at UBC in February 2018, on one of the rare days that it snowed, and we found ourselves in adjacent departure lounges at the airport the next day and had more time to chat.

Jed and Larry Stewart recently produced a Festschrift in honor of Trevor Levere, who was my doctoral supervisor (and shares Jed's skepticism of the value of economics). I decided to branch out and write on Coleridge's economics, since Trevor had produced, long ago, a path-breaking study of Coleridge's science (but neglected the economics). It proved to be a fascinating project. Coleridge had a lifelong interest in economics, and wrote a number of pamphlets, some of which influenced John Stuart Mill. Coleridge had read the leading sources in economic discourse (as everyone did in 19th century Britain), and developed a Christian duty-of-care ethic that overrode market forces in the time of a poor harvest and shortage of food. It does address the cultural context of economics more than the core theory, but sheds light on the genesis of Mill's economics that dominated the field for decades. Jed secured publication of the book in his *Archimedes* series, so I am very grateful for that opportunity.

Let me close by saying that Jed represents the position in the history of science field that I still adhere to, the one that demands one first learn the nuts and bolts of the science and start with the primary sources before weaving in the broader historical context. Alas, there are far too many scholars in the field who do not in fact know any science firsthand and, as a result, they write cultural history but not, in my or Jed's view, the history of science. Having studied the history of music to an advanced level, it is clear that in that field as well, one must develop one's ear and eye to comprehend musical scores well and, ideally, perform them on the piano. My best music professors could play Wagnerian operas by ear on the piano (without looking at the keyboard). I think of Jed as in that rare and exalted class. Maybe he wouldn't distinguish *Tannhäuser* from *Lohengrin*, let alone read the scores or reduce them to a comprehensible form on the piano. But he understands at a highly sophisticated level the physics of the 19th century, and now, the period of Newton as well. He could read a paper by Fresnel or Hertz and decipher the train of thought and make note of oversights or unwarranted inferences. And he could trace the sources and follow up on the absorption of their ideas in their immediate successors.

I attended the recent session at the Seattle HSS meetings that Jed and Moti organized on recent finds in Newton scholarship. It reminded me that there is still much work to be done on the core material of the history of science, even on a giant such as Newton. Jed is at the vanguard of that tradition and with luck, will continue to take us forward for decades to come.

Olivier Darrigol

Jed Buchwald: A Joyful History of Science

In the winter of 1984, a small group of scholars convened in Schloss Rindberg atop a Bavarian mountain, invited by Lorenz Krüger to celebrate the 100th anniversary of Helmholtz's death. For a few days we were fully isolated from the rest of the world. Overabundant snow—not Buñuel's exterminating angel—forced us to stay longer than planned. This was my first opportunity to know Jed Buchwald, whose writings had been an important source of inspiration in my own researches. In the talk he delivered on Hertz and Helmholtz he stunned his small audience with an unusual introduction well worth quoting in full:

> On Christmas Eve, 1987 an older chemist whom I shall call G climbed slowly to his attic. His father, himself a well-known physicist in the early years of the century, had long ago told him about a box of papers that was not to be opened until that very day. After many hours of digging through the dust of decades, G found a small, leather-covered box with the initials "𝕳𝕳" prominently inscribed on it in gold in the old German script as the box's cover. G sat in a broken chair by the pale winter light that filtered through an attic window and began to read.
>
> It did not take long for G to realize that he had in his hands the laboratory notes for a completely unknown experiment undertaken by one of his father's closest and long-mourned friends, the great Heinrich Hertz, discoverer of the electric waves. G recalled his father's tales of Hertz's glory days, so soon ended by the terrible blood poisoning that stole from him his rightful place at the helm of German physics as successor to his mentor, Hermann von Helmholtz. In the spring of 1888, G's father had often said, Hertz suddenly and astoundingly proved Maxwell's electric waves to exist by reflecting and refracting them. But the papers in G's hand gave him an uneasy feeling. They seemed to have something to do with waves. There, clearly diagrammed, were Hertz's devices—his oscillator and clever, detecting resonator. Numbers that seemed to be wavelengths appeared in appropriate places. And yet, something did not look quite right, for nowhere could G find the slightest trace of Maxwell's equations or anything even vaguely like them.
>
> The weak light was rapidly fading now, so, puzzled and perturbed, G took the papers downstairs with him. The family had gathered for the evening's celebration, but G could not keep his mind on the festivities. When everyone had gone home, he quickly grabbed the old papers and started reading them again from the beginning, this time with pencil in hand. Every afternoon and evening for the next three weeks G poured over Hertz's lost manuscript again and again. In mid-January he felt that he had grasped its inner meaning. And he also knew that he would never breathe a word about it.
>
> The lost manuscript, G now realized, contained an astonishing record of experiments that ran completely counter to the demand of the very theory for radiating dipoles that Hertz had himself developed on the basis of Maxwell's equations in the summer and fall of 1888. These experiments had been done in December of 1887, exactly a hundred years before G was permitted by his father's will

> to open the sealed box. According to them, the field near the dipole behaves quite differently from the requirements of Hertz's equations. Equally unfortunate, Hertz had apparently measured a substantial difference between the wave's speed in air and its speed when guided by wires, which runs completely counter to Maxwell's theory.
>
> Far from having confirmed Maxwell's theory, G now saw, Hertz's earliest laboratory work confirmed something very different from it indeed, something that had nothing at all to do with fields. G could not quite see what that other thing was, except that Helmholtz had produced it, for he was of course no historian. Hertz, G concluded, must have turned quickly to the experiments on reflection and refraction that had made him famous then carefully hidden away these early ones, trusting them in the end to the care of G's father, who could not bring himself to burn these last few relics of his closest friend. G felt the warmth of the fireplace behind his back. With only a light twinge of regret he turned and tossed the manuscript into it. Hertz's reputation was forever secured.

After a well-measured pause, Jed continued:

> These events never happened. Nor did anything *quite* like them. There is however a chemist named Gerhard Hertz, grand-nephew to Heinrich and recently retired at Karlsruhe, who not long ago uncovered his grand-uncle's laboratory notes. Unlike the G of my story, he made the notes immediately available. By means of them it has been possible to reconstruct in precise detail the course of Hertz's work during a critical three month period from October through December, 1887. Though the events of my story may never have taken place, nevertheless the contents of my fictitious manuscript and the actual discovery document lead to the same conclusion: namely, that Hertz did not at first discover new kinds of waves; he discovered new kinds of forces. Unlike the fictitious Hertz, the real one did not hide his discovery; he trumpeted it loudly in the pages of the Berlin *Sitzungsberichte* and soon thereafter in the *Annalen der Physik* itself. My purpose here is to make clear what Hertz felt he had found, and to explain what Hertz did when he later decided he had been mistaken. (*Buchwald 1994f*, pp. 43–44)

Historians of science, Jed included, are naturally attracted by major turning points or discoveries that seem mysterious at first glance: textual evidence typically does not square with the way we would naturally reconstruct the discovery. In favorable cases, a careful study of the historical context by means of published and unpublished sources reveals the true itinerary of the discoverer, and this itinerary becomes intelligible even though it may widely differ from any naive reconstruction. The mystery is dissolved and we can move on to another topic. Thus, to a large extent, the historian's purport is to dissolve the mysteries that draw him to history in the first place. Just as literary criticism may destroy the beauty of a poem, history may dull the edges of its object. This never happens when the historian is named Jed Buchwald.

In the cited sample of his Hertz talk, an unusual literary artifice serves Jed's communicative purposes. Through a fictional tale he amplifies an enigmatic feature of Hertz's researches and he invites us to consider the forgotten, partly hidden context. In the rest of his talk, he introduces an explanatory principle, "Helmholtzianism," through a comparative study of the laboratory and theoretical practices of the chief electrodynamicists of Hertz's time. In the Helmholtzian view, Jed tells us, physical phenomena derive from characteristic potentials for the interaction between pairs of bodies in specific states (charge carrying, current carrying, polarized, etc.). A Helmholtzian experimentalist like Hertz tends to explore all possibilities in a taxonomy of two-body interactions. For instance, in the series of trials that ultimately led to the electric waves, Hertz wanted to know whether a variably polarized dielectric interacted with a current-carrying conductor. It is important to note that Helmoltzianism, as Jed described it, is not defined by Helmholtz and Hertz themselves. As Jed writes:

> That deep intellectual bond between the master and the apprentice runs through many pages of this book. And yet Helmholtz's way of doing physics, with which Hertz grappled long and hard, is historically elusive because it was not explicitly articulated either by Helmholtz himself or by others. My arguments for its very existence, beyond electrodynamics at any rate, depend on the presence of a common, persistent, and unifying pattern in many different aspects of Hertz's work, on rips that Hertz perceived within that pattern, on the structure of Helmholtz's electrodynamics and the experiments concerning it that were carried out in his Berlin laboratory during the 1870s. (*Buchwald 1994f*, p. 325.)

As we will see, in all his works in the history of physics, Jed did not hesitate to cross the danger zone in which historians invent hidden causes for the historical processes they are trying to explain. This audacity is the chief source of excitement when reading Jed's histories: whether or not one is willing to accept his explanatory principles, they reveal continuities and coherences that are too stunning to be merely accidental. Even if the invented causes are not quite real, one feels they must capture a good deal of the historical truth.

Another, earlier example of Jed's explanatory principles is the "Maxwellianism" defined in the first part of his *From Maxwell to Microphysics*. Maxwellianism is there defined by the adherence to basic notions of charge, current, and polarization that are radically incommensurable with the continental notions with which we are now familiar. To some extent, this principle is not any historian's invention because Maxwell and his disciples were certainly aware of the novelty of their basic notions and spent much time to explain and illustrate them. But their explanations lacked consistency and uniformity, and they did not express implications which, in Jed's opinion, conditioned the subsequent history of electrodynamics. For the sake of clarity, Jed invented the "$\vec{\lambda}$ shift" for Maxwell's incompressible electric fluid, and he carefully distinguished it from the displacement $\vec{D}$ in Maxwell's field equations. Most important, Jed proposed that the Maxwellian concept of electricity was inherently macroscopic and incompatible with the ascription of electric properties to atomistic entities. Even though the Maxwellians did not express this incompatibility, Jed convincingly argued that the later history of electrodynamics, including Larmor's and Lorentz's breakthrough, depended on ways of transgressing Maxwellianism (*Buchwald 1985a*.)

In his later book *The Rise of the Wave Theory of Light*, Jed similarly focused on a problematic concept, the concept of ray, and showed that it carried traces of its unconscious association with the corpuscular theory of light. Just as different concepts of electric charge generated misunderstanding between Maxwellian and continental electrodynamicists, different concepts of ray generated misunderstanding between the supporters of Laplacian optics and those of Fresnel's optics:

> The kind of theoretical difference I have tried to explore through the example of ray and wave theory seems to me pervasive in the history of physics. In electromagnetism I have argued that what one thinks of as perhaps the most

> elementary concept—the idea of electric charge—was so different in Britain and on the Continent in the last quarter of the nineteenth century that it was very difficult for physicists to communicate fruitfully with one another.

For optics in the early 19th century, Jed's explanatory principle is "selectionism," according to which a device that produces light with a specific property (color, polarization) is a device that selects rays carrying this property in the original beam. This principle naturally occurs in corpuscular, Newtonian optics because the rays then are the trajectories of the light corpuscles that carry all the properties of light. As Jed explains, selectionism conditioned the laboratory practice of Laplace's disciples in an unconscious manner. For instance, Jean-Baptiste Biot believed his concept of ray and the associated theory of chromatic polarization to be compatible with both the corpuscular and the wave concept of light, whereas in reality he relied on a selection principle incompatible with the wave concept. Much of the misunderstanding between Biot and Fresnel depended on the implicit selectionism of Newtonian optics. The standard view according to which it was all a fight between waves and corpuscles appears to be insufficient:

> One must also try to capture the effects of the more delicate and difficult distinctions between rays and waves, distinctions that usually remained just below the surface of scientific discourse, subtly affecting its texture and tone. (*Buchwald 1989a*, p. xx.)

Thus, according to Jed, a proper analysis of scientific controversies implies unarticulated principles that differentiate the positions and practices of the protagonists. Even after the controversy is resolved, the protagonists of the winning view may remain unaware of these implicit principles:

> When one finds irresolute controversies, one may also find that the ostensible, the explicitly recognized differences are not the only ones at issues, and that other differences run so deep that they have precluded mutual understanding. Over time one way of thinking becomes perhaps not incorrect, but certainly irrelevant. Ray theory, for example, became irrelevant well before it became incorrect, and the electron replaced the British conception of charge before the latter became difficult to reconcile with experiment. And yet in neither case did anyone explicitly recognize what it was that had been replaced. In electromagnetism not only did Lorentz's or Larmor's fields and particles replace ether models, but electrons also replaced field discontinuities. In optics not only did ether waves replace light particles, but wave fronts also replaced physical rays. (Ibid., pp. xxi–xxii.)

Any reflecting historian knows that a history is not built by a merely passive synthesis of sources. The historian selects materials in a virtually unlimited corpus and arranges them in a sense-bestowing narrative. The principles that preside over this active synthesis vary according to the historian's style. The least adventurous will try to keep the narrative at the level of documented scientific discourse and seek to justify the actors' moves within this discourse. The most adventurous will project preconceived ideas of the nature of scientific work on a supposedly malleable corpus. Jed offers a via media in which the historian detects unconscious presuppositions that affect an actor's positions and interactions. Although these presuppositions may never appear under the actor's pen, they are so closely tied to the actor's practice that no documentary evidence can contradict them. A history à la Buchwald thus illuminates us without venturing into easily refutable "rational reconstructions." Even if some of Jed's explanatory principles would turn out unnecessary, even if a more down-to-earth narrative could provide an equally coherent picture of a given scientific episode, the latter narrative would lack the charm inherent in Jed's revelations. Lost would be the sense of wonder and the pleasure of intellectual conquest.

Since the memorable Helmholtz event at Schloss Rindberg, I have had many opportunities to enjoy shared time with Jed. Whereas a typical historian may look more like a weary Sisyphus, Jed is an ever enthusiastic scholar with new exciting ideas and projects constantly popping in his mind. A dinner with him in Paris, whose pleasant resources he seems to know better than me, is the best remedy I know for intellectual melancholia. A new book by him is an eagerly anticipated thrill. You never quite know to which untrodden terrain his insatiable curiosity will take you. But you surely know you will enjoy his inventive guidance.

A. J. Kox

From Amsterdam to Boston, Pasadena, and Elsewhere

I first met Jed in August 1985, at the XVIIth International Congress of History of Science at UC Berkeley. It was my first meeting in a field that I had recently entered after having given up my career as a theoretical physicist. I had just started a two-track existence, which I am still following to this day: my work on the history of science in the Netherlands, in particular the work of Hendrik Lorentz, whose correspondence I have published and whose biography I am in the process of finishing, combined with my "second life" in the U.S., as a member of the editorial team of the Einstein Papers Project.

When I met Jed at Berkeley, I already knew his work because of its relevance to my work on Lorentz. In particular, I had carefully studied Jed's first book, *From Maxwell to Microphysics*. But our meeting, in a larger group that also included Martin Klein, Alan Shapiro, and Roger Stuewer, was purely social. As a mere beginner, I was very pleased to be taken seriously by all these luminaries. In later years, Jed and I met infrequently on various occasions, and very rapidly an interesting pattern developed: every time we met, we launched into technical discussions, no so much about 19th-century physics, but about computers and all kinds of related matters. I thought of myself as reasonably knowledgeable on those things, but compared to Jed I was, again, a mere beginner. He was always way ahead of me, which was fine because I learned a lot.

After Jed's move to Cambridge our meetings became more frequent, as I had begun spending half of my time at the Einstein Papers Project at Boston University. I have especially vivid recollections of three of those meetings. One was a Sunday brunch at his Cambridge home, which Martin Klein attended as well. Jed presented a new gadget, this time not electronic, but purely mechanical. It allowed one to cut a bagel with precision in two exactly equal parts. I was duly impressed, but Martin called Jed a "wimp" for needing this contraption instead of using his bare hands and a knife. A second meeting took place at Jed's apartment in one of the MIT dorms, where he was house master. There he showed me the largest TV screen I had ever seen. Combined with a series of gigantic speakers, it transformed his living room into a veritable movie theater. To demonstrate how effective it was, he played for me a particularly loud and scary part of the movie Twister. I was sure scared all right! The third meeting that is still vivid in my memory, although I now have mixed feelings about it, was a lunch at Legal Sea Foods in Cambridge. My colleague Robert Schulmann and I tried to persuade Jed, at the Dibner Institute, to take over the Einstein Papers Project, which was facing serious difficulties at the time. Jed refused in a firm but very diplomatic way, sweetening the pill by buying us a delicious lunch, accompanied by a beautiful bottle of wine. The events that followed and Jed's role in them are better left to a future historian of the Einstein Project.

When the Einstein Papers Project moved to Caltech and I rejoined it after a break of a few years, our contacts became much more frequent and much more informal. One thing remains, though: whenever I see Jed again after an absence

of a few months, he always has nifty new hardware, amazing gadgets, or the latest software to show me.

In the past years I have spent countless evenings with Diana and Jed at their beautiful home in Altadena. I have even taken some memorable trips with them to places like Palm Springs and Santa Barbara. We have also spent many good times together in other places, both in the US and in Europe, especially in my home town of Amsterdam when Jed spent a sabbatical as Zeeman Professor at the University. Over the years, my friendship with Jed and Diana has become an important and indispensable part of my life and my regular stays at Caltech, and has come to include not only us but our respective families. For this I am very grateful and I hope our friendship will continue for many more years to come.

Robert Fox

More Elusive Forces at Work

ALTHOUGH WE HAVE MET ONLY INTERMITTENTLY, Jed has been a presence in my scholarly life for over forty years. I have found much to admire in his work, not least two guiding principles that seem to me to run through it. First, the principle that a rigorous engagement with the detailed technicalities of physics is no impediment to a sensitive regard for the broader context and that historical writing that eschews the former risks being a diminished enterprise. Secondly, the principle that such an engagement is, and should be, an invitation to the more challenging task of quarrying below the surface appearances. In a reminiscence published some years ago, Jed saw this quest for a deeper engagement at work in the Thomas Kuhn he encountered as an undergraduate at Princeton in the late 1960s and then as Kuhn's research assistant.[1] As Jed recalled it, this was less the Kuhn of *The Structure of Scientific Revolutions* (1962) than the very different, text-focused Kuhn of *Black Body Radiation and the Quantum Discontinuity* (1978). It was precisely this latter Kuhn that I encountered myself, some years after Jed, in a year at the Institute for Advanced Study in 1974–1975, with an office down the hall from Kuhn when he was writing *Black Body*. It was a year that reinforced my own conviction that the capacity and patience to undertake the minute scrutiny of a text, in all its technical

1. *Buchwald 2010b.*

intricacies, meanderings, and multiple layers of meaning, are essential parts of the historian's armory; they are not sufficient, of course, but they are unquestionably necessary.

Jed, I think, needed no such reinforcement. He has always dug deep in his encounters with past science, in search of more elusive ways of working that have their origins in repeated practice rather than in programmatic statements of method and world-view. As he showed in *The Rise of the Wave Theory of Light*, it was in part an unspoken commitment to very different ways of working that made it so easy for partisans of "ray optics" and "wave optics" in the early 19th century to talk past one another rather than to debate from a position of agreement on the problems deemed to be relevant and the kinds of answer that might determine the issue.[2] Incommensurability, if you like, but all done with the attention to detail and specifics that marks Jed's style.

I have clear and happy recollections of all our encounters, invariably convivial, never twice I believe in the same place. I recall a memorable Sloan international summer school in Brewster on Cape Cod in 1998, in Jed's Dibner days, when among many other things we convinced ourselves, in an improvised optical laboratory, that Newton's experiments with prisms were far trickier than we liked to believe, or trickier than we liked to tell our students. Then there were our chats on the margins of the International Congress of History and Philosophy of Science in Liège, in 1997, when Belgian beers (at least, I assume it was Belgian beer) gave us relief from congress business and aided our reflections on changes in the field and our community since we began, both of us before the new waves of the 1970s.

2. *Buchwald 1989a.*

Participation in *Metascience*'s book symposium on *The Zodiac of Paris* with Charles Gillispie, Theresa Levitt, and David Aubin was an encounter of a different kind.[3] But an encounter it was, with the scrutiny by Jed and Diane Greco Josefowicz of two parallel "texts": one the sandstone Dendera zodiac itself, the other a flurry of exchanges sparked off by its arrival back in Paris in 1821, more than two decades after the French had examined it in situ during the Egyptian campaign. What *The Zodiac* brought out very persuasively was that an "improbable" intensity of excitement about a seemingly arcane exercise in the dating, provenance, and interpretation of an ancient artefact could be made eminently understandable once the niceties of text and context were brought together and an examination of appearances (appearances again, but here on a matter far removed from physics) gave way to more searching questions. Was the affair just a minor, even predictable confrontation between science and religion over an inscription and a history of zodiacal signs going back, as many thought and others denied, several thousand years before any biblically endorsed date for the Creation? Or were more complex forces at work, to do with the tides of religiously fired conservative suspicion of Enlightenment rationalism that were circulating during France's restored Bourbon monarchy in the 1820s? Those forces were precisely the ones that Jed and Diane uncovered. They did so in a story characterized by fuzzy cultural boundaries and a multiplicity of criteria by which sides might be taken in what they showed was a far from straightforward debate. Was more or less credence to be given to an approach to dating based on astronomical theory and calculation or to one rooted in history and philology? And by what convolutions had science-based argument

3. *Buchwald and Greco Josefowicz 2010a* and *Buchwald et. al. 2012b.*

come to be deployed in support of wholly contradictory positions: that of the pious and now increasingly conservative Jean-Baptiste Biot, who used his mastery of mathematical astronomy to date the zodiac to the unthreatening date of 716 BCE, while others, including François Arago and Joseph Fourier, drew on their own "rationality" in casting doubt on Biot's conclusion? Among key answers, as *The Zodiac of Paris* showed, were the hard-to-penetrate complexities of power relations and generational change in a French physics community in the throes of rejection of the authority of Laplace and his circle at Arcueil.

My most recent contact with Jed was the result of an enquiry from Oxford University Press, just down the road from where I live. Asked whether I thought a *Handbook of the History of Physics* might be a goer, my answer was "yes, let's talk." From the start, though, there was a "but." The "but," impressed on me by colleagues who had edited other Handbooks, was that any such venture needed an editorial pairing, for exchanging thoughts, encouragement at low points, and just getting the job done in timely fashion. My immediate thought, if I was going to be involved, was a partnership with Jed. And happily, a few emails and an Oxford lunch later, our collaboration was launched. Jed did much to help in assembling a slate of talented and cooperative authors, contributed three chapters himself, and exercised his usual rigorous standards, both in what he wrote and in his judgements of the other chapters, all subjected to his eagle eye for less than limpid expression, whiffs of presentism, or a failure to take due account of the current literature. My sense is that Jed was pleased with the result as I certainly was.[4] I like to think we worked together pretty well.

So congratulations, Jed, on this milestone. And here's to many more to come.

4. *Buchwald and Fox 2013b.*

Kathryn Olesko

The Creation of Historical Effects

Few would disagree that *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves* (1994) marked a major turn in how Jed conceptualized the history of physics. His first two monographs, now regarded as classics, focused on key physical issues undergirding major theories—the tensions between microscopic and macroscopic approaches to electromagnetism in *From Maxwell to Microphysics* (1985) and particle vs. wave theories of light in *The Rise of the Wave Theory of Light* (1989). Although especially in the latter he considered experiment, it was by no means the predominant category of his historical analysis. His approach changed in *The Creation of Scientific Effects*, where he expanded upon and refined our understanding of experiment in 19th century physics. Here he went beyond what was then largely a bipolar distinction between exploratory physics (where something new was discovered) and measuring physics (where one obtained numerical data, mostly for the determination of physical constants). His expanded tripartite distinction between experimental approaches to electrodynamics is worth considering not only for what it offers to the history of physics, but for what it reveals about Jed's subsequent approach to history.[1]

1. *Buchwald 1994a; 1985a;* and *1989a*. I write this little essay partly tongue-in-cheek, but I believe it contains more than a grain of truth.

In *The Creation of Scientific Effects* he neatly separated the Fechner-Weberian, Faraday-Maxwellian, and Helmholtzian approaches to electrodynamic theory according to how they treated charged object states in laboratory settings. For Fechner and Weber laboratory instruments detected and measured the physical interactions not between the charged objects, but between charged objects and the material particles around them. In the case of Faraday and Maxwell, laboratory measurements also did not register the physical interactions between charged objects (which effectively disappeared from view). Instead they postulated something new, the field, which occupied the space of the objects as well as the regions surrounding them. The condition of the object was thus rendered in terms of the state of the field, and laboratory measurements were the result of the state of the ether that constituted the field. Helmholtz, in contrast to both of these views, refused to reify laboratory objects or to introduce an imagined construct like the field. Instead he focused on the possible states that objects have in relation to other objects interacting with them. States of interacting objects in a laboratory setting were electromagnetic interactions in the form of energy. So the Helmholtzian research agenda focused on uncovering the possible interactive states that objects could have in a laboratory setting or, as Jed put it, of discovering the *taxonomy* of interactions.

Despite Jed's clarity in differentiating these three approaches to electrodynamic reality, each one was rather abstract and rested on different theoretical formulations. The epistemological differences between those realities aside, their laboratory manifestations—what could be done and expected in a laboratory context—differed markedly. In the Weberian laboratory, devices (which remained fairly constant) measured primarily constants of nature; new effects rarely emerged from experimental settings. The principal way to improve one's experimental results, then, was to seek greater precision in measurement. Here one built, measured, and then analyzed. In Helmholtz's laboratory, by contrast, where Hertz and Boltzmann learned their craft, devices were not fixed, but neither were they mere targets of instrumental modification and improvement for the purpose of obtaining more precise results. Instead they were objects of investigation in their own right. One built, tested, and then altered the device for the purpose of achieving novel states in the laboratory setting. As Jed put it, for the Helmholtzian the physical laboratory was an "engine for discovery."[2] Hence Hertz created the dipole oscillator and resonator to fabricate the first laboratory production of electromagnetic radiation and in doing so made "Helmholtz's interaction physics come to life."[3] A Helmholtzian laboratory was thus not only more flexible in its material outfitting than a Weberian one, it also presented far greater challenges in the interpretation of scientific effects. Both the context and the configuration of these effects, including laboratory machines and instruments, became part of the matrix of objects taken into consideration when assessing the results of a physical investigation.

This schema is a provocative way of understanding how differences in laboratory experiments were manifest in late 19th-century electrodynamics. Yet in terms the evolution of Jed's *historical* methodology, there's something more here than a shift from the analysis of theory to the analysis of experiment, or than the introduction of a more refined parsing of the types of experiments undertaken in the 19th century. I would argue that his perspicacious unraveling of the creation

2. *Buchwald 1993b,* p. 344. The article nicely summarizes the principal arguments of *Buchwald 1994a.*

3. *Buchwald 1994a,* p. 327.

of *scientific* effects was also the advent of a new way to create *historical* effects. It is this novelty that brought into mutual dialogue historians of physics (including social constructionists among them, a group hitherto outside his purview), philosophers of science, and members the STS community through his 1995 edited volume, *Scientific Practice*.[4] This volume further reinforced Jed's intellectual turn. *Scientific Practice* critically examined history—the relationship between theory and experiment—and historiography—how we as historians tell stories about their interaction. As his conclusion to this volume testifies, Jed was at the time thinking deeply about how historians of science exercised their craft—what categories of analysis they used, how they treated historical actors and why, and how, they constructed narratives.

I don't think it's serendipitous that immediately following his analysis of scientific effects in Helmholtz's laboratory, these historiographical concerns emerged and were further articulated and developed in his subsequent two books.[5] Each can be viewed as a laboratory for the creation of historical effects in that they brought into being and made real historical entities that historians of physics either might have overlooked or simply ignored from more parochial perspectives. To see how this "creation" works, first consider the parallels between the scientific laboratory and the historical laboratory. In each one has to decide which objects will be investigated, how they are related to one another, and what functions those relationships represented. Then one has to consider the raw material available for analysis: archival material is for the historian as laboratory data is for the scientist. Finally, skill in

4. *Buchwald 1995a.*

5. *Buchwald and Greco Josefowicz 2010a. Buchwald and Feingold 2013b.*

executing laboratory methods can be likened to the historian's skills at analysis and narrative.

Let's see how these parallels express themselves when we analogize the principles of the Faraday-Maxwell field theory and the Helmholtzian states of objects to historical investigations. Take the Faraday-Maxwell field theory first. From a scientific perspective, field theory dictates that what surrounds objects is more important than the state of the objects themselves, and that interactions between those objects are nearly irrelevant. From a communal or social perspective, field theory generates paper-based practices that spread easily but are derivative, mostly from textbooks. Jed has made known in reviews his opinion of histories that prioritize this type of ambient atmosphere for explaining the content of scientific work. By contrast, the dual Helmholtzian assumptions—that states of objects do not have fixed properties but are dependent upon interactions, and that it's counterproductive to introduce new fabricated entities—suggest a historical laboratory where context and relationships matter. This history *contextualizes* objects of investigation in the reality of the past in order to uncover and then accentuate the historical meaning and significance of discovered results. Here the importance of generating novel (and not merely refined) laboratory data is similar to acknowledging the strategic importance of thoroughly examining archival material in order to produce novel effects in history—in contrast to merely refining our understanding of the past by creating a "mash-up" of secondary sources.

There is something to be learned here from extrapolating from the methodology that led to Hertz's *scientific* effects to the methodology that informed Jed's *historical* effects: making something "real" in historical scholarship is very similar to making something "real" in physics. And just as Jed felt

that he had to hold modern notions of electrodynamic effects at bay in order to interpret Hertz, we have to do the same in history; for the most difficult part of understanding the past is when actors believe something is real or important when we know from the vantage point of the future that it did not remain so. That's why Jed's apology for the way he unfolded the story of Hertz's scientific effects applies equally well to his two most recent books: "This book," he wrote, "has taken the reader on a long and perhaps disorienting ride through unfamiliar territory. It could not have been otherwise."[6]

Now turn to those two most recent books. Why should we regard the zodiac of Dendera or Newton's chronologies as important and significant objects of historical investigation in the history of science? Convincing us as readers of the historical reality and significance of these artefacts of the past and the stories told of them is exactly what Jed did with the cooperation of his two brilliant co-authors in these recent works, which took unlikely but not entirely unexpected methodological turns after *The Creation of Scientific Effects*.

The Zodiac of Paris (2010), co-authored with his gifted former doctoral student, Diane Greco Josefowicz, begins with a trip to a Parisian bookshop that turns into a mystery story about what, exactly, was the date of the zodiac of Dendera, taken to Paris after Napoleon's expedition to Egypt in 1798? Here Jed and Diane take an object we, as historians of physics, otherwise would have overlooked and reinsert it into its full and proper historical context where physicists had a role to play. The flurry of interest in the zodiac pitted physicists and astronomers, with their tools of precision in measurement and projective geometry, against humanists adept at interpreting and translating texts. Since chronology was also a

6. *Buchwald 1994a*, p. 325.

biblical issue (the date of Noah's flood being the pivot point), the controversy over the dating of the zodiac was also one of science vs. religion—of stereographic projection vs. biblical interpretation—a conflict stoked especially by conservatives who were on the rise in the political turmoil of Restoration France after the fall of Napoleon and who regarded the natural sciences with suspicion. This is not a story devoid of physics, which provides the means for understanding the "object state" of the zodiac. Fourier is omnipresent (he was with Napoleon in Egypt), but also present are Biot, Arago, Delambre, Ampère, Monge, Poisson and others who are embroiled in the race against humanists to date the zodiac properly.

Jed's and Diane's study does more than embed physics in culture by revealing what physicists did in their spare time. It also exposes the intrigue and distrust surrounding the role of expertise in the state, a role which had emerged and grew over the 18th century but only became widely accepted in the early 19th century (and has regrettably become a contentious political matter today). Physicists and astronomers, who treated the zodiac as an image (e.g., for Biot, a planisphere), got it wrong; while the linguist and philologist Jean-François Champollion, who studied the Rosetta Stone and regarded the zodiac as a text, cracked the code of Egyptian hieroglyphs and got the chronology of the zodiac right. Ironically it was the empty cartouche in the zodiac, devoid of words or images, that provided the key to its dating.[7]

Whereas earlier Jed's questioning of historical methods focused on ontological and epistemological issues (e.g., emission vs. wave, what we can learn/know from experiment), here

7. I really wanted to give *The Zodiac of Paris* the type of analysis that Jacques Lacan applied to Edgar Allan Poe's "The Purloined Letter," but fortunately time did not permit. For Lacan's analysis see http://www.lacan.com/purloined.htm (accessed November 1, 2018).

he and Diane delved into the politics of how significance and meaning were attributed to an artifact. Their analyses—especially concerning image vs. text—prefigured what is only now becoming a thriving research field in the history of science, particularly in Europe: the relationship between the methods of the sciences and those of the humanities. Fittingly *The Zodiac of Paris* generated a provocative set of reviews, including a symposium in *Metascience* and an essay review that brought the book into dialogue with Langdon Winner's classic article, "Do Artifacts Have Politics?"[8]

Chronological interpretation and its relationship to the physical sciences were also the main themes of *Newton and the Origin of Civilization* (2013), which Jed co-authored with his eminent colleague Mordechai Feingold. This volume joins a growing literature that has fleshed out Newton's religiosity, but does so in a new and radically different way: by comparing and relating how Newton assessed historical evidence to how he assessed data about the natural world. Once again Jed, now with Moti, targeted the relationship between the natural sciences and the humanities as the focus of investigation. The pivot on which the book turns is Newton's posthumous *The Chronology of the Ancient Kingdoms* (1728), a major piece in his oeuvre of religious writings, but one that sought to get the historical record straight by dating with reliability and certainty the flood and the repopulation of the earth after the deluge. And therein lays the problem: How can one make reliable and certain historical statements? What is trustworthy historical knowledge?

8. *Buchwald et.al. 2012b;* Ralph Kingston, "Do Ancient Artefacts have Politics?" Historical Studies in the Natural Sciences 41(4) (2011): 457–469; Langdon Winner, "Do Artifacts Have Politics," *The Whale and the Reactor: A Search for Limits in the Age of High Technology* (Chicago: University of Chicago Press, 1986), pp. 19–39.

To uncover how Newton answered these questions the authors begin with two exemplary chapters, each worthy of being a separate publication in their own right: how Newton handled numerical data, and how he regarded knowledge acquired by the senses.[9] Aided by his critical examination of the new instruments of the 17th century, primarily the telescope, Newton analogized sensory data to instrumental data. Both, he thought, should be regarded with skepticism due to imperfections in their construction and operation. Here Jed's and Moti's analysis links to the history of science and history of technology literature on craftsmanship and artisanal skills. With regard to numerical data, Newton did (secretly) what others did not. Rather than choosing the most appropriate data point and discarding discrepant data, Newton took averages of larger data sets as a means of eradicating error, of obtaining reliable results, and of constructing trustworthy knowledge—all achieved prior to the development of the method of least squares, which only legitimated the average as the most probable result around 1800. Averages became "the weapon with which he slew the inevitable dragons of sensual error." Newton then analogized this method to the corrections he offered for dating and chronologies in the humanities where he recognized that words, like data, were to some degree unreliable. Newton's confrontation with humanistic methods led him "to consign [them] to the same dust bin that held Cartesian mechanism" because of their unreliability.[10]

In a brilliant chapter on "Evidence and History," Jed and Moti explain how Newton turned "a morass of conflicting words into probative evidence."[11] While the reform of Aristotelianism was one of the key features of the 17th century, Newton regarded the reform of chronology as equally pressing. And here astronomical data became a key tool in demolishing the reliability of humanistic methods. Jed's excursion into the interface of the physical sciences and the humanities in *Newton and the Origin of Civilization* thus is not merely a study of how and why Newton handled ancient chronology. It is also, and perhaps more importantly, a penetrating study of how knowledge and the evidence upon which it is built were regarded as reliable and with what degree of certainty in an era that did not yet have probabilistic methods. By viewing chronologies as dealing with epistemological issues analogous to those that arise in laboratory measurements, Jed and Moti not only made Newton's chronology real as an historical object of investigation, but also relevant to the history of the physical sciences.

It is often said by anthropologists (especially the structuralists among them) that it is only by making something unfamiliar that we can understand it. That's certainly the message too of the Helmholtzian method of laboratory science, this "creation of scientific effects," as Jed has told the story. But as Jed recognized of his journey to understand Hertz, and as he and his co-authors made abundantly clear through their stories of the zodiac of Paris and Newton's chronology, the journey from the unfamiliar to the familiar is one carried by narrative. The "creation of historical effects" thus is not merely the introduction of novelty where it did not exist before or the coming-into-being of a historical object we had not previously known. It is also, and perhaps more importantly, the construction of a historical narrative, the telling of a story that we all want to hear. Thank you, Jed, for the creation of these historical effects.

9. Jed developed Newton's treatment of data in greater detail prior to the publication of this volume in an exemplary article on early modern data analysis, *Buchwald 2007a.*

10. *Buchwald and Feingold 2013a,* p. 93 and p. 306.

11. Ibid., p. 423.

Daniel J. Kevles

About Jed

I don't recall when I first met Jed—it must have been more than thirty years ago. Since then my admiration of the scholar and the man have grown apace, mixing awe with affection. His range of interests and expertise is stunning, running from Newton and methods to the wave theory of light and electromagnetism, and on to disputes over hieroglyphs in relationship to science and religion. Absorbed with the structure, logic, and epistemology of historical scientific systems, he has been concerned with issues of interest to philosophers to a degree that is unusual among historians. Yet he is a master of close, detailed study of technical physical theory as expressed through actual scientific practice. In recent years, his pursuit of the history of practice has led him increasingly to the institutional, economic, and technological environments of the scientific enterprise. He is a champion of attending to what scientists actually have done historically, as distinct from relying on what they said they did.

We are all in Jed's debt for his energetic encouragement of the history of science—as the Director of the Dibner Institute, the editor of at least two influential series of books, and generous encouragement of other scholars through the years, including here at Caltech. We are equally in his debt for his good company, his wry sense of humor, adventurous thinking, surprising observations—all often delivered in charming conversation around a hospitable, well-supplied table.

What I personally treasure about Jed is his boyish, irrepressible love of gadgets. Almost every time I've visited him, either at his Caltech office or his home with Diana in Pasadena, he has introduced me with delight to some new technology. I've come away hungering to possess one or another—a high-speed scanner, a powerful camera, a multitrack entertainment system that filled much of a room, a neat car. I've hungered for some because they struck me as useful, for others just because, even if useless to me, they were dazzling. Jed relishes them all, just as we cherish this man of so many parts at the celebration of his seventieth year.

Jane Maienschein

Jed Buchwald and the History of Biology

If asked to name those who have contributed most to the history of biology it is likely that almost nobody would name Jed Buchwald. After all, he is known as a leader in history of physics, technology, Einstein, Newton, and so much more. Yet most would not add history of biology. That would be a mistake. In fact, Jed has had a major impact on the history of biology because of his support of the Marine Biological Laboratory (MBL) through the Dibner Institute.

Jed's investment through the Dibner has led to a cluster of programs at the MBL. For 2019, we are working on two generous grants from the James S. McDonnell Foundation with the goal of "Putting History and Philosophy of Science to Work with the Life Sciences." Dozens of researchers are busy in several working groups doing just that. Again, this all started with work that Jed made possible.

In 1987, Garland Allen and I started a History of Biology course at the MBL in anticipation of their centennial the next year and expecting to offer a similar course every other year. The first course focused on genetics and development. Then, in 1989, we planned a second course, focused on neurobiology and behavior. A couple of months before its start, the MBL warned us that they were short of funds and we needed to find additional support. The new Dibner Fund for the history of science stepped up, and the director Sam Schweber helped us

out. He added further funding the following year. Then the Dibner Institute began, led by Jed.

From that point on, as long as the Dibner Institute continued, Jed provided the full support for what became an annual history of biology seminar at the MBL. Topics ranged widely across different fields of biology, and every year has brought different seminar leaders and participants. First Garland Allen and I served as directors, and then philosopher of biology John Beatty joined us. Then we added biologist James Collins. Gar eventually retired from running the course, but he attends every year and adds to the lively discussions. Then biologist-turned-historian of cell biology Karl Matlin joined us. Rick Creath often joined in, bringing his perspectives on logical empiricism to the mix. A diverse group of historians, philosophers, and biologists have come together every summer, often forging new collaborations and definitely stimulating new ideas. The seminar has completed thirty years and is heading for more, and has been supported by Arizona State University since the Dibner Institute closed.[1]

What did Jed contribute? Money? Yes. But more importantly, he provoked us to ask harder questions and push for deeper answers. He visited the seminar, at times bringing his children, and he (and they) asked hard questions. When we talked about history of conservation biology, for example, he wanted the presenters to dig more into the science and not settle for skimming the more accessible questions about policy and social impacts. He asked: what was the science, who was doing the work, and how was it used?

In conversations about the seminar, I have often found myself being pushed by Jed to do more and to do it better. I sat in his office a number of times, considering future seminar

1. See: https://cbs.asu.edu/mbl

topics and getting suggestions about possible leaders to bring different perspectives. Sometimes I still imagine him wondering why we are doing things the way we are.

The seminar was just the start. It led me to carry out considerable research on the history of science at the MBL, so that I became part of the MBL community, was named a Fellow, and developed an MBL History and Philosophy of Science program. That program received considered funding from the National Science Foundation, trained graduate students, and produced the MBL History Project open access website.[2] This work occurred in collaboration with Manfred Laubichler and has begun to include a computational HPS approach to research and graduate training.

The current McDonnell grant project picks up on the seeds planted with the seminar and related activities. It responds to Jed's urging us to connect history with the science. We are putting historians and philosophers of science to work with life scientists. This project has involved working groups exploring the impacts of imaging technologies, techniques, and their uses in the study of details of cell structure and function. This group worked with MBL researchers pushing the limits of light microscopy, research that began in World War II's innovations by Shinya Inoué, who turned gun barrels into microscopes.[3]

A second group asked questions regarding underlying historical and philosophical assumptions about biodiversity classification systems. How do species names change over time, and what computational tools and models can we use to track changes in diversity, the group asked. Both of these projects have led to ongoing discussion groups.

2. See: https://history.archives.mbl.edu

3. See: https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2172095/

A third group is looking at ideas of regeneration across the biological scales of organisms, ecosystems, and microbial communities. That project has developed into a set of five working groups studying regeneration: regeneration of neural function, stem cells, germ lines, ecosystems, and microbial communities.

In addition, we now have an annual endowed Friday Evening Lecture in the History and Philosophy of Science, for which Jed and Paul Hoyningen-Huene together gave the first talk reflecting on Thomas Kuhn's legacy for science. That lecture is named after Edmund Beecher Wilson, an early cell biologist at Columbia University and the MBL, who was also a serious cellist and called for understanding cells in terms of the physical structures. Jed and Wilson could have had lively discussions about how biology can learn from physics.

Little did we know in 1987 where that first MBL history course would lead. I'm confident that without Jed's financial but most importantly intellectual support, we could not have continued more than two or three years. Instead, we have had the fun of watching the history of biology grow in intriguing ways that respect the science while exploring larger historical questions.

Thanks Jed!

Mordechai Feingold

Hypotheses non fingo

It is never a waste of time to study the history of a word.
Such journeys, whether short or long, monotonous or varied, are always instructive.
Lucien Febvre,
Civilisation: Evolution of a Word and a Group of Ideas (1930)

It was during Jed's tenure as Director of the Dibner Institute at MIT that I developed my interest in the life, work, and legacy of Isaac Newton. Bernard Cohen, a regular guest at the Institute, was then finalizing his edition of the *Principia*—assisted by George Smith—and the numerous exchanges that took place there and elsewhere laid the ground for my *Newtonian Moment* and then my collaboration with Jed on *Newton and the Origin of Civilization*.[1] None of this would have happened without Jed's encouragement, and I can't think of a better way to pay him homage than by offering an essay that displays an attention to scholarly detail that always guides Jed's own research.

In 1962 Bernard published an article that sought to explore the precise meaning of Newton's celebrated "hypotheses non fingo."[2] Cohen took his cue from a brief analysis of the phrase

1. Mordechai Feingold, *The Newtonian Moment: Isaac Newton and the Making of Modern Culture* (Oxford, 2004). *Buchwald and Feingold 2013a*.

2. I. Bernard Cohen, "The First English Version of Newton's *Hypotheses non fingo*." *Isis* 53 (1962): 379–388.

made by Alexandre Koyré several years earlier. According to Koyré, Newton's "hypotheses non fingo" was intended to assure the reader that he had neither resorted to fictions nor used "false propositions as premises or explanations." And since "feign" suggests falsehood, whereas "frame" doesn't make it quite so implicit, Koyré was convinced that Newton "nowhere used the word 'frame', which is employed by Motte in his translation of the *Principia*." To bolster his contention, Koyré invoked Query 28 of the *Opticks*, where Newton wrote: "Later Philosophers banish the Consideration of such a Cause out of natural Philosophy, feigning Hypotheses for explaining all things mechanically, and referring other Causes to Metaphysicks: Whereas the main Business of Natural Philosophy is to argue from Phaenomena without feigning Hypotheses, and to deduce Causes from Effects, till we come to the very first Cause, which certainly is not mechanical." So convinced was Koyré of his interpretation that he bemoaned the great hold that Motte's "false interpretation" has exerted on Mme du Châtelet's equally misguided interpretation—"'Je n'imagine pas d'hypothèses' rather than 'Je ne feins pas d'hypothèses'"—while ignoring the fact that Du Châtelet translated from the Latin, and not from the English.[3]

Cohen was more cautious but he still contended that Newton neither sanctioned Motte's version nor employed the verb "frame" in the above sense. Indeed, Cohen believed that at the time the word "frame" was not "customarily applied to 'hypotheses'"—which brings me to the gist of this essay. My review of contemporary sources makes it clear that contemporaries, as well as Newton, made use of the phrase. In 1653, for example, Guy Holland described an "evil astronomer" as

3. Alexandre Koyré, *Newtonian Studies* (London, 1965), pp. 35–36; Isaac Newton, *Opticks*, 2nd ed. (London, 1718), p. 344.

one "who will not frame his Hypotheses according to his... celestiall apparences, but contrariwise, correct his apparences according to his *Hypotheses*."[4] Six years later, a translator of Gassendi's critique of astrology rendered the latter's "concinnandarum" in similar terms: When Epicurus "seems to deride Astronomical Curiosities, we ought to understand him only of the too scrupulous and unprofitable study of framing several Hypotheses or suppositions for the solution of the Apparences."[5] In the following year, Robert Boyle described an odd flash of light he had observed in the receiver of his air pump, a phenomenon he failed to explain—not least because he was "much discourag'd from venturing to frame an *Hypothesis* to give an account of it." Nearly half a quarter of a century later, Boyle suggested pleasantly that "some Naturalists and Physicians that delight to frame Hypotheses" might be interested in the experiments "to make Aurum fulminans" that he had carried out "for curiosities sake."[6]

Other examples abound. In 1666, the polemicist Samuel Parker proclaimed his partiality to the mechanical and experimental philosophy; he expected considerable progress from the experimental work of the Royal Society, through which "we shall see whether it be possible to frame any certain *Hypotheses* or no." At the same time, he scorned the symbolic knowledge of Pythagoreans and Platonists: "It does not only require a great deal of pains to frame conjectures of their meaning, but the surest we can pitch upon are withal so uncertain and ambiguous, that they unavoidably leave us

4. Guy Holland, *The Grand Prerogative of Human Nature* (London, 1653), p. 78.

5. Pierre Gassendi, *The Vanity of Judiciary Astrology* (London, 1659), p. 10. *Opera Omnia*, 6 vols. (Lyon, 1658) 1: 716.

6. The *Works of Robert Boyle*, Michael Hunter and Edward B. Davis, eds. 14 vols. (London, 1999–2000), 1: 265, 10: 91.

fluctuating, in mere uncertainties."[7] In October 1672, Henry Oldenburg published a letter from Thomas Platt detailing experiments conducted by Francesco Redi on the poison of vipers, which prompted him "to frame a new Hypothesis."[8] For his part, Thomas Baker jeered in 1699 at Descartes's attempt to establish his philosophy on matter and motion alone, an endeavor he considered as nothing other than "to frame *Hypotheses* out of one's own imagination, without consulting Nature." In the conclusion to the book, he added: "We frame to our selves New Theories of the World, and pretend to measure the Heavens by our Mathematical Skill."[9] John Locke also used the expression "to frame hypothesis" in his *An Essay Concerning Humane Understanding*,[10] as did Samuel Clarke in his 1704 Boyle lectures.[11] Noteworthy, too, is the ubiquity of the phrase in broader philosophical and theological context. Edward Stillingfleet found it unsurprising in 1685 that men differ "about the Beginnings of things, which are generally very obscure; and therefore thinking Men are apt to frame different Hypotheses about them."[12] In a similar vein, Samuel Parker castigated a decade and a half earlier those "proud and imperious men" who, at the start of the Reformation, "not

7. Samuel Parker, *A Free and Impartial Censure of the Platonick Philosophie* (Oxford, 1666), pp. 45, 48–49, and 81.

8. *Philosophical Transactions* 7 (1672): 5061.

9. Thomas Baker, *Reflections Upon Learning*. 2nd ed. (London, 1700), p. 81 and 236.

10. John Locke, *An Essay Concerning Humane Understanding* (London, 1690), p. 316. More often, Locke resorted to the pejorative "frame an idea."

11. Samuel Clarke, *A Demonstration of the Being and Attributes of God* (London, 1705), p. 6.

12. Edward Stillingfleet, *Fifty Sermons Preached Upon Several Occasions* (London, 1707), p. 372. In another sermon Stillingfleet added: "let Men frame what Hypothesis in Philosophy they please . . ." Ibid, p. 653.

regarding the Princes Power, took upon themselves to frame precise Hypotheses of Orthodoxy, and to set up their own Pedantick systems and Institutions for the Standards of Divine Truth." Equally contemptible were philosophers who "allow one another the Liberty, when they frame Theories and Hypotheses of things, to suppose some precarious Principles."[13] Arthur Burry believed that "no Sceptist can frame an Hypothesis comparably probable, to bring tidings to such remote Time and Place, by Twelve such Preachers."[14] To add yet another dimension, the American theologian Cotton Mather mocked those "Refined Wits," who "have Employ'd themselves, to frame Hypotheses, of the Methods of Nature, in which the Flood was brought about; until some of them fall into the Distemper, which Learned Men have wisely called an, *Hypothesimania*."[15]

Recourse to "feigning hypotheses" was much less common. An early, and unique, instance is found in Francis Bacon's *Essays*, where he recounts an opinion expressed in the Council of Trent: "the schoolmen were like astronomers, which did feign eccentrics and epicycles, and such engines of orbs, to save the phaenomena; though they knew there were no such things; and in like manner, that the schoolmen had framed a number of subtle and intricate axioms and theorems, to save the practice of the church."[16] But it was only toward the end of the 17th century that the term came into broader use. James Tyrrell, for example, utilized both terms when criticizing Thomas Hobbes in 1692:

13. Samuel Parker, *A Discourse of Ecclesiastical Politie* (London, 1670), pp. 56–57, p. 119.

14. Arthur Bury, *The Rational Deist Satisfy'd by a Just Account of the Gospel*. 2nd ed. (London, 1703), p. 76.

15. Cotton Mather, *Thoughts for the Day of Rain* (Boston, 1712), p. 9.

16. *The Works of Francis Bacon*, ed. Robert L. Ellis James Spedding and Douglas D. Heath. 7 vols. (London, 1887–1892), 6: 416.

> And therefore, tho I grant it is both lawful and usual for natural Philosophers, who not being able through the imbecility of our humane Faculties, to discover the true nature and essences of Bodies, or other Substances, do therefore take a liberty to feign or suppose such an Hypothesis, as they think will best suit with the nature of the things themselves, of which they intend to treat; and from thence to frame a body of natural Philosophy, or Physicks, as *Aristotle* of old, and Monsieur *Descartes*, in our age have performed: Yet can we not allow the same liberty in moral or practical Philosophy, as in speculative. And therefore such a precarious Hypothesis, as this of a natural state of War.[17]

Others followed suit. The die-hard Presbyterian John Edwards felt entitled in 1693 to dismiss Descartes's vortices, because the latter openly acknowledged that he had taken "the liberty to feign and invent" such a theory.[18] John Norris thought it best "not to feign a long Hypothesis of Sinners being admitted into Heaven,"[19] while Basil Kennett, Samuel Pufendorf's translator, rendered the latter's "hypothesi ficta" as "feign'd hypothesis."[20] As for John Toland, he concluded that those who had rejected the inherency of motion in matter were "very often oblig'd… to feign very ill-sorted and ridiculous Hypotheses."[21]

Newton tapped into this terminological repertoire. Within months of the publication in the *Philosophical Transactions* of

17. James Tyrrell, *A Brief Disquisition of the Law of Nature* (London, 1692), pp. 353–354.

18. John Edwards, *Brief remarks upon Mr Whiston's new theory of the Earth* (London, 1697), p. 39.

19. John Norris, *Christian Blessedness* (London, 1692), p. 166.

20. Samuel Pufendorf, *Of the Law of Nature and Nations*. Trans. Basil Kennett (London, 1703), p. 331.

21. John Toland, *Letters to Serena* (London, 1704), p. 172.

Moffat's above-mentioned letter, Newton injected the phrase into his own letter to the Secretary of the Royal Society: "Nor is it easier to frame an Hypothesis by assuming onely two originall colours rather than an indefinite variety."[22] However, as far as I know, Newton did not return to the metaphor before preparing the queries to the *Opticks* and then composing the second edition of the *Principia*.[23] Thus, query 20 of the 1706 *Optice* included "hypothesium commenta confingentes," with the translator, Samuel Clarke, adding in the preface—perhaps following Petrus Ramus—"non fictis Hypothesibus."[24] In subsequent years Newton paid growing attention to the phrase, in a manner that exhibits both an attempt to arrive at some precision, as well as an effort at textual variety—as Bacon and Tyrrell did. For example, in an early version of what would become query 28 of the 1718 English edition Newton wrote: "Later Philosophers banish the consideration of the supreme cause out of natural Philosophy framing Hypotheses for explaining all things without it & referring it to Metaphysicks; whereas the main business of natural Philosophy is to argue from effects to causes till we come to ye very first cause." A later draft incorporated the second phrase as well: "Later Philosophers… framing Hypotheses… whereas the main business of natural Philosophy is to argue from Phænomena without feigning Hypotheses…" Only in a subsequent reiteration did Newton decide to use "feigning" twice.[25]

22. *The Correspondence of Isaac Newton*, ed. H. W. Turnbull et al., 7 vols. (Cambridge, 1959–1977), 1: 264.

23. A possible exception may be found in a 1681 letter to Thomas Burnet: "As to Moses I do not think his description of ye creation either Philosophical or feigned." Newton, *Correspondence*, 2: 331.

24. Isaac Newton, *Optice* (London, 1706), p. 314, sig. A2.

25. Cambridge University Library, MS Add. 3970.3, fols. 249 bis, 247, 271.

It appears, then, that Newton's formulation of "hypotheses non fingo" followed his terminological struggle with the queries of the *Opticks*. And yet, despite his having settled on "feigning" for query 28, Newton did not naturally turn to the verb in his initial drafts of the General Scholium to the second edition of the *Principia*. In one version he wrote: "I have not yet been able to deduce the cause of these properties of gravity from the phenomena; and I do not follow [non sequor] hypotheses whether mechanical or of occult qualities." Newton originally wrote "I flee [fugio] from hypotheses," a verb he repeated in several drafts.[26] Only then did he decide to settle on "fingo."

Newton's wavering between alternative formulations was matched by early readers of the second edition of the *Principia*. John Maxwell, who translated the General Scholium in 1715, opted for "I do not Form *Hypotheses*."[27] In the same year William Derham pronounced: "What the Cause of Gravity is, Sir *Isaac Newton* doth not pretend to assign, his design being not to engage himself in framing *Hypotheses*, but to explain the *Phaenomena* by *Experiments* only."[28] William Whiston's version differed from both: "But the Cause of these Properties of Gravity I have not been able to draw from the Phaenomena: And I do not make Hypotheses."[29] Samuel Clarke took the same tack in his exchange with Leibniz: "And Hypotheses I make not."[30] For his part, the editor of Newton's posthumous *System of the World* proclaimed that the author "did not frame

26. I. Bernard Cohen, *Introduction to Newton's Principia* (Cambridge, Mass., 1971), pp. 242–243.

27. John Maxwell, *A Discourse Concerning God* (London, 1715), p. 105.

28. William Derham, *Astro-theology: Or, A Demonstration of the Being and Attributes of God* (London, 1715), p. 151.

29. William Whiston, *Sir Isaac Newton's Corollaries from his philosophy and chronology; in his own words* (London, 1729), p. 21.

30. Samuel Clarke, *A Collection of Papers which passed between . . . Mr. Leibnitz, and Dr. Clarke* (London, 1718), p. 357n.

hypotheses as other philosophers used to do, but set himself to examine the phenomena themselves, by mathematical reasoning."[31] Henry Pemberton followed suit. Before Newton, he wrote, the "custom was to frame conjectures."[32] As for the censorious Roger North, he charged the great man with the penchant "to frame hypotheses" while pretending "to decline them."[33] Hardly surprising, therefore, that Motte opted to translate "fingo" as "I frame," while translators into other vernaculars varied in their interpretations. Pierre Des Maizeaux rendered the phrase as "je ne fais point d'Hypothèses"—a version adopted by Voltaire—whereas Mme du Châtelet preferred "Je n'imagine pas d'hypothèse."[34]

What should we conclude from such a lexical odyssey? Newton wrestled to find terms that would convey his rejection of groundless, fictitious, hypotheses. Careful attention to his experimenting with various terms might contribute to a future refinement of his attitude toward hypotheses more generally. Conversely, contemporary translators and interpreters relied on the same repertoire of terms used by Newton in order to distil their understanding of the celebrated dictum. The extent to which their rendition was faithful to Newton's is, again, in need of further research.

31. Isaac Newton, *A Treatise of the System of the World* (London, 1728), p. xiii.

32. Henry Pemberton, *A View of Sir Isaac Newton's Philosophy* (London, 1728), p. 4.

33. BL MS ADD 32546 fol. 117^{v}.

34. Pierre Des Maizeaux, *Recueil de diverses pièces sur la philosophie, la religion naturelle, l'histoire . . .* 2 vols. (Amsterdam, 1720), 1: 191n; Isaac Newton, *Principes mathématiques de la philosophie naturelle*. Trans. Du Châtelet, 2 vols. (Paris, 1759), 2:179.

William R. Newman

On Difficult People

I first met Jed in the early 1990s when the Dibner Institute was being newly established at MIT. My most vivid memories of those days involve my first stay as a fellow at the DI, a year after its establishment. This was during the heyday of the "Science Wars," when social constructivism was the order of the day, and Shapin and Schaffer's *Leviathan and the Air Pump* was serving as a sort of litmus test for much of the history of science community.

As first director of the DI, Jed was immensely excited about the opportunity that his position gave him to provide funding for historians who wished to pursue an alternative course of scholarship more focused on the technical content of scientific texts. I was certainly one of those scholars, but my job in a lowly revolving door position at Harvard placed me in the belly of the beast, with the social historians ranked on one side and John Murdoch and Bashi Sabra on the other. For someone with no job security and few publications, this was not a comfortable situation.

I still have raw memories of the departmental colloquium series at Harvard, where my questions about content were routinely brushed away, or perhaps more commonly, where I was struck dumb by the speakers' blithe assumptions and total lack of interest about anything earlier than the 19th century. These memories should perhaps lie buried in their crypt except for the opportunity that they give me to acknowledge the happy times that I spent at the DI under Jed's leadership. I well recall his introductions to the Wednesday afternoon lunch seminars, where he made it clear that he had not only read and absorbed the individual speakers' works, but was visibly excited about them.

One of Jed's observations about the fellows particularly stuck in my mind. He was fond of noting that they could be "difficult people," but that this was simply something one had to work around in order to get to the novel insights that irritable scholars sometimes offered. In a word, this was a view of scholarship not as cult of personality or bandwagon-hopping but as knowledge, a position that has marked Jed's entire career and will no doubt continue to do so.

Liba Taub

Some Thoughts from the Ancient, and Not So Ancient, Past

As the organizers suggested, this conference is an excellent opportunity to reflect on foundational issues regarding developments in our field over recent decades. Of course, as we are all aware, history of science did not begin only a few decades ago, or even in the last century. Indeed, histories of science have been produced for many centuries, and date at least to Greek antiquity.

Herodotus, in his *Histories*, was primarily concerned with addressing the causes of the Greco-Persian Wars. Nevertheless, he touched on a wide range of other topics, offering a wealth of information, as well as his own views, on several subjects. For example, he discusses at some length various explanations of the seasonal flooding of the river Nile.

One of the hallmarks of the *Histories* is that Herodotus offers first-hand accounts from people living in different places, with different customs and points of view. However, regarding an explanation of the seasonal flooding of the Nile, he complained that he could get no information from Egyptian priests, or ordinary Egyptians. Herodotus recounted that

> what I particularly wished to know was why the water begins to rise at the summer solstice, continues to do so for a hundred days, and then falls again at the end of that period, so that it remains low throughout the winter until

> the summer solstice comes round again in the following year. Nobody in Egypt could give me any explanation of this, in spite of my constant attempts to find out what was the peculiar property which made the Nile behave in the opposite way to other rivers, and why—another point on which I hoped for information—it was the only river to cause no breezes.[1]

Herodotus went on to explain that while no one in Egypt could give any explanation, "certain Greeks, hoping to advertise how clever they are, have tried to account for the flooding of the Nile." The rather detailed analysis he then presents of at least three Greek explanations of the Nile's flooding is an indication as to what would constitute an acceptable elucidation of this particular phenomenon. Here, Herodotus may have been articulating a view, shared and developed by many later historians, that Greeks who wished to be thought clever had devised reasonable explanations of the world.

Aristotle is sometimes regarded as an early—if not the first—historian of science, partly because he provides so much information about his predecessors, being one of our principal sources about the ideas of the so-called Pre-Socratic philosophers. Of course, sometimes he reports others' views because he wants to argue against them, and show that his ideas are preferable (e.g. in the *Meteorology* 348a15-31, where he rejects another's explanation of hail). Whatever his original motivation, Aristotle's influence was profound; Leonid Zhmud has suggested that "the history of science belongs to the series of

1. Herodotus 2.19.2–3. Translation in A. de Selincourt and J. Marincola, *Herodotus: The Histories* (Penguin, 2003). This translation is used in what follows as well.

historiographical genres that emerged at the Lyceum" in the fourth century BCE.[2]

While I agree that the work done by Aristotle and the Peripatetics was crucial, I would argue that Herodotus deserves to be considered as an early example of an historian, and possibly even a philosopher, of science, for he not only reports on ideas and explanations about nature (*physis*) and natural phenomena, he is also concerned about methods and the reliability of scientific theories.[3] For example, in his discussion of Greeks who wished to be thought clever, Herodotus criticizes those explanations that do not accord with observation. Not only interested in ideas, Herodotus also provides information about scientific instruments, for example, reporting that the Greeks gained knowledge of the *polos* (sundial) and the *gnomon* (the shadow-caster) as well as the twelve parts of the day from Babylon (2.109).

In discussing the three different explanations of the flooding of the Nile offered by Greeks, Herodotus opines that "two of the explanations are not worth dwelling upon." He recounts that one of them maintains that "the summer north winds cause the water to rise by checking the flow of the current towards the sea." But, he counters that "in fact, however, these winds on many occasions have failed to blow, yet the Nile has risen as usual; moreover, if these winds were responsible for the rise, the other rivers which happen to run against them would certainly be affected in much the same way as the Nile."

2. Leonid Zhmud, *The Origin of the History of Science in Classical Antiquity*, trans. A. Chernoglazov (Walter de Gruyter, 2006), p. 10.

3. Herodotus may have been describing contemporary ideas, rather than explanations from an earlier time. While we might question whether he was interested in history of past science, his work demonstrates his concern with intellectual history, including his enquiries regarding explanations of natural phenomena.

He notes that "there are many such rivers in Syria and Libya, but none of them are affected in the same way as the Nile."[4]

He rejects the first explanation because it does not accord with observation and experience. He objects to the second explanation as well, that the Nile behaves in the way it does because it flows from Ocean, the stream mentioned by Homer.[5] Here, Herodotus's objection is that this account is less intelligent and seems to lack any factual basis; he complains that "I know myself of no river called Ocean, and can only suppose that Homer or some earlier poet invented the name and introduced it into poetry" (2.23.1). The final theory offered by the Greeks is that the water of the Nile comes from melting snow. For Herodotus, this theory is at the same time more plausible than the others, and yet it is also furthest from the truth. His objection is that the Nile flows from Libya through Ethiopia into Egypt, from a very hot climate to a cooler one. Since this is the case, how could the Nile possibly originate from snow?

After rejecting the theories of the Greeks, Herodotus then goes on to offer his own detailed explanation, based on his view that the position of the Sun (an important source of heat) is affected by storms: "during the winter the sun is driven out of his course by storms towards the upper parts of Libya." He argues that "it stands to reason that the country nearest to, and most directly under, the sun should be most short of water, and that the streams which feed the rivers in that neighborhood should most readily dry up" (2.24.1–2). Because the Nile is close to the course of the Sun it is more subject to the motions of the Sun than are other rivers, and behaves in a way that is completely different from that of other rivers. Herodotus's original question about the Nile's behavior linked it to astronomical events (namely, the summer solstice), and his own explanation of the Nile's flooding involves not only the river itself, but also the Sun.

4. Herodotus 2.20.1–3.

5. See, for example, Homer, *Iliad* 18.607; *Odyssey* 20.65.

Herodotus's discussion of the flooding of the Nile is striking in several ways. First of all, the level of detail given for the description of each explanation is significant, and is sufficient to not only bolster Herodotus's arguments against the validity of each explanation, but also to enable us to form our own opinions. Herodotus is not simply reporting his own explanation of the Nile, but providing the means to engage with—and judge—his thinking. He invites us to think through the problem with him. This sort of approach might be called by some 'internalist.'

Secondly, the discussion of different Greek explanations of the flooding of the Nile is presented not as an isolated piece of intellectual history, but is contextualised within a broader treatment of Greek life and culture, especially in terms of engagement with other ethnic, political, social, cultural, linguistic, and religious groups. Herodotus is concerned with the broader settings in which scientific theories have been developed and offered, an approach that might be deemed 'externalist.'

The very opening of the *Histories* makes it clear that the great and marvellous deeds of *both* the Greeks *and* the barbarians (that is, those who didn't speak Greek) will be considered. Notably, Herodotus singles out Greeks as being the only ones who were interested in offering explanations about the flooding of the Nile. His positioning of Greek thinkers in this way may itself have been the start of a long tradition of emphasising a Greek desire to find causes, for natural phenomena as well as historical events. Within the history of philosophy, the aim to explain and identify causes for phenomena such as the

Nile's flooding has been regarded as a signal contribution of the ancient Greeks. Indeed, some ancient Greek authors were of the view that their predecessors were responsible for the invention of philosophy itself.

In his opening paragraph, Herodotus refers to his work as *historia* (enquiry); he also talks about *aitiai* (causes). The aim of his histories (enquiries) is to identify causes. While, arguably, his primary objective was to explain the causes of events involving people (specifically, the Greco-Persian Wars), it is clear from his lengthy discussion of the Nile that he is also interested in explaining certain natural phenomena (for example, he goes on to discuss winds in some detail). Rosalind Thomas has argued that Herodotus's work should be seen, at least partially, within the "general milieu of debate, 'scientific' and philosophical exposition, the *koine* [what is shared, or common] of Greek intellectual life in the second half of the fifth century" BCE.[6] While we may not normally think of Herodotus as a philosopher, he arguably shared something of the same intellectual environment as that of the early Greek philosophers, and his great work was clearly driven by tremendous intellectual curiosity. His enquiries into nature were not simply a report of others' ideas, but a critical engagement with those ideas, demonstrating love of wisdom about nature.

The Greek word *historia*, in a very basic sense, meant 'enquiry.' Some of the ancient 'histories' and ancient 'historians' (including Herodotus and Thucydides) aimed at offering explanations based on their enquiries.[7] Similarly, *historia* about nature can be understood as focused not only on enquiry but also explanation. Thomas has argued that by

6. Rosalind Thomas, *Herodotus in Context: Ethnography, Science and the Art of Persuasion* (Cambridge University Press, 2000), p. 26.

7. Virginia J. Hunter, *Past and Process in Herodotus and Thucydides* (Princeton University Press, 1982/2017).

describing his work as *historie* (the Ionic form of *historia*), Herodotus was signalling to his contemporaries that "his work belonged in the world of scientific enquiry, whether it be into nature, or the nature of man" or, as in his own *Histories*, the "nature of the conflict between Greeks and barbarians."[8] To sum up, Herodotus's contributions as an historian of science are characterised by his detailed account of scientific theories, his contextualisation of these theories within broader intellectual and cultural landscapes, and his deep commitment to enquiry (*historia*) driven by far-ranging curiosity.

When I received the invitation to participate in this conference, I had been thinking about Herodotus, and whether he qualified as an historian of science.[9] Those characteristics of his treatment of scientific theories just highlighted have deep resonances with developments in the field over recent decades, and certain key thinkers exhibit some similarities in their approaches. Indeed, I would suggest that there are some parallels in the work of Jed Buchwald, an historian of science whose work has had far-reaching influence.

For example, in his books on Maxwellian electrodynamics and the rise of the wave theory of light, Buchwald noted the difficulties in reconstructing "dead theories" as he sought to "penetrate to the core of the issues," examining "a number of exemplary problems" and discussing "at some length the arguments."[10] As he himself noted, "until recent years most histories of physics have concentrated almost entirely on theory, with experiment appearing only at the edge." In welcome contrast, Buchwald emphasized the close connections

8. See note 6, p. 167.

9. I was working on the 'Introduction' to *The Cambridge Companion to Ancient Greek and Roman Science* (Cambridge University Press, forthcoming 2019).

10. *Buchwald 1985a* and *Buchwald 1989a*, p. xxii.

between theory and experiment, and his historical arguments required close attention to fine detail, to bring the "dead" theories and experiments to life.

Buchwald has also been committed to bringing the histories of science and technology "into closer contact with the philosophy of science," as he explained in the Introduction to the first volume of *Archimedes: New Studies in the History and Philosophy of Science and Technology*. There, he set out a challenge to the field: "It seems to me that we should look… for a novel philosophy of science, one that probes the nature of scientific work by grappling forthrightly and deeply with how it comes about that this particular form of human activity manages with such fecund regularity to produce novel entities that are inevitably bound to novel artifacts."[11]

And, even as Buchwald has produced important studies of the development of scientific concepts and the instruments used to create and explore new effects, he has also focused his attention on wider intellectual, cultural, social and political contexts. He has for some time worked on how scientists in the 18th and 19th centuries engaged with new archeological discoveries. His work with Diane Greco Josefowicz on *The Zodiac of Paris* (2010) was a result of what may at first have seemed like a chance discovery made whilst travelling, of a bound collection of pamphlets on the Dendera zodiac. The sub-title of the volume highlights the theme of their study: "how an improbable controversy over an ancient Egyptian artefact provoked a modern debate between religion and science."[12] Some others may have noticed that Buchwald, like Herodotus, is fascinated by ancient history (he has a self-proclaimed interest in

11. *Buchwald 1996b*, p. vii and p. ix.

12. *Buchwald and Greco Josefowicz 2010a*. Here, we are reminded of Herodotus's own travels in Egypt, and of his questioning of Egyptian priests regarding the cause of the flooding of the Nile.

the History of Understanding of Pre-Classical Antiquity), and Egypt in particular.

Buchwald's various interests led to a collaboration with Mordechai Feingold that resulted in their *Newton and the Origin of Civilization*.[13] They studied Isaac Newton's *Chronology of Ancient Kingdoms Amended*, published in 1728, and his attempts to date the past using astronomical evidence. Intriguingly, at the end of the Introduction to the work, Newton explained that "I have drawn up the following Chronological Table, so as to make Chronology suit with the Course of Nature, with Astronomy, with Sacred History, with Herodotus the Father of History, and with itself."[14] Newton used astronomy as the principal tool for re-establishing the chronologies of the early Greeks, as well as the Egyptian, Assyrian, Babylonian, Median, and Persian empires. Buchwald and Feingold argue that for Newton "a single conception of the probing character of *human knowledge* bound together a Newtonian triad of history, theology, and science."[15] Much of Buchwald's work—and these latter two books in particular—demonstrates his own deep engagement with understanding human knowledge, by studying the place of scientific work within broader culture. And, above all, Buchwald's work attests to his extraordinarily wide-ranging intellectual curiosity.

In recent decades we have seen exemplary work done in history of science that has engaged closely with the origins and

13. *Buchwald and Feingold 2013a*.

14. Isaac Newton, *The chronology of ancient kingdoms amended: to which is prefix'd, a short chronicle from the first memory of things in Europe, to the conquest of Persia by Alexander the Great* (London: Printed for J. Tonson in the Strand, and J. Osborn and T. Longman in Paternoster Row, 1728), p. 8.

15. *Buchwald and Feingold 2013a*, p. 4. J. L Myres, *Herodotus: Father of History* (Clarendon Press, 1953), p. 43, pointed to Herodotus's emphasis on "the collection of facts about Man, and in the interpretation of them."

foundations of science, and with intellectual, cultural, and political history more broadly. What does this suggest for the future of our field? The continuing importance of producing finely-grained, detailed studies that engage deeply with questions relating to human knowledge and research that is driven by intellectual curiosity and engagement with philosophy. In other words, history of science begins with and is driven by *historia*, that is, enquiry.

Giora Hon

The Art of Thinking History in Science

Let us move straight to the heart of the matter as the story has been well told and we need not rehearse it again. Robert A. Millikan (1868–1953), the first president of the California Institute of Technology, and Felix Ehrenhaft (1879–1952), the Director of the Physical Institute at the University of Vienna before and after WWII, developed a long running controversy over the nature of the electric charge: is it discrete and hence fundamental or is it continuous and can thus assume any value? Much has been written about this case. For my purpose here I wish to draw attention to two contrasting experimental methodologies which reflect the distinction between sensitivity and optimization.

Ehrenhaft sought to increase the sensitivity of his apparatus by observing ultra-microscopic metal particles whose radii were of the order of 10^{-5} cm. "I set myself the problem," he stated, "of measuring the electric charge on the smallest possible individual particles and thereby subjected the foundations of the electron theory to the sharpest test." Assuming that Stokes's law for the resistance to the motion of a sphere in a viscous fluid holds for these particles, Ehrenhaft found that the particles he investigated carried electric charges smaller than 1×10^{-10} esu. In contrast to Millikan, who experimented with relatively large oil drops with radii varying from 2.5×10^{-5} cm to 23×10^{-5} cm, Ehrenhaft focused his research on particles whose radii varied between 0.8×10^{-5} cm to 2.5×10^{-5} cm.

Ehrenhaft reached the conclusion that "test bodies below a radius of 3×10^{-5} cm often carry charges smaller than that of the electron and, hence, that the charge on test bodies decreases on the average with their capacity (and hence radius). In this connexion charges of the order of a tenth or twentieth part of the electron's are by no means the smallest." Ehrenhaft did not seek to simplify his apparatus with a view to obtaining optimal conditions; he took rather the opposite approach of making his experiments more and more sensitive, and observed the falling particles under extreme conditions.

Millikan's apparatus was somewhat crude in comparison with Ehrenhaft's, but this feature was precisely in line with Millikan's approach: his observations were not taken under extreme conditions when one cannot be sure of the processes taking place. Millikan sought to create optimal conditions when the interferences caused by other phenomena are minimal and under control. As Millikan remarked, Ehrenhaft had made his observations with an ultra-microscope and determined the rates of fall and rise of particles investigated through exceedingly minute distances of about 0.01 cm, in contrast to the 1.3 cm distance of his own oil-drop method. Millikan in fact attributed the accuracy and consistency of his results to the oil drop's relatively long distance of fall and to the smallness of the required magnification.

Both Millikan and Ehrenhaft claimed that their results were not just a "statistical mean," since they studied specific properties of individual particles. Indeed, Millikan thought that his result constituted proof that the unit charge was a real physical entity. What we witness here are two contrasting conceptions of experimentation with diverging results—hence the controversy.

Enter Jed.

While Ehrenhaft and Millikan argued over sensitivity and optimization, Jed reconciles the two contrasting approaches. I find Jed's accomplishment in history and philosophy of science both sensitive and optimal. Jed's art of historical thinking is, I suggest, the miraculous combination of great sensitivity to minute technical details—call it the particular—and the optimal approach which offers the view from above, the general, the approach that brings a myriad details into a coherent and convincing whole. Jed has taught us that technical details are of prime importance when history of science is concerned. One should first master the details when one embarks on the writing of the history of a scientific episode, and only then offer a thesis that optimally places the details of the episode in a general framework with a view to obtaining insights into the generation of scientific knowledge.

But this is not the end of it. There is another aspect to Jed's teaching of the art of thinking history in science, namely, the advancement of learning. This is Jed's professional commitment to the community of scholars. To be sure, I speak for myself, but I am confident that I express the experience of many fellow historians and philosophers who have worked with Jed and benefitted from his harsh critique and immense erudition. Again, miraculously, this professional commitment, however sharp and critical, is thoroughly amicable. Upon being presented with an idea, Jed will promptly respond, immediately subjecting the idea to a thorough criticism with the goal of identifying weaknesses in the details. But once the particular is set aright and the general claim is approved, Jed will forcefully promote the idea for the benefit of both the individual scholar and the community at large.

I thank you, Jed, for these lessons and for the constant scholarly support.

Michael D. Gordin

Reading Jed Reading

"Post-modernism and Benoit Mandelbrot have found their way to the history of science."[1] These were the first words I ever read from Jed Z. Buchwald's pen. Never having studied with him directly, for a long time I only knew him through the written word. As it happens, the words I initially found came from a book review. This encounter proved to be auspicious: it has shaped my appreciation of Jed over the years both on the page and across the table. As I know many others will honor Jed's monographs and articles with the praise they deserve, I thought it not unseemly to focus on this particular short piece because it proved so formative for how I think about the discipline.

I am an enormous fan of book reviews, perhaps the most neglected and maligned genre in the historical profession. They irritate many tasked with their composition due to the genre constraints. They tend to be short (although at ten pages Jed's wasn't), and that forces the writer to an economy of expression that can—and often does—have the effect of stunting original thought. Their compression is precisely what I love about them. In the right hands, the authors of reviews do something that the authors of the books in question often cannot: they show you how they read. When I first read Jed's writing, he was writing about reading. Although there were

1. *Buchwald 1991*, p. 85. Further citations are to this text.

probably many other people whom I first read in the columnar confines of the book review, I honestly can't recall them. Jed's review stuck with me, as it did with many others, for showing how to read a book that many readers in the profession would have approached only with trepidation.

The quotation is, of course, the first line of Jed's essay review of Crosbie Smith and M. Norton Wise's *Energy and Empire: A Biographical Study of Lord Kelvin* (1989). The review came out in the *British Journal for the History of Science* in 1991, and the two-year lag says something about the care with which Jed read—terrifically closely and multiple times, as he states in the review—this almost 900-page tome containing everything that we can know about William Thomson and his world, demanding the reader comprehend the equations of electromagnetism as well as the nuances of Scottish theology. There are few readers who would brave the text without a guide, and Jed was of course one of the intrepid. His review in turn became that guide which has been indispensable for every reader of Smith and Wise since.

Jed's capacities as Virgil to the world of Lord Kelvin was not, however, what I initially found so captivating. It was rather the glimpse Jed gave as to how he approached a text. The review operates on three levels. The core is a careful and detailed analysis of a few key examples from the book, parsed not in terms of how much he likes or doesn't like the claim but in terms of how the evidence holds up, illustrated with extensive quotations and close reading. ("This is the authors' strongest evidence of this sort from Thomson's own pen, but it is very nearly unique" [90].) One level up, he presents the authors' project in their own terms, without prejudice, in order that the reader can see how the evidence at the micro-scale connects with the architecture of whole. ("These [the "two central characteristics of Thomson's early physics"], argue Smith and Wise, are both part of a single mathematical thrust that in turn connects to prior convictions that are grounded in a religio-political amalgam of voluntarist theology and abhorrence of partisanship, with connections to beliefs about progression and industrial economy" [86].) A model of concision.

The third level, the one that made this review remarkable for me and for so many I have talked to about it, is encapsulated in the opening line. Jed Buchwald invokes "post-modernism," but not as a term of abuse. On the contrary, the term is Jed's pithy way of characterizing Smith and Wise's narrative strategy of juxtaposing distinct realms of Thomson's thought and action without invoking causality or intention. This astute point frames the book as a narrative project that necessarily—not least because of the absence of much direct first-person evidence—subverts many of the assumptions of histories of science, and of biographies most of all. This is where Mandelbrot comes in: Jed famously characterized Smith and Wise's layering of culture and physics without causation as "the *fractal model for history* since it replicates the same pattern at every scale of complexity" [87]. This methodological point goes beyond the text in question and illustrates how close attention to the broad sweep of contemporary (to us, not to the historical subjects) scientific developments can serve as a resource for understanding the craft of writing.

This is how Jed reads. It pays the highest compliment to authors by taking them as seriously as they took their own research. It is the same compliment he pays to Heinrich Hertz, James Clerk Maxwell, and Isaac Newton. In his historical scholarship he reads his primary sources in the same graduated way he read Smith and Wise: the evidenced example, the writer's intentions, and the methodological implication.

His reading practice gives texture to his scholarly accounts (even though he has not yet embarked on fractal histories of his own, so far as I have been able to detect).

My next encounter with Jed reading was as an avid consumer of the books he published in his series with MIT Press, *Transformations: Studies in the History of Science and Technology*. The labor that goes into curating a book series is thankless and, aside from the byline in the front-matter, invisible. (In this, series resemble book reviews, but they are vastly harder work.) For almost twenty years, through some magic of discrimination and taste, Jed has produced the single best monograph series in the discipline. All the ones I have read (a large subset) are superb. Distinctive here is the sheer breadth of what is covered, both in terms of subject matter *and* methodology. It seems that nobody could judge authoritatively about all these different areas, that nobody could read so widely, yet Jed obviously does. These books do not read like clones of what their editor does in his own scholarship. Rather, *Transformations* shows us once again Jed Buchwald as an impeccable reader: he takes these manuscripts and coaxes them into being the best scholarship the authors can produce.

Jed's readerly instincts are once more—dare one say "fractally"?—on display in his latest ventures into the history of antiquarian scholarship, first in his fascinating *Zodiac of Paris* (written with Diane Greco Josefowicz), and again his current research on Champollion and the decipherment of hieroglyphs. Reading Jed reading these materials is an education in itself. While I was working on a book about Immanuel Velikovsky, a shambolic reader of Mesopotamian and Egyptian texts (in translation)—and who was active around the campus of Princeton University when Jed was an undergraduate—we began a continuing conversation about how we today understand the scholarship produced during the past two centuries about deep antiquity. Jed converses like he reads: carefully, with conviction, and with more than a dash of puckish wit. Post-post-modernism has found its way to the Buchwald oeuvre.

Myles W. Jackson

Ode to Jed

I met Jed on one snowy afternoon in Cambridge, Massachusetts, back in February of 1993. I had finished my PhD with Simon Schaffer two years before, and I was Peter Galison's postdoctoral fellow at Harvard at the time. I had received an email from Jed saying that he had read a piece of mine on Goethe's theory of color. So, I braved the weather and dared to traverse the infinitely long distance between Harvard's Department of History of Science and MIT's Dibner Institute for the History of Science and Technology, where I met Jed and "tugged at me lock." He noted that my article on Goethe was not "half bad"—since all of the others he had read were "worthless." Having only known of Jed from various campfire stories that one tells when one wants to scare the daylights out of folks, I actually thought that this might be a very vaguely concealed complement. He told me that of course my article could be vastly improved, but it did make a few interesting points. That meeting would change my life (yes, for the better) far more than I could ever have imagined or indeed could have hoped for. It sparked a collaboration and indeed I may dare say a dear friendship, which has endured some quarter of a century later.

Jed has published all three of my books in his series Transformations: Studies in the History of Science and Technology with MIT Press. And I must admit, I have never had anyone read my work so closely as Jed has—all three times. His marginalia themselves are worthy of publication. "This makes no sense." "You do not understand this." "Myles, old boy, what the Hell does this mean?" One might not realize, however, that Jed is harsh because he is actually very caring. He deeply cares about expertise, not only the skills of rational reconstruction of the technical knowledge of physics for which Jed is so justifiably renowned, but also for close readings. I think many of us tend to forget that his ability here would put most 19th-century German philologists to shame.

I was fortunate enough to be named the Francis Bacon Visiting Professor of the History of Science and Technology at Caltech back during the winter and spring terms in 2012. While there I had unprecedented support for a conference on gene patenting. I was able to bring together a number of leading scholars in various fields, including molecular biology and the biomedical sciences, history, and the law. The papers of the conference became the basis of a very successful volume of *Perspectives in Science*. Such productive interdisciplinarity was a result of Jed's ability to raise funds for the Francis Bacon Professorship.

It has been fascinating to watch Jed's scholarship over the last three decades. On the one hand, he has migrated from the history of technical physics of the late 19th century to the history of the zodiac. I never forget when Jed told me about his new project with Diane Greco Josefowicz about a decade ago. I smiled and said, "Welcome to the cultural history of science, Jed." He paused and retorted: "Yes, but it is a technical cultural. I actually read the books." Back in the 1980s and 1990s, I think it is fair to say that Jed was rather skeptical about the types of histories coming out of Cambridge, UK. But he did pull some of us aside (Rob Iliffe, Andy Warwick and me) and engaged with us. He loved the sparring: and believe me, he tested your mettle. If the strength of a sword is a function of

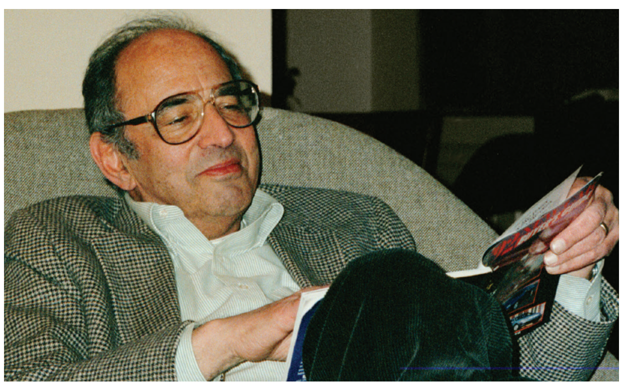
the heat of the furnace in which it was forged, I think I possess Excalibur! And it was truly sad to see the usurpation of the history of science but what has been called "cultural studies" during the 1990s and 2000s. Jed in a sense no longer wanted to have a battle. I agreed with him: it just was not worth the fight. But I did enjoy asking Jed if he could in his wildest dreams ever image nostalgically reminiscing about Simon Schaffer and his students.

On the other hand, there are important ways in which Jed's work has very much stayed the same over a period of three decades. First, as mentioned above, he is an incredibly close and careful reader of texts. Second, he is very disciplined about notions of causality. He is quick to put out when the contextual analysis is merely window dressing and possesses no explanatory power over the content of the technical, scientific knowledge. Juxtaposing bits of culture and science that were occurring simultaneously, and which might appear to be analogous, is a necessary yet woefully insufficient historiography to demonstrate causality. I think that was the hallmark of his famous review of Crosbie Smith and M. Norton Wise's *Energy and Empire* (*Buchwald 1991*). And I would argue it plagues more recent scholarship as well. Historical explanations must do work; otherwise, the role of the historian is trivialized to telling cocktail party stories to scientists. Jed is so demanding precisely because the very future of this field is at stake.

Jed is of course most famous for his technical history of physical. There is the fear, which many of us share, that this type of history writing will soon die out if the practitioners, who continue in the tradition of rational reconstruction, are no longer supported. And that would be very sad. His technical work on the wave theory of light in 19th-century France was an important contribution to my early work on Fraunhofer's artisanal optics. His work on Hertz's experiments played a pivotal role in the genre of the history of experimentation of the late 1980s and 1990s.

His legacy will also undoubtedly be shaped by his edited series, *Transformations*. The range of that series, which in my view is the best series we have right now in the history of science, is truly impressive. To wit, Jed is actually much more open to historical methodologies than one might have thought. He is a stickler for saying what one means, and meaning what one says. And, the arguments must be backed up with substantial, credible archival evidence. Jed's editorial inventions make us all better historians, better scholars, and indeed better humans.

I honestly do not know where this discipline is going. I do, however, know that Jed's contributions to it have been multiple and transforming. Jed, may the next seventy years be as rewarding for you and all of us as the first seventy!



Thomas S. Kuhn (1922–1996) PHOTO COPYRIGHT JED Z. BUCHWALD

Paul Hoyningen-Huene

Philosophy, When Possible and Desirable

I do not remember exactly, of course, when I first heard of Jed's existence. Most probably, it was in 1992 or 1993 after he became Director of the Dibner Institute for the History of Science and Technology at MIT, during which time I was in frequent contact with Thomas Kuhn, his Princeton teacher. At any rate, I will certainly not forget the occasion when I first—as far as I remember—met Jed in person. This was immediately after the end of a book symposium on my *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* at the 1994 HSS and PSA conference. Together with the other symposiasts, I was heading for a nearby bar in order to celebrate what I thought was a successful event. Right after the lecture hall's door, Jed stopped me and asked me whether I had some time to discuss something. I told him that this was an unfortunate moment, but he insisted and told me about the planned *Archimedes* book series, and asked me whether I would be willing to join its editorial board. Of course, I was, and I immediately realized that Jed had waited with this invitation after he saw me on stage together with historians of science at the symposium.

By 1994, I had realized fully the difficulties of cooperation between philosophers and historians of science, especially from the historians' point of view. Philosophers of science often do not fully understand how different the historical approach to science is; and historians of science sometimes find discussions with philosophers of science tiresome. However, I had hoped that my engagement with Kuhn's work and, in addition, frequent discussions with Paul Feyerabend, enabled me to do better. I wanted the historical perspective on science to be a fundamental and integral part of my philosophical work. So I took Jed's invitation as an indication that historians of science had accepted me as a philosophical partner in our cross-disciplinary discourse, and I was very happy about that.

Jed's reluctance with philosophers of science is elegantly expressed in the first sentence of his introduction to the first volume of *Archimedes*:

> ARCHIMEDES [...] has three fundamental goals: to further the integration of the histories of science and technology with one another; to investigate the technical, social and practical histories of specific developments in science and technology; and finally, where possible and desirable, to bring the histories of science and technology into closer contact with the philosophy of science (*Buchwald 1996b*, p. vii).

In other words, it is not always possible nor is it always desirable to talk to philosophers of science about the histories of science and technology.

After this event, Jed and I met on various occasions, mostly at conferences, and we always had very friendly shorter or longer conversations about the state of history and philosophy of science, more often than not in a rather critical tone. We were both unhappy that rigorous scholarship seemed to decline in some areas of our disciplines, and I considered it a great privilege to be able to have my amateurish impressions of the history of science examined by his professional experience. I remember very well when at a conference in Durham, Jed explained to me his intellectual standards as a historian

of science. He would only begin to write about a particular episode in the history of science if he could consider himself a legitimate (virtual) member of the respective scientific community. This is, it seems to me, how it must be.

Our most extensive and most remarkable encounter was in July 2012 when we spent a couple days at the Marine Biology Laboratory (MBL) at Woods Hole, Mass. We were invited to give the famous Friday Evening Lectures on the occasion of the 50th anniversary of the publication of Thomas Kuhn's *The Structure of Scientific Revolutions*. It was apparently for the first time since 1890, when the Lectures were established, that two people spoke, and it was for the first time that the subject matter was exclusively focused on history and philosophy of science. It was a wonderful event in front of an audience of more than 500 people. The next day, Jed and I were interviewed by a radio station on Kuhn's philosophy and history of science, and there I experienced what a fantastic team player Jed was. In his contributions, he often referred back to my contributions, and I must admit that only after realizing how friendly and effective this sort of mutual reference was, did I start to do the same. This was the proof: practitioners of the history of science and of the philosophy of science are able to cooperate, after all.

Jed, your birthday is an excellent occasion not only to celebrate you, but also our friendship. I am extremely grateful for all the conversations we have had, and I have no doubt that we will continue this fruitful exchange for quite some time to come.

Elaheh Kheirandish

'Zodiacs of Paris' Revisited: Verses, Places, and Faces

The zodiac of paris being one of the many outstanding publications of Jed Buchwald,[1] a *liber amicorum* in his name provides a fitting occasion to revisit the subject through other rare and notable historical sources that in the case of the present selection share the same subject (zodiacs), and current location (Paris), however different their medium and period. My selection highlights three little-known and understudied texts from the Islamic Middle Ages which involve not only one-of-a-kind features and contexts, as in the 'Zodiac' of our distinguished scholar, but also subjects within my own specialty areas long supported and advanced by him as the director of the Dibner Institute, and editor of the Springer Verlag's *Sources* series.[2] Below is a discussion of each selection, followed by translations and reproductions, as applicable, in corresponding appendices (I–III). Images reproduced from one of these for the present occasion complement my essay as

1. *Buchwald and Greco Josefowicz 2010a*.

2. Jed Buchwald was the director of The Dibner Institute for the History of Science and Technology during my postdoctoral fellowship years in 1996–1997 and 2001–2002, and on the editorial board of the Springer Verlag series: *Sources and Studies in the History of Mathematics and. Physical Sciences* during the publication of my two-volume book: *The Arabic Version of Euclid's Optics*, New York: 1999.

a visual gift for this volume, offered along with the original artwork.[3]

I. Zodiacs and Verses

The infamous 'Paris codex' (BNF Pers 169),[4] a collection of early scientific manuscripts in Persian and Arabic has long attracted the attention of historians mostly due to an 'anonymous compendium' of 61 repeat geometric patterns, with descriptions—not just images—recently published as part of a collected volume by me and my colleagues.[5]

A single folio in that same codex, tucked in among the other 24 unpublished items of that sizable volume of close to 200 folios, is striking for its own historical rarities and leads.

A twenty four-line astronomical poem naming its author as the 'King of Scholars Naṣīr al-Dīn Ṭūsī' (d. ca. 672/1274), the founder and director of the Maragha Observatory in North-West Persia, is a rarity in itself by treating the twelve zodiac constellations with reference to the Moon and their joint effects, all in rhymed verse.[6] But the formerly unnoticed text has more notably led me to the discovery of another versed treatment of zodiac constellations in Persian attributed to the

3. The original artwork involved MS reproduction (App. III: tracing and threading by M. Shah Mohammadi)

4. Paris, Bibliothèque nationale de France (BNF), Manuscript Persan =MS Pers.169.

5. *The Arts of Ornamental Geometry: A Persian Compendium on Similar and Complementary Interlocking Figures: Fī Tadākhul al-ashkāl al-mutashābiha aw al-mutawāfiqa* (Bibliothèque nationale de France, Ms. Persan 169, fols. 180r–199r). Edited by Gülru Necipoğlu with Contributions by Jan P. Hogendijk, Elaheh Kheirandish, Gülru Necipoğlu, Alpay Özdural, and Wheeler M. Thackston. Leiden and Boston: Brill, 2017, winner of the *26th World Award for Book of the Year*, Iran, 2019.

6. Paris, Bibliothèque nationale de France, MS. Persan 169: 140a–140b: *Ikhtiyārāt-i masīr-i al-qamar min kalām-i Sulṭān al-Ḥukamā'*. The poem is uncommonly set in three, rather than two, columns for each verse.

same author, and occasionally one of his contemporaries, this time corresponding each of the twelve zodiacs to all the seven ‘planets’ of an old cosmology in a pre-Copernican, Ptolemaic order, verse by verse.

The latter versed treatment in twelve lines is part of a long and rich ‘Introduction’ (Madkhal) to astronomy, which regardless of the ‘Tūsī’ attribution in the Paris codex that led me to it,[7] is a remarkable source with historical interests beyond its rare features. Treatments of the zodiacs in early historical sources are by no means rare, whether texts ranging from Greek and Latin to Arabic and Persian,[8] or objects from decorative to instructive anywhere from bowls to globes;[9] nor textual sources in verse instead of prose, the genre more common to early scientific texts. I have published eight such versified cases as ‘astronomical poems from the four corners of Persia (ca. 1000–1500)’,[10] featuring the twelve zodiac

7. Poetry and poetry-writing in the works of Naṣīr al–Dīn Ṭūsī includes a ‘Madkhal-i Manẓūm’ (Versed Introduction) on astronomy: see *Shiʻr va Shāʻirī dar Āthār-i Khwājah Naṣīr al-Dīn Ṭūsī: Bi Inḍimām-i Majmūʻah-i Ashʻār-i Fārsī-i Khwājah Naṣīr va Matn-i Kāmil va Munaqqah-i Miʻyār al-Ashʻār* by Iqbālī Aʻẓam, Muʻaẓẓama. Tehran: Sāzmān–i Chāp va Intishārāt–i Vizārat–i Farhang va Irshād–i Islāmī, 1379/ 2000, p. 98 (p. 96 cites three manuscripts and one edition outside Paris, including one non-Ṭūsī attribution).

8. Grasshoff, Gerd. *The History of Ptolemy’s Star Catalogue*, New York, NY: Springer New York (Studies in the History of Mathematics and Physical Sciences 14.), 1990. *Painting the Stars in A Century of Change: A Thirteenth-century Copy* of al-Ṣūfīs *Treatise on the Fixed Stars: British Library Or.5323*: Moya Catherine Carey, School of Oriental and African Studies PhD thesis 2001, 2 parts (unpublished).

9. Varjāvand, Parvīz. *Kāvush-i Raṣadkhānah-i Marāgh*a. Tehran: Amīr Kabīr, 1366/1987, pp. 242, 322: includes cases like glazed bowls and celestial globes.

10. Kheirandish, Elaheh. ‘Astronomical Poems from the “Four Corners” of Persia (c. 1000–1500 CE)’, *Essays in Islamic Philology, History, and Philosophy*. Edited by Alireza Korangy, Wheeler M. Thackston, Roy P.

constellations and seven planets, sometimes jointly, where the zodiacs are associated with subjects ranging from literary classics to bodily organs. What is rarely encountered is a treatment of not just both the zodiacs and planets in one and the same poem, but one with a one-to-one correspondence between them, all in verse.

Between the two Persian ‘zodiacs in verse’—the twenty-four-line ‘zodiac of Paris’ featuring the Moon and the twelve zodiac mansions, and the twelve-line excerpt from the ‘Versed introduction’ coupling the zodiacs with the planets in manuscript libraries outside of Paris, the latter is featured here for being historically more important, and supplied with the original Persian and English translation in appendix I. There, lines of verse on configuration and designation of the stars (lines 1–12) and enumeration and sequence of zodiac constellations (lines 13–16), which I had partly published before,[11] are refined and extended, and followed by verses where a direct correspondence is uniquely made between one or two of the zodiac constellations, and one of the seven ancient ‘planets’ (lines 17–24): Aries and Scorpio with Mars; Taurus and Libra with Venus; Gemini and Virgo with Mercury; Cancer with the Moon, Leo with the Sun; Sagittarius and Pisces with Jupiter; and Capricorn and Aquarius with Saturn.

II. Zodiacs and Places

While the ‘zodiac of Paris’ opening the selection above corresponds the zodiacs to the planets, the one treated next has a feature no less rare in linking them with places, this time no

Mottahedeh and William Granara, Berlin/Boston: Walter de Gruyter GmbH: *Studies in the History and Culture of the Middle East* 31, 2016, pp. 51–90.

11. Kheirandish, ‘Astronomical Poems from the “Four Corners” of Persia (c. 1000–1500 CE)’, Appendix VI.

longer in the sky above, but on the earth below, from cities to regions. This is in a work by Abū Rayḥān Bīrūnī (d. after 442–1050), titled the *Book of Instructions* (*Kitāb Tafhīm*),[12] a work with illustrations as well as discussions of the twelve zodiac constellations, and more than one manuscript currently housed in Paris.[13] The text has many unique features, from compositions in both Arabic and Persian by one and the same author, to dedication to a female patron,[14] however unknown her name and associations.

Among various notable features of this text, which includes mathematical subjects from arithmetic and geometry to astronomy and astrology, there is a table that corresponds the twelve zodiac constellations to specific 'cities and regions'.[15] In the context of the present occasion, namely the 70th birthday of Jed Buchwald, it is worth noting that his birth sign of Taurus corresponds to the city of Alexandria placed at the center of the first seven places cited, which at the time Bīrūnī wrote, had long been integrated into Arab-speaking Egypt. But at the time of Ptolemy, and his influential 'Star Catalogue,'[16] Alexandria was still linked to ancient Greek scientific traditions, and other Alexandrian authors like Euclid and Pappus, before and

12. Bīrūnī, Abū Rayḥān Muḥammad ibn Aḥmad, (sometimes dated ca. 363–440/973–1048). *Kitāb Tafhīm li-awāʾil ṣināʿat al-tanjīm*: Arabic: *The book of instruction in the elements of the art of astrology,* London: Luzac, 1934; Persian: Jalāl al-Dīn Humāʾī. Tehran: Anjuman-i Ā<u>s</u> ār-i Millī, 1974.

13. Rozenfel'd, Boris. A. and Ekmeleddin İhsanoğlu. *Mathematicians, Astronomers, and Other Scholars of Islamic Civilization and Their Works (7th–19th c.)*. Istanbul: Research, 2003, pp. 144–155, Arabic: A2: p. 148; Persian: A3, p. 149.

14. Rayḥāna bint al-Ḥasan may have been a princess in the Persian court whose identity remains unknown.

15. Wright, *Book of instruction in the elements of the art of astrology*, p. 355; Humāʾī, *Kitāb Tafhīm,* p. 335.

16. Grasshoff, *The History of Ptolemy's Star Catalogue.*

after Ptolemy, those whose respective optical and mechanical traditions are subjects of my own publications naming Jed Buchwald as series editor.[17]

The zodiac constellations of Taurus and Gemini representing Alexandria and Egypt were selected for the artwork created for this occasion for their own good reason, this time as the respective birth signs of Jed Buchwald and Diana Kormos Buchwald who variously stand out together: both in past publications like that on the 'zodiac of Dendera,' where Alexandria is the starting point of expeditions occasioning their joint adventures in Egypt,[18] and in the present celebrations, where not only the letters of their first names in 'abjad' letters fittingly add up to 70; the number of the stars forming the constellations of Taurus and Gemini join our two main 'stars' to magically add to that same number.[19]

'Looking back as we move forward' being the title of the upcoming symposium marking this occasion, something is due in that spirit about the past, present, and future of not only historical sources and early manuscripts, but also illustrations such as those of zodiac constellations, including the

17. Kheirandish, Elaheh. *The Arabic Version of Euclid's Optics: Kitāb Uqlīdis fī Ikhtilāf al-manāẓir* Edited and Translated with Historical Introduction and Commentary, 2 vols., Springer-Verlag: *Sources in the History of Mathematics and Physical Sciences*, series editor: Jed Buchwald, no. 16, 1999. Kheirandish, *The Arabic and Persian Traditions of Pappus of Alexandria's Mechanics,* to be published in the same series.

18. *Buchwald and Greco Josefowicz 2010a*, p. 9 and Acknowledgements, pp. 341–342, respectively.

19. J (3) + E (0) + D (4) + A (1) + Y (10) + A (1) + N (50) + A (1) =70. The letter E between J and D is not transcribed, and letter D is shared, not counted twice (App. III: caption). Stars of Taurus (32 internal+11 external+1:44) + Stars of Gemini (18 internal+7 external+1:26) =44+26 =70. Taurus's stars (internal, external) and two 'views' (sky, globe) are included in John Murdoch's *Album of Science: Antiquity and the Middle Ages*; I. B. Cohen (ed.), New York: Charles Scribner's sons, 1984, p. 251 (App. III: MS images).

rare Paris manuscript from which the present two zodiacs are taken. The faces of Taurus and Gemini were taken not from the many known manuscripts of Bīrūnī's *Book of Instruction* (*Kitāb Tafhīm*) in Arabic and Persian,[20] the majority of which—including those in each language in Paris's Bibliothèque Nationale—are not illustrated; nor from rare illustrated copies of the text's Arabic version or its later Persian translation by Ṭūsī all outside Paris.[21] The source is a notable 'Paris' manuscript representing another work altogether, a work that is the subject of our final selection.

III. Zodiacs and Faces

In contrast to the manuscripts of Bīrūnī's Book of Instruction whose illustrations rarely place faces next to the 48 constellations that include the twelve zodiacs discussed, those of the Book of Constellations composed by a slightly earlier author 'Abd al-Raḥmān Ṣūfī (ca. 292–375/903–986) feature illustrations for all known constellations in mirror 'views,' sky and globe: of the latter's similarly numerous productions, including Arabic and Latin manuscripts in Paris,[22] the one acting as a prototype for the set of zodiacs highlighted here is doubly notable. Though not 'the earliest or most authoritative manuscript' of Ṣūfī's Book of Constellations,[23] as compared to those in Doha and Oxford,[24] the 'zodiac of Paris' selected here for reproduction significantly contains a dedication to Ulugh Beg, the student prince of the school and observatory in Samarqand, whose reign there (r. 850–853/1447–1449) not only estimates the manuscript date after the text's dedication in the form 'Sulṭān',[25] but one that predates the birth of Jed Buchwald by nearly 500 years.

As distinct as the three above selections are from one another and from the 'Zodiac of Paris' opening the present essay, they variously overlap with images that close its final section: the first selection, highlighting 'verses,' aligns the zodiacs of Taurus and Gemini in verses on constellations in association with the similarly adjacent planets Venus and Mercury (App. I) which, in turn, represent 'instruments' and 'pens' not just in the sky, as in other Arabic and Persian verses,[26] but also on Earth, as in the able hands of our 'tool' and

20. Rozenfel'd-Eİhsanoğlu. *Mathematicians, Astronomers, and Other Scholars of Islamic Civilization and Their Works (7th–19th c.)*, pp. 148–149 include the copies of the Arabic and Persian manuscripts in Paris.

21. Illustrated manuscripts of the Arabic version include an old copy in London British Library ADD 7697, 685/1286, from which a constellation was reproduced for my exhibit 'Windows into Early Science and Craft', John Hay Library, Brown University, 2010, contribution and gold illumination: Elizabeth Cavicchi. Illustrated manuscripts of Naṣīr al–Dīn Ṭūsī's Persian translation of the Book of Constellations (*Tarjamah-i Ṣuwar al-kawākib*) include one cited as an autograph manuscript in Istanbul: facsimile, Tehran, 1348/1969.

22. Rozenfel'd-Eİhsanoğlu. *Mathematicians, Astronomers, and Other Scholars of Islamic Civilization and Their Works (7th–19th c.)*, pp. 86–87; Latin manuscript in Paris: source, note 24, p. 6.

23. Savage-Smith, Emily. 'The Most Authoritative Copy of ʿAbd al-Raḥmān al-Ṣūfī's Tenth-century Guide to the Constellations', *God is Beautiful and Loves Beauty: The Object in Islamic Art and Architecture*, Jonathan Bloom and Sheila Blair (eds.), New Haven: Yale University Press, 2013, pp. 122–155, p. 152, where the Oxford manuscript is treated as "semi-fake," citing Abolala Soudavar, *Studia Iranica* 28, 1999.

24. *An Islamic book of constellations Bodleian Library.* Oxford: Bodleian picture books; no. 13. 1965.

25. In this illustrated manuscript, the name of the Timurid ruler Ulugh Beg appears as '*Sulṭān*': MS cited in Rozenfel'd-Eİhsanoğlu. *Mathematicians, Astronomers, and Other Scholars of Islamic Civilization and Their Works (7th–19th c.)*, p. 86: as Paris 5036 (w/o the library) as 'a copy from the library of Ulugh Beg. Access to it via Professor Taha Yasin Arslan of Istanbul Medeniyet University is gratefully acknowledged.

26. Kheirandish, 'Astronomical Poems from the "Four Corners" of Persia (c. 1000–1500 CE)', Appendix VII: 'In the name of the designer of the surface of the earth, the one crafting the seven aspects; placing an instrument in Venus's hand, . . . giving Mercury, an ink and pen [to write].'

'book' masters. The second selection, highlighting 'places,' situates Taurus and Gemini jointly not only in lands as ancient as Alexandria and Egypt (App. II), but also in bodily organs as close as necks and arms.[27] As for the third selection, highlighting 'faces,' not only are the 'he' bull and the 'she' twin facing each other, with his tilted neck facing her dancing arms (App. III); in conjunction, they turn full circle into realms beyond zodiacal constellations, those no longer bound by the limits of any verse, face, and indeed, time or place.

Appendix I. Zodiacs and Verses

Versed Introduction (*Madkhal-i manẓūm*), attributed to Naṣīr al-Dīn Ṭūsī. Iqbālī, M. Shiʿr va Shāʿirī dar Ashʿār-i Khwāja Naṣīr al-Dīn Ṭūsī, pp. 97–112, p. 98 cites one manuscript with an attribution to Ṭūsī's contemporary ʿAbd al-Jabbār Khujandī, named in catalogues by Monzavi, A. (Persian Manuscripts), 1969, v. 1, p. 237; and Gulchīn Maʿānī, A. (Mashhad), 1971, v. 8, p. 223. The cited composition date (616/1219) fits either authorship once further substantiated.

Versed Introduction (*Madkhal-i manẓūm*):
Explanation of the Heavens (*Bayān-i aflāk*)

1 First I speak of heaven's configurations
2 Then, take up stellar prognostication
3 Know that the creator of fairies and angles
4 Created nine orbs and heavenly spheres.
5 Earlier scholars by decree of observation
6 Have come up with star enumeration
7 Of the twenty-nine and one thousand

27. Kheirandish, 'Astronomical Poems from the "Four Corners" of Persia (c. 1000–1500 CE)', Appendix VIII: 'know Aries as head, Taurus as neck, . . . and Gemini, your two hands . . . with open arms'; Bīrūnī's *Book of Instruction* associates Taurus with the Face 'according to the Hindus', ed. Wright, p. 216.

8 Seven are wondering, and in motion
9 [These are named] Moon, Mercury, Venus
10 Sun, Mars, Jupiter and Saturn[28]
11 The rest have 'fixed star' designations
12 From which are made, constellations

Versed Introduction (*Madkhal-i manẓūm*):
Enumeration of the Zodiacs (*ʿAdad-i burūj*)

13 [These are named] Aries, Taurus, and then Gemini
14 Cancer and Leo, then Virgo.
15 Scorpio, Sagittarius, after that Libra,
16 Capricorn, Aquarius, and Pieces come after . . .
17 [With] the first and eighth of the zodiacs
18 This one called Aries, the other, Scorpio
19 Both reside in the house of Mars
20 Like Jupiter, housing Archer and Pisces
21 Taurus and Libra are housed in Venus
22 As Leo in the Sun, and Cancer in the Moon
23 Mercury's house joins Virgo and Gemini
24 That of Saturn, Capricorn and Aquarius

Appendix II. Zodiacs and Places

Book of Instruction (*Kitab Tafhīm*) by Abū Rayḥān Bīrūnī

1 Aries: Babylon, Fars, Palestine, Azarbājyjājn . . .
2 Taurus: Sawād [Iraq], Hamadān, Ctesphon, Cypress, Alexandria, Constantinople, Omān, Ray . . .
3 Gemini: Egypt, cities of Barqa, Armenia, Gurgān Gilān (shares in Iṣfahān and Kirmān)
4 Cancer: Armenia, Africa . . . Baḥrayn . . . (shares in Balkh and Azarbādgān)

28. Lines 9–10 are cited from one manuscript.

5 Leo: Turkistān … Jerusalem … Ctesphon … Ṭūs …
6 Virgo: Andalusia, Syria, Crete, Mesopotamia, Kūfa … cities of Fārs towards Kirmān, Sīstān
7 Libra: Greek empire … Mecca … Balkh … Herāt, Kābul
8 Scorpio: Ḥijāz … desert of Arabia as far as Yemen, Tanjier, Āmul and Sārī.
9 Sagittarius: Iraq ʿajam, Isfahān, Ray, Baghdād, share in Bukhārā and Gurgān as far as Morocco
10 Capricorn: Sea between Omān and India, Ahwāz, Perspolis
11 Aquarius: Southern Iraq as far as Kūfa, Ḥijāz (shares in Fārs)
12 Pisces: Ṭabaristān, north of Gurgān, Bukhārā, Samarqand … Egypt, Alexandria …

Appendix III. Zodiacs and Faces
At right, above, are folios of Taurus and Gemini in *The Book of Constellations of the Fixed Stars* (*Ṣuwar al-Kawākib al-Thābitah*): ʿAbd al-Raḥmān Ṣūfī (Bibliothèque nationale de France, Arabe 5036), ff. 118, 126 a–b; they include the stars (on these stars and their sum as 70, see n. 19).

Below are reproductions of the same folios, but excluding the stars present in the manuscript (on the reproduction, see note 3; on the caption and the letters of the two names adding to 70, see n. 19).




Hasok Chang

"Why So Much About Batteries?"

I remember very well how I first came to know Jed Buchwald during the fall of 1992. I was spending that semester as a visiting graduate student at Harvard working with Peter Galison, who was one of my two main PhD advisors at Stanford and who had just moved to Harvard. Peter stressed very strongly that I should not miss the opportunity while I was there to go across town over to MIT to learn at Jed's feet. I followed that advice, and ever since then Jed has been one of my most important mentors, even though we have not been in frequent contact beyond the early years.

With my limited perspective as a student, I had not fully realized what an exciting moment 1992 was for Jed, and for the whole field of the history of science, with the establishment of the Dibner Institute that year and Jed's arrival from Toronto as its first Director. But I was fully aware of what a great privilege it was to be invited to attend his graduate seminar during that semester, and then again when I returned to Harvard as a postdoc working with Gerald Holton from July 1993 to December 1994. These seminars were very select groups, only 3 or 4 people sitting with Jed in his office every week chewing our way through some very difficult primary sources. And we had the benefit of learning some topics that Jed himself was fully engaged with; one of the seminars was on the history of electricity and magnetism, and Jed was just in the process of publishing *The Creation of Scientific Effects: Heinrich Hertz and*

Electric Waves (*Buchwald 1994a*). Participants in these seminars included important people such as Babak Ashrafi, now the Executive Director of the Philadelphia Area Consortium for History of Science, Technology and Medicine, and Diane Greco Josefowicz, Jed's co-author of *The Zodiac of Paris* (*2010a*). In these seminars we learned to *do* history of science, I mean, really do history of science. Under Jed's strict guidance there was no escape from the core task of understanding the primary texts inside and out. It was a relentless quest for sense-making, which ought to remain an essential mode of work for historians of science. I cannot get out of my head, nor do I want to, Jed's firm and matter-of-fact voice saying "That doesn't make sense" whenever we offered a half-baked interpretation of a passage.

As I was doing my PhD in philosophy (though with a strong historical dimension), I found it enormously encouraging that Jed thought I had the potential to do first-rate historical work. That encouragement has stayed with me throughout my career, in which the integration of the history and the philosophy of science has been one of the most important motifs. I remember vividly how Jed sat me down in his office one day and declared: "All the good history of science these days is being done by philosophers." Of course that wasn't quite true, but it was powerful encouragement. It was also a sign of things to come: many years later, in 2006, Jed and I would be among the founding members of the international Committee for Integrated History and Philosophy of Science, on which we have served together since then and for which we have helped organize seven international conferences. It has been a great honor for me to collaborate with Jed in this way.

Jed's encouragement and guidance played an essential role in convincing me that I could really be a good historian, rather than just a historically informed philosopher. In addition to the graduate seminars, Jed invited me to participate in a number of select research seminars and workshops during my time in Boston and for several years afterwards. In those events I was able to begin to enter the discourse and the community of historians of science at the highest level. I remember especially the luxuriously supported workshops funded by the Sloan Foundation.

I think many people would readily recognize Jed's uncompromising commitment to quality as a hallmark of his scholarly work. He does not mince words, and he does not suffer fools gladly. And he truly does not seem to care who you are, where you come from, what you are associated with, or anything, as long as your work is good. One of the things that I have appreciated most about Jed is that he takes the trouble and effort to encourage and promote young scholars for whom he has no formal obligations. I was one such person. I did have an introduction from Peter Galison (and Thomas Kuhn and Evelyn Fox Keller may have mentioned something positive about me, too), but I don't think that would have been a very important factor in Jed's decision to take me under his wing. On the contrary, it was widely thought that there was a certain rivalry or tension between Jed and Peter (though, to their credit, neither of them gave an indication of such in their extensive interactions with me). Jed has been a great role model for me as I now try to promote serious and talented young scholars wherever and however I may discover them. I also do my best to connect them with each other, and the paradigm of that is how keen Jed was to make sure that I met Sungook Hong, who was his star PhD student in Toronto. This is a connection that I have cherished and benefited greatly from for almost 30 years now, and I daresay that it is an important link at the core of the Korean history and philosophy of science community.

Jed's teaching also had a direct effect on my productive choice of research problems. It was during my postdoc years that I switched my focus from quantum mechanics and relativity to 18th and 19th century physics, and Jed's influence was undeniable in that. More specifically, everything that I learned in his graduate seminar on the history of thermal physics formed a strong foundation for my work on the history and philosophy of temperature and heat that resulted in my first book *Inventing Temperature* (2004). Jed's Sloan workshop in 1997 on the history of the continuous spectrum of radiation led to a long two-part paper on "infrared metaphysics" (with Sabina Leonelli). Even now I frequently reach back to all the various things I learned from Jed, from the influence of German Romanticism on energy conservation to Clifford Truesdell's interpretation of thermodynamics. Recently it was a particular pleasure to re-read Helmholtz's paper "On the Conservation of Force" (1847) in the course of my current research on the history of batteries, and encounter there, on the old xerox copy, all the traces of my struggle in learning how to read primary sources! A puzzled comment I had scribbled on the margin reads: "Why so much about batteries?" Nearly one fifth of that classic text is devoted to the discussion of batteries, and I didn't see the point of it back then; now, all these years later, I am finally starting to understand. If all goes to plan, my book on the history of batteries should be published by 2021, nearly 30 years after Jed's graduate seminar first planted its seed.

So I look back on the three decades during which I have known Jed and learned from him. Our relationship has been a very precious gift to me, given freely by Jed with no other motive than the hope that a young foreign student who walked timidly into his office at the Dibner Institute might eventually be able to contribute something worthwhile to the area of scholarship about which he has cared so deeply and to which he has contributed so decisively.

Allan Franklin

Discrepant Measurements & Replication

I first met Jed in the summer of 1994 when I was a visiting fellow at the Dibner Institute, where he was then director. Since then we have been friends, co-authors, co-editors, and colleagues. He has also been a scholar of the highest quality. In all of these roles I could always count on Jed's honesty, and scholarly integrity. Almost invariably in playing these roles Jed has lessened my work load. Some years ago I was asked to comment publicly on one of Jed's books. It is well known that reviewing a good book is easier than reviewing a bad book. That was certainly true in this case. My review opened with, "If you want to read history of science the way it should be written, with careful attention to technical detail, valuable discussions of the scientific context, and interesting personal glimpses from diaries and letters, then Jed Buchwald's *The Creation of Scientific Effects* is for you." When we co-edited *Wrong for the Right Reasons*, our introductory essay was almost entirely Jed's work.[1] My contribution to the editing was more substantial, but Jed did most of it. Sometimes his efforts did not result in good news. Jed served as editor of one of my papers for the *Archive* and broke John Heilbron's record for the most comments on one of my papers.[2]

1. *Buchwald and Franklin 2005a* and *2005b*.

2. To be fair, the paper in the *Archive for History of Exact Sciences* was

In the spirit of this symposium I have been thinking about the future of the history of science. I would like to suggest that we pay more attention to the ordinary practice of science and to the scientists and their contributions that do not make it into science textbooks or into books and journals on the history of science. We should also pay attention to scientific failures. This is not to say that we should neglect the great achievements, but only that these other studies can also give us insights into the practice of science.[3]

With this in mind I will discuss some of the work of Francis Baily, Esquire, Fellow of the Royal Society, and Vice President of the Royal Astronomical Society.[4] Baily was one of the early critics of Henry Cavendish's experiments on gravity.[5] Cavendish's experiments are enshrined in virtually all introductory physics textbooks. More often than not, Cavendish is credited with measuring G, the universal gravitational constant, in the modern statements of Newton's Law of Universal Gravitation.[6] This is not correct. Cavendish measured

more than twice as long as the paper in *Historical Studies in the Physical Sciences*. The average number of comments per page was approximately the same.

3. For example, William Wilson's 1909 paper "On the Absorption of Homogeneous β Rays by Matter, and on the Variation of the Absorption of the Rays with Velocity." *Proceedings of the Royal Society (London)* A82: 612–628, is a masterpiece of scientific methodology.

4. That is the way he listed himself in the paper. Baily was also president of the Royal Astronomical Society four times. He is best known for his discovery of Baily's beads, an optical phenomenon visible during a total eclipse of the Sun.

5. Cavendish, H. (1798). "Experiments to Determine the Density of the Earth." *Philosophical Transactions of the Royal Society (London)* 88: 469–526.

6. Newton did not, in fact, use G, but stated that the gravitational force between two masses was proportional to the product of those masses and inversely proportional to the square of the distance between them.

the density of the Earth.[7] J. H. Poynting's later evaluation of Cavendish's work was laudatory: "…[Cavendish] made the experiment in a manner so admirable that it marks the beginning of a new era in the measurement of small forces."[8]

Baily was far less enthusiastic. He did not, in fact, believe that Cavendish had performed a serious measurement, but had merely demonstrated an excellent method of measuring the density of the Earth: "He is of the opinion that Cavendish's object in drawing up his memoir was more for the purpose of exhibiting a *specimen* of what he considered to be an excellent method of determining this important inquiry, than of deducing a result, at that time, that should lay claim to the full confidence of the scientific world."[9] He further criticized Cavendish for performing only 23 experiments,[10] whereas he, Baily, had made 2,153 such measurements. This is an exaggeration. "Baily adopted the method of Reich for reducing the time required to make the number of turning-points requisite for calculating the deviation and the period; that is the masses were moved quickly from one near position to the other and the last turning-point on one series served for the first of the next. Three new turning-points were observed at each position of the masses, and each group of 4 was called an experiment.[11]

7. The title of Cavendish's paper was "Experiments to Determine the Density of the Earth."

8. Poynting, J. H. (1913). *The Earth: Its Shape, Size, Weight and Spin*. Cambridge, Cambridge University Press, p. 63.

9. Given the detailed corrections that Cavendish made to his calculations, this seems highly unlikely. Baily, F. (1842). "An Account of some Experiments with the Torsion-rod, for Determining the Mean Density of the Earth." *Philosophical Magazine* 21: 11–121, p. 111.

10. Cavendish actually reported 29 experiments.

11. Mackenzie, A.S. (1900). *The Laws of Gravitation; Memoirs by Newton, Bouguer, and Cavendish*. New York, American Book Company, p. 117. See discussion below for some of the problems of Baily's method.

Had Cavendish used Baily's counting method, he would have reported many more experiments.

One possible confounding effect that Cavendish himself discussed was the need to keep the temperature in the room constant and to avoid temperature gradients. Cavendish arranged to have his apparatus in a sealed room, manipulated the masses by means of a series of pulleys, and made his observations using a telescope located outside the room (Figure 1). This was insufficient for Baily: "Cavendish chose an out-house[12] in his garden at Clapham Common; and, having constructed his masses *within* the building, he moved the masses by means of ropes passing through a hole in the wall, and observed the torsion-rod, by means of a telescope fixed in an anteroom on the *outside*. The general temperature of the interior was therefore probably

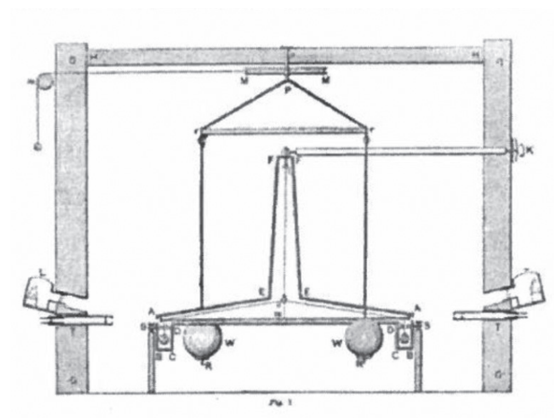


Figure 1. Cavendish's experimental apparatus. Notice that the pulley for moving the weights and the telescope are outside the room. Source: Cavendish (1798).

12. Modern American usage regards an outhouse as a privy, an outdoor toilet. The *Oxford English Dictionary* defines outhouse as a building such as a shed or barn that is built onto or in the grounds of a house.

uniform during the time that he was occupied in any one set of experiments: but it is scarcely to be expected that a building of this kind, and in such a situation, would preserve the same uniform temperature for twenty-four hours; especially at the season which he selected for his operations."[13]

Baily's final result for the density of the Earth was 5.67, where the density of water is 1. That disagreed with the value reported by Cavendish of 5.48 ± 0.38. Baily remarked that, "It cannot escape observation that the general mean result, obtained from these experiments is much greater (equal to 1/25th part) than that deduced either by Cavendish or Reich, who both agreed in the very same quantity, namely 5.44:[14] but he does not assign any probable cause for this discordance (p. 121)." Baily was quite confident in his own result. "It is evident from the detail which he [Baily] has given of his own experiments, that perceptible differences not only arose according to the mode in which the torsion rod was suspended but also depended on the materials of which the suspension-lines were formed: but it is somewhat singular that none of these mean results, in any of the classifications, are so low as that obtained by the two experimentalists above mentioned (p. 121)." Baily had made measurements with small masses of different sizes, different materials, and with different modes of suspension (Figure 2). The results were quite consistent.

Cornu and Baille, however, pointed out a problem with Baily's method.[15] They noted that using the fourth reading of the turning-point as the first one of the next experiment resulted

13. As discussed below, Baily was not always so careful with his own measurements. See note 8, *Bailey 1842*, p. 117.

14. See discussion below.

15. A. Cornu and J. B. Baille (1878). «Sur la mesure de la densité moyenne de la terre.» *Comptes Rendus des Séances de L'Académie des Sciences* 80: 699–702.

Balls.	Double silk.		Double wire.		Single wire.	
	No.	Density.	No.	Density.	No.	Density.
2½-inch lead	148	5·60	130	5·62	57	5·58
2-inch lead.........	218	5·65	145	5·66	162	5·59
1½-inch platina ...	89	5·66	...	...	86	5·56
2½-inch brass	46	5·72	...	...	92	5·60
2-inch zinc	162	5·73	20	5·68	40	5·61
2-inch glass	158	5·78	170	5·71	...	...
2-inch ivory	99	5·82	162	5·70	20	5·79
2½-inch lead, with brass rod......			44	5·62		
2-inch lead, with brass rod			49	5·68		
Brass rod, alone			56	5.97		

Figure 2. Baily's results for the density of the Earth. Source: Baily (1842).

in an error: "They showed that the rotation of the plank holding the masses could not be performed rapidly enough to get the masses into the new position before the arm had begun its return journey."[16] They then calculated the results for the density of the Earth using only the last three measurements in 10 of Baily's experiments. They found that for those 10 experiments the mean value for the density of the Earth was reduced from 5.713 to 5.615. Applying the same percentage correction to Baily's mean value changed that result from 5.67 to 5.55.

W. M. Hicks found yet another problem with Baily's measurements of the density of the Earth. By reanalyzing Baily's data he found that the density fell with a rise in temperature: "I have recently been examining Baily's observations on the mean density of the Earth in order to see if they showed any traces of a dependence of the attraction between two masses on their temperature. I was astonished to find in his numbers most decided signs of some temperature effect."[17] Hick's

16. See note 10, *Mackenzie 1900*, p. 119.

17. W. M. Hicks (1886). "On some irregularities in the values of the mean

Temperature F.	Number of series.	Number of daily means.	Number of observations.	Mean density.
				5 +
36° (mean)	4	7	46	·7296
40 ± 2	12	22	128	·7341
45 ± 2	20	43	247	·6823
50 ± 2	18	38	302	·6799
55 ± 2	12	23	187	·6594
60 ± 2	13	31	333	·6495
65 ± 2	7	17	140	·5935
68 (mean)	4	11	96	·5828

Figure 3. Hicks's analysis of Baily's results showing the dependence of the density of the Earth on temperature. Source: Hicks (1886).

results are shown in Figure 3. He concluded, "The gradual fall of mean density with rise of temperature is most marked, the only exception being in the case of the lowest temperature (36°) which is slightly smaller than for the temperature of 40° (p. 158)." Mackenzie suggested that the most probable explanation of this effect was given by Poynting, who remarked that the experiments with light balls were performed in winter whereas those with heavy balls were done in summer. These results seem ironic given Baily's criticism of Cavendish's efforts to maintain a constant temperature during his measurements.

There is an oddity in Cavendish's final result. Cavendish claimed that the average value he obtained from the first six measurements of the density, 5.48, those with a less stiff wire, was equal to that of the last 23 measurements, those found

density of the earth, as determined by Baily." *Proceedings of the Cambridge Philosophical Society* 5: 156–161, pp. 156–157. Hicks used Baily's data that was included in his much longer account of his experiments, Baily, F. (1843). "Experiments with the torsion-rod for determining the mean density of the earth." *Memoirs of the Royal Astronomical Society* 14: 1–120 and i–ccxlviii.

The following Table contains the Result of the Experiments.

Exper.	Mot. weight	Mot. arm	Do. corr.	Time vib.	Do. corr.	Density.
1	m. to +	14,32	13,42	′ ″	-	5,5
	+ to m.	14,1	13,17	14,55	-	5,61
2	m. to +	15,87	14,69	-	-	4,88
	+ to m.	15,45	14,14	14,42	-	5,07
3	+ to m.	15,22	13,56	14,39	-	5,26
	m. to +	14,5	13,28	14,54	-	5,55
4	m. to +	3,1	2,95		6,54	5,36
	+ to —	6,18	-	7,1	-	5,29
	— to +	5,92	-	7,3	-	5,58
5	+ to —	5,9	-	7,5	-	5,65
	— to +	5,98	-	7,5	-	5,57
6	m. to —	3,03	2,9	-	-	5,53
	— to +	5,9	5,71		-	5,62
7	m. to —	3,15	3,03	7,4 by mean.	6,57	5,29
	— to +	6,1	5,9			5,44
8	m. to —	3,13	3,00		-	5,34
	— to +	5,72	5,54	-	-	5,79
9	+ to —	6,32	-	6,58	-	5,1
10	+ to —	6,15	-	6,59	-	5,27
11	+ to —	6,07	-	7,1	-	5,39
12	— to +	6,09	-	7,3	-	5,42
13	— to +	6,12	-	7,6	-	5,47
	+ to —	5,97	-	7,7	-	5,63
14	— to +	6,27	-	7,6	-	5,34
	+ to —	6,13	-	7,6	-	5,46
15	— to +	6,34	-	7,7	-	5,3
16	— to +	6,1	-	7,16	-	5,75
17	— to +	5,78	-	7,2	-	5,68
	+ to —	5,64	-	7,3	-	5,85

Figure 4. Cavendish's results for the density of the Earth. Source: Cavendish (1798).

with a stiffer wire. He reported that both sets of measurements gave a density of 5.48 and that his final result was 5.48. This is not correct. The average of the first 6 measurements is 5.31 ± 0.22, whereas the average for the last 23 measurements is 5.48 ± 0.19. The average of all 29 measurements is 5.448 ± 0.22.[18] This discrepancy was first noted by Baily, and later by

18. Thus, the agreement with Reich referred to by Baily.

Poynting and others. They attributed this to an arithmetic error by Cavendish. Baily pointed out that if the third measurement (Figure 4), published as 4.88, was in fact 5.88 the discrepancy disappears. Baily recalculated the density of the Earth using Cavendish's original data for this experiment and found that the value is indeed 4.88.[19]

Baily's work is only a small part of the measurements of the density of the Earth in that period. Mackenzie (1900) lists 20 measurements of the density between Cavendish's 1798 report and the end of the 19th century. These measurements used several different methods; the torsion pendulum, a balance, a simple pendulum, and the deflection of a plumb line by a hill or a mountain.[20] Several of these results were analyzed not only by the author, but also by other scientists. The results vary from 4.25 to 7.60.

Given the recent discussions of the issue of replication in science, examining this history might provide interesting insights.

19. Perhaps Cavendish made an error and actually used 5.88 rather than 4.88.

20. Sir George Airy, the Astronomer Royal, remarked on the difficulty of gravity measurements. 'He measured the periods of two pendulums, one at the top of a mineshaft and the other deeper in the shaft. "We were raising the lower pendulum up the South Shaft for the purpose of interchanging the two pendulums, when (from causes of which we are yet ignorant) the straw in which the pendulum-box was packed took fire, lashings burnt away, and the pendulum with some other apparatus fell to the bottom. This terminated our operations for 1826 (Airy, G.B. (1856). "Account of Pendulum Experiments undertaken in the Harton Colliery, for the purpose of determining the Mean Density of the Earth." *Philosophical Transactions of the Royal Society (London)* 146: 297–355, p. 299).

Diane Greco Josefowicz

Into the Blue: Through the Years with Jed Buchwald

When I first met Jed in 1995, I was twenty-three, four semesters into my graduate program, and still completely without a clue as to what I wanted to study. My dithering reflected the fact that, because I was being paid to read and to attend classes, I already had exactly the job I wanted. My friends were much worse off, working at miserable entry-level positions in management consulting and wondering if they should have started garage bands. At least being a student was something I felt qualified to do. But I sensed the clock ticking on my complacency. I was expected to hit the usual milestones, to find an advisor, to prepare for general exams. Like the pilot of an airplane running out of fuel, I would soon need either to land or to eject. For some reason the time seemed just right to enroll in Jed's graduate seminar in early modern science.

The syllabus was marvelous—dense, rich, ambitious. The reading featured a week devoted to Newton's *Principia*, another to *Opticks*, and a third to Galileo's *Dialogue Concerning the Two World Systems*, which contained less math than the Newton volumes but still looked gratifyingly dense and absorbing. The primary sources were supplemented by reams of secondary material. This abundance made a compelling case on its own, but what really sold me was the week devoted to Aristotle, for which Jed assigned the omnibus edited by

Jonathan Barnes. I still have the set, an edition of two volumes printed in small type on onion skin. It weighs five and a half pounds. Just before winter break, Jed had circulated the first day's assignment: We were to read everything in those two volumes except the *Poetics*.

For five weeks, I dragged those volumes everywhere. I read Aristotle in Washington, in Charlottesville, in Providence, in New York. In Philadelphia I dozed off over Volume 1 and only woke when it crashed to the floor. As I retrieved it, the room resounded with the downstairs neighbor's angry thumping. I lodged myself more deeply into the sofa and returned to my task, resolved to be quieter.

From "On Marvellous Things Heard," I learned that Cretan goats shot with arrows crave a plant called "dittany," an oregano variant that, according to Aristotle, causes the arrows to leave their bodies. I read reports of lumps of lead leaping from the Ganges and of iron-eating mice on the island of Gyanos. Turning to "Problems," I confronted such mysteries as "Why does man sneeze most of all animals?" and "Why is the face chosen for representation in portraits?" (As opposed to what? The question does arise.) In "Topics," I encountered Aristotle's belief that observable reality cannot be discussed without first being defined; and in "On the Heavens," I learned that even geometry was a matter of definitions: "A magnitude if divisible one way is a line, if two ways a surface, and if three a body."

On the first day of class, a few students crowded around a table in Jed's office at the Dibner Institute. As large as the office was, there was not quite enough room for all of us, burdened as we were by our winter coats and two large volumes of Aristotle apiece. As we settled in, Jed divided the whiteboard into quadrants labeled *earth, air, fire, water*.

He pointed at the labeled quadrants and asked, "What are these?"

Was this a joke? I stole a glance at the others. No one wanted to speak. We all knew what the words referred to—and we knew, I suspect, even before we'd plowed through more than two thousand pages of the Barnes Aristotle.

Jed jabbed a finger at the whiteboard. "Come on. What are these?"

I'd dragged those two volumes up and down the east coast. I'd read everything except the *Poetics*. "Nouns," I said finally. "Those are nouns."

Jed regarded me for a long moment. One raised eyebrow met another. But what could he say? I wasn't wrong. In the end, he laughed, and I remembered a line from *Casablanca*: *I think this is the beginning of a beautiful friendship*.

This was hardly a foregone conclusion. In the great high school yearbook of life, I am the one voted Least Likely to Work with Jed Buchwald. My childhood told against it. Growing up under conditions we would now call "free range," I was skilled in distinctly non-academic arts: I knew my way around a tackle box, I could gut a fish, fix a toilet, repair my bike. My friends ensured that I knew other things, too—for instance, how to start a beautiful blue Trans Am when in the awkward circumstance of not being in possession of the key. This is not the sort of person who is befriended by a MacArthur Genius. As my husband likes to say, "The great miracle is that you are not incarcerated."

Yet, there were signs. For reasons I can't recall, as an adolescent I spent a long period holed up in the public library, on the track of some mystery that could only be resolved by extensive reading. As a damp, cold spring turned to a damp, cold summer, I haunted the stacks and devoured my selections right where I found them, sitting on the floor with my wet high-tops propped on the baseboards. From that vantage I had a grand view of the neighboring prison yard, its edges marked by loops of concertina wire black against the dense white sky. Nothing I read scratched my particular itch: I had questions about the nature of reality, the nature of our descriptions of it, and the nature of the relationship between these things. Eventually it dawned on me that I might find answers in writing of my own, but many years had to pass before that prickle of motivation would fledge into a goal. By the time I washed up in Jed's office, I'd kicked over enough of my own traces that a casual observer could easily miss all that still traveled with me—the fish I gutted, the bike I took apart, the library with its view of the prison, the near-miss with that blue Trans Am. I once briefly shared an elevator with two colleagues discussing *The Zodiac of Paris*. They didn't recognize me. One said to the other, "You know, the really shocking thing is it's actually a good book."

Actually, the really shocking thing is that I'm not incarcerated.

I slipped away from the professors, glad for the chance to be present at a moment when I might have been slandered but wasn't, thanks to the quality of the work Jed and I had undertaken together. As Jed has told me over and over, quality is the only thing that matters. Even the guys in the elevator had to admit that. For all my ambivalence about the history of science, I still like this aspect of the profession. With every book we write, Jed reminds me: This is not for our time, but for the ages. Over the years Jed has shared a lot of wisdom. His reassurances have carried me through some bad days, and his admonitions are exactly what they should be, crisp and direct and not too numerous. Some other entries from my Book of Jed:

On writing history: *Start with chronology.*

On fashionable ideas: *It's all crap.*

On irritations you can do nothing about:
That's what alcohol is for.

On regrettable expenditures: *It's only money.*

On traffic violations in foreign countries:
They won't arrest you, but you might get a little bill.
(This one saved a holiday. Thanks, Jed.)

One day last winter, Jed told me that our new book, on the decipherment of Egyptian hieroglyphics, was overdue and that we should plan to send the manuscript to press by the coming spring. It is hard to convey the panic this announcement stirred in me. Every project must balance speed, economy, and quality. At best, you get two out of three. And here was Jed, demanding speed and quality on a project from which neither of us had yet realized a dime.

"What you mean," I gasped, staring at the hundreds of unrevised manuscript pages stacked on my desk, "is *next* spring." In the end, Jed accepted a midsummer deadline. "I'm sending it to the press on July 1," he warned. "I don't care what condition it's in."

Like matter under extreme conditions, a labor of love under heavy deadline pressure will torque into odd shapes that make both the labor and the love rather more ambiguous. Inevitably there was an argument. As our deadline approached, Jed added several paragraphs to a chapter that was, I thought, substantially finished. The new material was rough, and it involved nothing less than Thomas Young's view of the nature of the relationship between language and reality. As far as I could make out, Jed believed that Young's ontological commitments were fundamentally geometrical—and not in the familiar Aristotelean way of being susceptible to definition. It was a view of reality that necessarily defied description in language, and I had a sinking sense that illustrative examples would also be thin on the ground. Which put us in a bind, since our task was to elucidate the position. Yet Jed seemed to think he'd been completely clear.

It was dusk in Providence, mid-afternoon in LA. Try as I might, I could not parse Jed's meaning. We were still shooting emails back and forth as I headed out for dinner with my family. Our destination was a new Greek place downtown. We were seated, and my husband, sensing some unpleasantness afoot, immediately ordered me a glass of wine. I was still typing furiously into my phone, asking a lot of questions, some apparently quite stupid. Between the meze and the souvlaki, Jed finally clarified his views, concluding with words to the effect of: *If you don't understand this, I don't really know who you are*. I understood this to mean: *It turns out that you really are as stupid as I have long suspected you to be*.

I looked up. The restaurant was elegant, loud, and busy; my husband and daughter were sharing a joke. I caught our reflection in the plate glass window opposite and was struck by the oddity of being physically at dinner in Providence and mentally three thousand miles away. I remembered the white sky of my childhood, the concertina wire and the blue Trans-Am, my sneakers steaming on the baseboard, the rows of unsatisfactory books. I remembered Aristotle's nouns, and then, recalling the wisdom of the *Book of Jed*, I set the phone down and ordered another glass of wine. Later I integrated Jed's clarifications into the draft and returned the revision along with some testy remarks of my own about the nature of the difficulty between us. As it turned out, the revision was a winner. We never talked about the exchange, but I believe the book itself, which like all our books is bigger than either of us, created the necessary enlarging context, making it possible

to arrive at something I might call, if I were in a simplifying mood, forgiveness. Maybe what Jed really meant was: *A capacity for surprise has always been essential to this relationship*.

Which is, in fact, true.

As our new book was coming fitfully to life, Jed sent me a photograph of the view from his vacation home in Sicily. In the photograph the Mediterranean was the vibrant blue everyone imagines it to be, the blue of every picture postcard from that part of the world. The blue for which Homer had no words. The blue of that lost Trans-Am.

I wrote back: "The sea really *is* that blue, isn't it." I see now that with my odd punctuation, I was both asking and not asking a question. *Is it?*

Among the items Jed brought on that vacation was a camera-fitted drone—the perfect instrument for making an answer to the question I had not quite asked. Jed sent this mechanical bird into the sky and it returned with footage of—what else?—the sea. I was at my desk, lost in the byways of early 19th century France, when Jed's video arrived. To a single long shot he'd added a soundtrack, the 1958 hit by Domenico Modugno with the extraordinary title, "Nel blu dipinto di blu," more familiarly known as "Volare." Evidently the song was inspired by Modugno's dream of a flying man who wanted to paint himself the blue of the sky he flung himself into; Modugno said this dream was in turn inspired by a painting by Chagall. A song inspired by a dream inspired by a painting—the layers stack like the materials of history, each addition deepening what we know. As if to say, since we'll never untangle being from seeming, the world as it is from our descriptions of it, why not just fly around on this beautiful day and wear the world's colors for once? *Into the blue, ourselves painted blue*.

John Krige

The Political Economy and/of Knowledge

I am delighted to have the opportunity to celebrate Jed's 70th birthday with so many good friends and colleagues. It is just ten years since I spent six months in the Division of Humanities and Social Sciences at Caltech as the Eleanor Searle Visiting Professor. I suspect it took something of a leap of the imagination and a willingness to bend the rules to allow me, with my Cold War intellectual focus, to count as a viable candidate for this distinguished position—but I am certainly glad that Jed managed to pull it off. My sojourn at Caltech was, and remains, one of the highlights of my intellectual career.

It is probably no coincidence that I arrived here just two years after Caltech had appointed Jean-Lou Chameau as President of the Institute, the same Chameau who had been my provost at Georgia Tech. I knew him in that capacity through his wife Carol Carmichael who had taken my graduate class in the history of science and technology. And although we barely socialized with them in Atlanta there was a bond there that we built on when we got here. Indeed Jed and Diana soon invited us to dinner at their home with Jean Lou and Carol and, in the balmy, relaxed air of that evening we began to feel that we knew him quite well.

When I came to Caltech everyone was talking about Jean Lou's olive oil project. He had enrolled the whole community in gathering olives from the trees on campus and

extracting and bottling the oil. By the time we arrived it was quite hard to get hold of a bottle of this oil, though I never knew if that was because it was so good that it had been sold out, or because it was inedible. All the same, my esteem for Chameau's community-building efforts soared.

Olive trees were just one aspect of the natural environment that I will never forget. There were the orange trees. There were the beautiful green grass lawns that contrasted with the blazing white tents at Commencement. There was the rock pool and its turtles. There was Roy Ritchie's rose garden. And there were the Jacaranda trees that turned the streets of Pasadena into a carpet of purple flowers, reminiscent of the town that I grew up in in South Africa. Above all, of course, there were the gardens at the Huntington Library, and the spectacular desert and Japanese gardens in particular.

I taught a couple of courses on the Manhattan project and on superpower rivalry in space. They were oversubscribed, perhaps because of their intrinsic interest, perhaps because they provided a contemporary alternative to Ptolemy's *Almagest* and to Newton's *Opticks*. The best of my students were far better than those I had taught at Georgia Tech though, to be fair, the majority were roughly on a par with our majors at what is, after all, a top engineering school. What I only grasped at the end of the semester was the pressure to succeed that these young people endured. Tragically there were two suicides in the week just before Commencement.

I came to the Division of HSS at a time when there was a particular need to build bridges with the Huntington Library. I had an office there, or rather a room without a view, where I made sure to spend at least one day a week. I spent my time working on the archives of the German astronomer Walter Baade. Baade had come to the US in the 1930s and decided to stay on at Mount Wilson during the war, apologizing to his authorities for not going back to Germany to take charge of the new telescope at the Hamburg observatory and to make his contribution to the intellectual life of the 'Volk,' as he called it. Baade interested me because he was a major promoter of the European Southern Observatory (ESO) that eventually built a superb telescope in the Chilean Andes. His enthusiasm for this venture was driven by his determination to advance the career of his protégé Otto Heckmann, who took the job in Hamburg that Baade had refused. Heckmann was eventually nominated the director-general of ESO.

I learnt a lot about Baade from the records here, most notably that he remained culturally and linguistically anchored in his German past. He was famous for his nostalgic dinner parties where everyone, including his visitors from 'home,' spoke German. This was a loaded cultural statement, of course. Fritz Zwicky, a Swiss astronomer who also spent time at Caltech, reputedly called Baade a Nazi to his face, a claim that I have not been able to confirm. Heckman, for his part, engaged in the usual intellectual gymnastics during the war, at first distancing himself from 'Jewish physics' and Einstein's theory of relativity and then, after the war, claiming that in fact he had always believed in Einstein's theories but was just pointing out that they were open to different, classical interpretations. He also watched over the pillaging of equipment from observatories in Nazi-occupied lands, including Belgium, equipment that was then used to contribute to the war effort. It was the cold war, of course, that enabled men like Heckmann to be accepted back into the international scientific community so quickly after 1945. There was no boycott of German scientists, as had been advocated after WWI. Their Nazi past was brushed under the carpet to secure their allegiance to the west in the face of the communist threat.

Not everybody was so forgiving. Albert Einstein—who attended the first formal dinner at Caltech's Athenaeum in February 1931—was one of the few who spoke out against the German scientific community after the war. Not because they had accommodated themselves to the Nazis during the war, but because they had shown no remorse for doing so when the war was over. Nor did they have the moral courage to apologize for German war crimes. We would do well to follow in Einstein's footsteps. Timothy Snyder is just one of many who see strong parallels between Germany in the 1930s and a resurgence of anti-Semitism, racism, and hate in many parts of the world. This surely imposes new responsibilities on us on historians of science and technology to tackle the perversion of reason that is destroying our democracy.

Intellectually my time here was immensely productive. My interest in the Cold War dovetailed with work being done by Naomi Oreskes, who was the Bacon Fellow around that time. She organized a workshop whose proceedings we edited together and published in Jed's series with the MIT press under the title *Science and Technology in the Global Cold War.* Equally important for me, I was exposed to scholarly debate on the early modern period that has always fascinated me, with Jed and Moti of course, but also with Kristine Haugen, Gideon Manning, Nick Popper, and Noel Swerdlow. I still remember the substance of some of those debates, and laugh now at how angry Noel was with a young scholar who tried to contextualize an ancient philosopher's claim that the world was flat. As harsh as his critique may have been, it was indicative of a deep-rooted hostility to anthropological and sociological approaches to science that treated truth and falsehood symmetrically, losing sight of the role of the world in shaping our knowledge of it. We are living with some of its consequences today, so that it is now incumbent on philosophers like Bruno

Latour to extricate themselves from the embarrassing appropriation of their critical thinking by climate change deniers.

National politics was never far from our social interactions while I was at Caltech, especially in our weekly lunches with the philosophers. George Bush Jr. was the President then: I can still remember that we thought that the dark ages had descended on the country after he had dragged America into the war on Iraq, irreversibly destabilizing the Middle East—not quite what he meant by 'Mission Accomplished'—and then muddled his way through the disaster of Katrina, famously praising the disgraced FEMA director with the words "Brownie you're doing a heck of a job." We could never have imagined then the state of the nation today, one that so defies even our worst nightmares. We are literally speechless, numbed by a flood of tweets, speeches, and actions whose damage to the country, its diverse peoples and institutions, and its status in the world far exceeds in scale and scope the devastation wreaked by Katrina on the Jefferson and Lower 9th Wards in New Orleans. Perhaps we were just naïve then.

Ten years ago I designed a card for Jed's 60th birthday. It had a picture of Sir Isaac Newton on one side, facing a picture of Madeline Albright, Bill Clinton's Secretary of State, on the other. An extract from one of her speeches sat side by side with the famous, modest claim attributed to Newton. That extract went thus: "If we have to use force, it is because we are America; we are the indispensable nation. We stand tall and we see further than other countries into the future." This breathtaking arrogance, this appeal to American exceptionalism as justification enough to use force to shape the world order as it wishes, this conceit has been appropriated by Trump, who admires dictators, despises 'shit-hole' countries, and tears up international agreements. But he has gone further. He has turned that arrogance and that violence inwards

against those that criticize him, against immigrants, liberals, Mexicans, Muslims, women, in the name of America first and white supremacy.

I have no birthday card for you today Jed. Instead I can only thank you and the entire group at Caltech for inspiring me intellectually and politically, and for reassuring me that, in fact, a better world is possible. In the process, though, I have become like one of the cacti in the Huntington's desert garden: increasingly prickly as I adapt to an extraordinarily hostile environment in order to survive.

Elizabeth Cavicchi

Effects, Devices, and Adventures

Jed Buchwald had a profound effect on my research in teaching and learning science and its connection to history. At MIT in the fall of 1994, he opened a world of historical effects that arose in historical investigations to understand electricity for me and my classmates Diane Greco and Babak Ashrafi, in a course on *Science, Technology, and Society (STS) 150: Aspects of 19th Century Physics*. The following term I joined these classmates in further sessions in Prof. Buchwald's office on the scientific revolution. Philip Morrison, my longtime undergraduate physics professor at MIT, had recommended this course during our discussion about my doctoral studies. STS 150 was the first, and only, history of science course that I ever took, so that at its beginning I could not foresee how fascinating and revealing electrical effects and their historical analysis would prove to be.

In my doctoral studies at the Harvard Graduate School of Education I was seeking to change how physics is taught and absorbed by avoiding the abstractions and problem formalizations I had encountered during my undergraduate training (and graduate physics courses elsewhere), and from the textbook-answer emphasis of engineering programs. I had earlier encountered past science as a set of colorful human-interest anecdotes when working as a researcher for Morrison's

1987 public science TV series and book, *Ring of Truth*.[1] But in Buchwald's lectures and readings I became aware of relationships, confusions, learning processes, and interpretations that arise during observations, experiments, and collaborations. I became intrigued by the long trajectory that connects past to present learners. Multifaceted, critical, investigatory relationships of doing and expressing science, as they figured in Buchwald's discussions of science history, are what I now seek to evolve in the classroom.

As Buchwald's student, I had yet to trust and research the possibilities for exploratory and active learning. Conventional instruction had framed the contexts of my undergraduate physics teaching. The previous year I had begun to explore alternatives to such conventional instruction on motion. With my advisor Eleanor Duckworth I studied the work of Jean Piaget with a view to learning and teaching processes as development, as an ongoing and interactive nonlinear, spontaneous, engaging dialogue with the world and our thinking processes. It occurred to me that the history of science might illustrate developmental processes, such as the questioning and uncertainty that I was beginning to see at the heart of learning and teaching.

Evidence of such instances of development and dialogue were almost nowhere present in physics studies such as the year-long graduate physics course on electricity and magnetism based on the demanding textbook *Classical Electrodynamics*.[2] Looking to follow that austere text in detail, I had worked out all the derivations, including steps that were frequently omitted, a practice exemplified even more in that course by

1. P. and P. Morrison. *The Ring of Truth: an inquiry into how we know what we know*. New York: Random House, 1987.

2. J. D. Jackson. *Classical Electrodynamics*. New York: Wiley, 1962.

my professor Richard Milburn. There was both elegance, and stress to the student, in grasping and deriving these equations. These exercises were entirely mathematical, analogous to the mathematics education at Cambridge in the 19th century described by Buchwald. Equations that Buchwald presented in his lectures—Coulomb's law, Gauss's law, and Maxwell's equations—were familiar. But his lectures expressed an awareness, interest, and outlook that were unlike that of physics instruction. I therefore became increasingly drawn into the course, and my STS 150 lecture notes seem as thorough as those I recorded in physics courses.

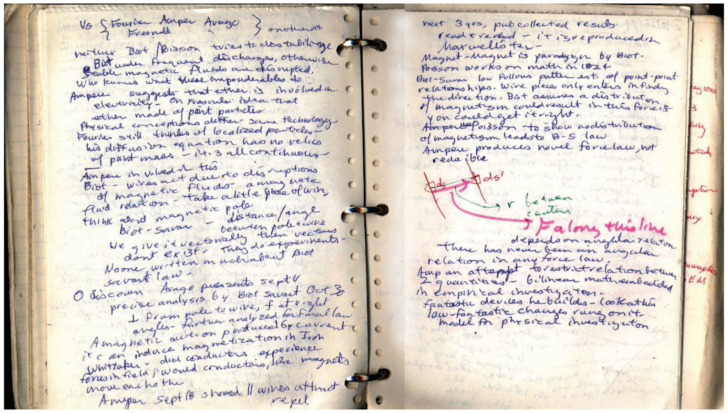
Numerous interpretations, experiments, and phenomena of electricity figured in Buchwald's lectures. Describing the Cartesian universe as completely filled, the Newtonian as mostly void, Buchwald asked: "Is electricity 1) Cartesian; 2) Newtonian? Are there two fluids or one?" These genuine questions were respectful of the depth and potential of conflicting interpretations. Where physics treats the conservation of energy as foundational and universal, not to be questioned, and evidence of a student's error when absent, Buchwald invited us to consider scientists for whom energy had not yet been identified, let alone conserved. Buchwald's view that Franklin's explanations of electrical charge could be "clear and consistent" and yet simultaneously inconsistent as concerns the relationship of the atmosphere to bodies suggested a source of tensions among renowned past investigators. These tensions were analogous to what I was beginning to notice as generative among active learners—although suppressed in conventional instruction. I became intrigued by the potential for dialogue and exchange between historical and contemporary learning experiences.

Of Galvani's account of contractions in the frog's leg, Buchwald asked: "Is it a novel fact? Is it different? Is it a new

class of phenomena?" And while some scholars had dismissed Galvani's findings, Buchwald noted the emergence of something new—the awareness of the character of a circuit: "discovery involves theory, not just observation; discovery is contextual." That attention to thinking and observation is what I was seeking to understand, learn, and facilitate among science learners.

I found a connection between Buchwald's responses to past efforts at understanding nature and Piaget's analyses of development which moves exploratively into new capacities and of limits and stasis where change does not come about. Buchwald described Ampére's work of suspending wires that act on each other as a challenge to electrostatic depictions of the voltaic pile that "produced current as a new category in nature." He emphasized how Franklin's principle of electricity, "What A loses, B gains, always an exchange," is analogous to the specific heats that J. Black measured with a calorimeter. While exchange is characteristic to a Newtonian outlook, Buchwald observed that Cartesian thinking left no opening for exchange.

It was also captivating to hear about the dynamic relation between thinking and experience. Oersted's sense of a unity in nature went beyond the realm of ideas. Buchwald said: "We must turn our attention to the world… where this truth will find its only corroboration; otherwise unity itself becomes a barren and empty thought leading to no insight." Oersted took risks when he performed his famous experiment for the first time during a lecture, placing a magnetic compass needle in various positions around a conducting wire. "Wouldn't you try it out first!?!," Buchwald asked of Oersted. He brought us to that day in 1820 when "Nature spoke loudly enough" in a classroom that its effect was "instantly reproducible" worldwide. While Oersted's finding posed "huge problems"



My notebook pages from STS 150, October 19, 1994.

for French theorists, reports of the wire's properties thrust Ampére into action. Initiating an "empirical investigation," Ampére suspended parallel wires that came together or pulled apart and produced a law describing a "novel" force due to its "angular relation." With awe, Buchwald identified the heart of Ampére's investigative work: "fantastic devices he builds—look at his law—fantastic changes rung on it. A model for physical investigation" (Figure 1). Investigation as taking risks and remaining open to 'the ringing of changes,' in Buchwald's frequent analogy, would become sustaining to the research, teaching, and learning I went on to do, and continue with.

In class, but without materials on hand, Buchwald encouraged us to try Oersted's experiment with a 1.5 volt battery, a paperclip, and a needle—"easy to make it happen." I had never before played with batteries—nor been invited to do so, or seen this effect, or observed other electromagnetic phenomena that our historical figures had observed. My entire training had been limited to theory. While we diagrammed electrical paths, oriented hands for the right hand rule, and calculated electrical outcomes, phenomena were seldom demonstrated.

Those who carried out the physical and intellectual developments discussed in Buchwald's class appeared only as names of units (oersted, farad, ampere, volt) or as carved inscriptions looming high over Killian Court at MIT, distant from the struggling learners below.

The magnetic effects of current-bearing wires became an experimental opening to the spatial character of magnetism in my dissertation project of redoing historical experiments, and later in my lab seminars for Harvard and MIT students, where we embarked on an extended exploration with batteries, bulbs, and wires. In long sessions, we developed an understanding of electrical relationships through creative and playful experimenting and discussion, while concurrently transforming our practice and vision of teaching and learning.[3]

Buchwald also introduced us to historical scientific instruments that are never mentioned in physics courses: the Leyden jar, the Volta pile, Ampére's wire devices, the telegraph, and the trans-Atlantic cable. On two occasions he ended class by bringing us downstairs to the gallery of the Dibner Library, where we could see an original Volta pile—as he pointed out, its metal end pieces being extraneous to the actual effect—and the amazing clear glass disc of an electrostatic friction machine standing on glass posts.

The possibility that past science might be experienced by any of us was not explicitly discussed. I was introduced to the work of historians involved in recreating scientific experiments, including the recent reconstruction of Coulomb's

3. E. Cavicchi. "Experimenting with Wires, Batteries, Bulbs and the Induction Coil: Narratives of Teaching and Learning Physics in the Electrical Investigations of Laura, David, Jamie, Myself and the Nineteenth Century Experimenters – Our Developments and Instruments." PhD dissertation, Harvard University, 1999.

torsion balance by Peter Heering,[4] who would become my longtime colleague. Emphasizing the lack of standardization in instrumentation and practice of the 18th century in regard to the ambiguities surrounding Coulomb's experiment, Buchwald's remark "I've never done that" may have been a precursor for the lab course he would later teach at the MIT Edgerton Center (where I now teach), and for reconstructions he would go on to carry out with my Dibner cohort member A. Martinez.[5] Uncertainty and complexity in such endeavors emerge as a theme across ongoing research into Coulomb's work,[6] and my work and that of my students.[7] Buchwald's attention to instruments as inextricable from science resonated with insights gained from Phil Morrison, and became integral to my teaching. Historical science instruments, and my humbler renditions of instruments, are central in my paper

4. P. Heering. "On Coulomb's Inverse Square Law." *American Journal of Physics* 60 (1992): 988–994; "The Replication of the Torsion Balance Experiment: The Inverse Square Law and its Refutation by early 19th-Century German Physicists." In C. Blondel and M. Dörries (eds), *Restaging Coulomb: usages, controverses et réplications autour de la balance de torsion*. Firenze: L. S. Olschki, 1994, pp. 47–66.

5. A. A. Martinez. "Replication of Coulomb's Torsion Balance Experiment." *Arch. Hist. Exact Sci*. (2006) 60: 517–563.

6. S. Heinicke & P. Heering. "Discovering Randomness, Recovering Expertise: The Different Approaches to the Quality in Measurement of Coulomb and Gauss and of Today's Students." *Science & Education* 22 (2013): 483–503.

7. E. Cavicchi. "Learning science as explorers: Historical resonances, inventive instruments, evolving community." *Interchange* 45(2014): 185–204; "At Sea: Reversibility in Teaching and Learning." *Interchange* 49 (2018):25–68; Y. Yang. "A Learner's Voyage: My Moon Study in 2009." *Interchange* 49 (2018): 69–84.

for STS 150,[8] my Harvard dissertation, my research as a Dibner Institute Postdoctoral Fellow,[9] and my teaching.[10]

Each experimenter discussed by Buchwald led me to further reading, and to envisaging them as future case-studies. But when it came to the figure that would sustain my fascination for years to come, my classmates and I fell behind in our reading. Starting a new topic on November 1, Buchwald's opening questions (which he presumably saw as easy, unthreatening, and obvious) "What is the Royal Institution? What was the training of Michael Faraday?" were met with dead silence. He rapidly recounted experiments whose startling effects and inferences "we know down to the hour." Faraday's writings and diary allow us to be at his side, moment by moment. In Faraday's work and records I found the connection to my aspirations for investigating and supporting active learning: they are a most vivid account, strikingly similar to Piaget's keen observing of infants in development.[11]

I thus became immersed in 19th century electromagnetic induction coils: I scouted for artifacts in collections, made my own drawings and interpretations of these coils, and examined experimental and therapeutic devices. Eventually I

8. E. Cavicchi. "Ways of Learning Physics: Magnets, Needles, Fields." Qualifying Paper, Harvard University, 1995; "Experimenting with magnetism: Ways of learning of Joann and Faraday." *American Journal of Physics* 65 (1997): 867–882.

9. E. Cavicchi. "Nineteenth century developments in coiled instruments and experiences with electromagnetic induction." *Annals of Science* (2006) 63:319–361; "Charles Grafton Page's Experiment with a Spiral Conductor." *Technology and Culture* 49 (2008): 884–907.

10. E. Cavicchi. "Historical Experiments in Students' Hands: Unfragmenting Science through Action and History." *Science and Education* 17(2008): 717–749 and note 5.

11. E. Cavicchi. "Faraday and Piaget: Experimenting in Relation with the World." *Perspectives on Science* 14 (2006): 66–96.

was thrilled to undertake my own laborious winding of wire coils. For months I tinkered with the sometimes intermittent and always beautiful sparking devices I had struggled to build—always recording my observations and confusions, like Faraday, in an ever-expanding notebook. Had I not taken STS 150, my students, colleagues, and I would never have experienced these effects, devices, and adventures.

Buchwald's course, infused with his probing understanding of phenomena, of ways in which those phenomena manifested in past devices and experiments, and of the evolving process of past scientists' investigations, was transformational for me and the work I had yet to undertake. All of us—learners, teachers, instrument-makers, researchers, and ordinary folk—are investigators in the world. While living and learning together we come to create a community of mutual respect and development through our sharing in intrinsically exploratory experiences that extend beyond any of our lifetimes.

Thanks to Prof. Buchwald for extending that welcome to me as a student and for students yet to come.

Chen-Pang Yeang

The "Buchwald School"

I came to know Jed in 1997, when I was a graduate student in electrical engineering at MIT. I was interested in the history of science, and followed Hasok Chang's recommendation to take a course with Jed. I still remember the moment when I walked into his office. By this time, I had already visited several MIT engineering professors' workspaces that all looked similar: a large desk with a big computer screen blocking the guest's line of sight; a few shelves piled mostly with periodicals; file cabinets; an equation-filled white board smelling of markers and, occasionally, an optical table, circuit boards, or a mechanical prototype. These offices were located either in a bunker-styled building of World War II legacy, or in one of 1960s brutalist architecture. Jed's office as director of the Dibner Institute was different: tall ceiling to floor bookcases stacked with antiquated tomes and hardcover volumes; a big window overlooking the Charles River; a sketch of a man in contemplation (who was Max Planck, as I later learned) hanging on one wall. I resist the temptation to invoke images from Umberto Eco's novels, or to agree with a senior colleague and call the space "a banker's office." Yet the room where I saw Jed for the first time did emanate a distinct sense of aura.

[the debate surrounding the method of fluxion and differential calculus, chemical atomism, Maxwellian electrodynamics, Fresnelian optics, Carnot cycles, the adiabatic expansion and the propagation of sound, the quantum hypothesis for the black-body radiation, and a lot more. And of course he talked enthusiastically about Heinrich Hertz's radio-wave experiments, on which he had just written a book. We spent time trying to figure out puzzling passages from Newton's *Principia* or *Opticks*, Huygens's *Horologium Oscillatorium*, articles in Laplace's *Oeuvres*, or Sommerfeld's *Atombau*. Most of these original texts were readily available on his bookshelves. Once in a while, however, he would descend to the Dibner Institute's depository, retrieve a rare book, and bring it back to his office. Some of these books most likely had never been properly read, since Jed had to use a paper knife to cut 'open' the folded leaves.

But Jed and I were not the only head-scratching individuals in the Dibner Director's office. Babak Ashrafi was another frequent student. Theresa Levitt participated in the reading course for a semester. From time to time, guests and visitors—including George Smith, Edith Sylla, Allan Franklin, and Ursula Klein—joined the discussions. It was not until much later that I became aware of the uniqueness of that experience for a neophyte studying the history of science. But the influence was prompt. Right after obtaining a degree in electrical engineering in 1999 I started to pursue a PhD in the history of science and technology at the MIT Science, Technology, and Society Program (STS).

After that, my apprenticeship with Jed turned a new page. He became my dissertation supervisor. I received more and more advice from him about my research on long-distance radio-wave propagation, as well as my professional development, writing, and career planning. We had frank conversations about the situation of the academic field in the history of science and technology. I also learned one other new thing from him during this period: the replication of historical experiments. Jed co-taught a lab course with Larry Bucciarelli for MIT undergraduate students at the Edgerton Laboratory, above the Infinite Corridor of the Tech's dome building. I was the only humanities graduate student in the room. We were tasked with replicating a few famous past physics experiments. We started with Ptolemy's test of refraction when light travels from air to water, but spent most of the semester struggling with Coulomb's measurement of electrostatic force. If reading Coulomb's terse and ambiguous description of his experimental procedure and results, and building and calibrating his torsion balance and glass chamber were already challenging enough, trying to obtain data with any remote resemblance to his numbers was even more difficult. In the end, none of the student groups were able to reproduce Coulomb's numerical results. A successful replication did not come about until Al Martinez's careful investigation in California, with Jed's guidance, many years later. Yet, this coursework was an eye-opening experience for me. While most scholars I had studied promulgated the examination of textual sources as *the* method of doing historical research, Jed and Larry taught me that the manipulation of materials via experimental replication could be an important element of historical research, too. Today, "labs" for history of science and technology are proliferating in North America and Western Europe. Prominent historians employ experimental replication as a means to examine tacit knowledge, make sense of esoteric scientific and technical texts, and explore the corresponding science. Philosophers and scientists talk about the crisis of replication. "Making and doing" expand the horizon of STS scholarship. To me, what we did in the Edgerton Lab was an early rehearsal for many of these exciting current developments.

In 2001, Jed moved from MIT to Caltech's Division of Humanities and Social Sciences, an academic unit without a doctoral program in humanities. That meant he would no longer accept new graduate students, and I thus became his last PhD supervisee.

There is an old description of a particular student in Chinese: Guanmen Dizi (關門弟子), the 'closed-door disciple.' The phrase refers to the last protégé admitted by a Buddhist or Daoist guru, a Confucian savant, or a Kung-fu master before he (metaphorically) closed the door of his academy and retired. In many Chinese legends, the 'closed-door disciple' carries the torch of the master's teaching, refines the spiritual, scholarly, or technical legacies of his school, or spreads them to the wider world. I am certainly not Jed's 'closed-door disciple' in any of those senses—he has trained numerous scholars and researchers who have made much more fantastic contributions to the history of science and technology. But in retrospect, I do think that I have learned a thing or two from the 'Buchwald School,' if there is such a thing. That school is characterized not only by a wealth of knowledge in the history of physics and mathematics, by the preoccupation with technicality, the preference for the "internal" approach—a methodological hallmark that Harry Collins has labeled the "technical history of science"—or the writing of papers and books filled with equations, diagrams, and descriptions of instruments and procedures. Rather, the 'Buchwald School' to me is the embodiment of an attitude toward historical scholarship—the attitude of paying supreme attention to details; of conducting research with extreme caution but bold hypotheses; of being driven and intrigued by the burning curiosity about what exactly happened, how it happened, and why it happened in this, and not that, way; and of letting facts and evidence speak for themselves but insist on finding reasonable interpretations. As a historian, I have taken these lessons to heart.

In the spring and summer of 2002, Jed arranged a visitor position for me at Caltech in order that we might work

together on my dissertation. When I saw him in Pasadena, he looked happy and relaxed. Although he no longer had the Dibner director's office overlooking the Charles River, his spacious two-room suite next to the Einstein Papers Project 'villa' had an equally magnificent view of a Caltech garden. The Californian sunshine and Diana's company cheered him up. Perhaps this was the environment that encouraged him to explore new research directions. In the following years, he ventured from the familiar terrains in the history of physics and mathematics and into the origins of antiquity studies in Europe from the 17th to the 19th century. With Diane Greco Josefowicz and Mordechai Feingold he wrote books on a controversy over the interpretation of an ancient Egyptian zodiac in Napoleonic France, Newton's bewildering inquiries on Biblical chronology, and the deciphering of Egyptian hieroglyphs by Thomas Young and Jean-François Champollion. This change in direction from wave optics and Maxwellian electromagnetism to Egyptian hieroglyphs and Biblical chronology may appear no less drastic than genre-switching from Stephen Hawking to Dan Brown. But underlying such a dramatic change of themes one can see a strong continuity that characterizes the aforementioned 'Buchwald School': an incessant scrutiny of all available primary sources in print and in archives, an integration of texts with other types of historical materials, an extremely careful examination of the data that does not shy away from quantitative analysis, and always the presence of an enticing, firm, and thought-provoking story. Although I know close to nothing about the topics Jed has been working on over the past eighteen years, I observe with awe and admiration his successful launches of book after book.

Recently, Jed returned to his old pal Heinrich Hertz. After more than a decade of work on the origins of antiquity studies, he decided to write a sequel to his 1994 book on Hertz’s discovery of radio waves (*Buchwald 1994a*), a project he planned while at the Dibner Institute. I have the honor of being his collaborator for this project, and we have already had some fun working on this undertaking—watching together the bright and buzzing spark-gap devices in operation in the basement of the Sidney Smith building at the University of Toronto and also at Paolo Brenni’s impressive instrument museum at Fondazione Scienza e Tecnica in Florence. We have also been exchanging Matlab codes for calculating electromagnetic scattering from interesting boundary conditions, etc.

I appreciate the opportunity to express my gratitude and my appreciation of him as a mentor and model scholar at the celebration of his 70th birthday. And I look forward to collaborating with him on this exciting new project.

Karine Chemla

Ancient and Medieval Science in Peril

In 2016, I had a few months of intense interaction with Jed. We had decided to put our names forward for the positions of President (Jed) and Perpetual Secretary (myself) of the Académie Internationale d’Histoire des Sciences in the 2017 elections. We prepared a joint statement addressing the most important actions necessary for the future of the history of science. One paragraph still resonates in my mind, since I am convinced that it pointed out essential challenges that our field should grapple with. We wrote: “Situated at the crossroads of many disciplines, our field is institutionally fragmented, which affects its visibility, threatens its cohesion and jeopardizes fair assessment. The Academy can play a significant role in fighting against fragmentation and helping shield the field from the **dismantlement** of institutions and **loss of positions**.” The bold characters were in the original text.

I would like to return to these lines, and in particular expand on the current state of the subfields of ancient and medieval history of science. This will be an invitation to Jed to return to a conversation begun on that earlier occasion and interrupted by circumstances. I further hope that my reflections might create the opportunity of addressing and pondering collectively the broader features of the evolution of our field.

Indeed, fragmentation of the history of science is manifest when we look at our current complex institutional structures. Some of us work in institutions devoted to the history and/or philosophy of science in general, or to that of a particular field, such as the Institute for the History of Natural Sciences of the Chinese Academy of Science in Beijing; the Centre Alexandre Koyré in Paris; the Harvard Department of the History of Science; my own research group at the University Paris Diderot (SPHere, that is, Science—Philosophy—History); or the Department of the History of Medicine at Johns Hopkins University. Some of us, however, teach in departments of history, such as our colleagues at the Goethe-Universität Frankfurt, the University of California at Los Angeles, the University of Wisconsin-Madison, and the University Paris Panthéon Sorbonne. Still others teach in mathematics and, more generally, science institutions. I think here in particular of three colleagues working on the history of the astral sciences in Sanskrit: Toke Lindegaard Knudsen (Mathematics, Computer Science, and Statistics, SUNY Oneonta); Clemency Montelle (Department of Mathematics and Statistics, University of Canterbury, New Zealand); and Kim Plofker (Union College Mathematics Department). Finally, we also have colleagues hired in philosophy departments, like the late Jean Gayon.

To these rather diverse types of institutional settings we must add several others for those of us working on what is all-too often referred to as "non-Western sciences." One must certainly concede that during the last decades a major change took place, since many are now hired with colleagues working on Europe or North America in institutions of the type mentioned above. However, in addition, we find colleagues working in the context of research centers devoted to East Asian Studies, like Annick Horiuchi, who carries out her research on the history of knowledge in Japan at the Centre de Recherches sur les Civilisations de l'Asie Orientale; or to South-Asian studies, in the context of which Christopher

Minkowski develops his work on Sanskrit astral sciences in the Faculty of Oriental Studies at the University of Oxford.

We reach a peak in institutional scattering of research efforts when we observe the history of ancient and medieval sciences and, in particular, those that were written in Chinese, Demotic, Greek, Sanskrit, and cuneiform scripts. In the mathematical sciences alone, Reviel Netz and Mark Schiefsky are in Classics departments, and Marc Kalinowski in East Asian Studies, whereas Daniel P. Morgan is in history and philosophy of science. Annette Warner-Imhausen teaches at the Historisches Seminar of the Goethe-Universität Frankfurt, whereas John Wee and John Steele work in The Oriental Institute of the University of Chicago and in the Department of Egyptology and Assyriology at Brown University, respectively.

It is true that institutions have created several resources to counter this fragmentation, such as joint appointments and programs in the history of science in which colleagues hired by different departments can teach. This is the case, for instance, of the programs in history and philosophy of science at Stanford University and at Seoul National University. Theoretically, one might thus argue that the diversity of positions open to history of science is a good thing (and the list goes on expanding with new types of appointments, e.g., in media studies). This expansion certainly suggests that, in recent decades, history of science and technology has been a successful field. Some colleagues believe that such an expansion actually creates institutional opportunities for the development of our field that will allow it to thrive even more. Given the multidisciplinarity of the history of science, it might in fact be difficult to avoid such a scattering. One might also argue that the benefits that the history of science derives from this situation are not merely institutional. Being in contact with mathematics, the exact sciences, history, or "area studies," to name but a few, also allows the history of science to draw fruitfully upon methods and types of questions from these domains. The history of science has indeed benefitted from its ties with many domains, and this certainly could have been an asset.

But here some doubts arise. Over the last decades we have witnessed how potentially fruitful approaches have created divisions in the history of science that seem to me detrimental to our field and which we have not yet resolved. Since the 1970s, alongside the classical conceptual history of science, new approaches have gained momentum and led to significant developments in, e.g., cultural and social histories of science and the sociology of science. This diversification was soon followed by an expansion of the subject areas covered by the history of science. One consequence of this extension was the inclusion in the history of science of, and in some cases its replacement by, a "history of knowledge" ("histoire des savoirs," "Wissensgeschichte.") Research on these transformations is still needed. However, in a first approximation and for the sake of discussion, one might put forward the hypothesis that these changes correlate with the multiplication of institutional sites in which history of science is practiced. The development of the field in the context of different disciplines confronted the practitioners with different audiences and different disciplinary norms. These different disciplinary contexts led historians of science to value specific (and different) types of topics and issues, and to abide by different expectations of rigor. As a result, the practice of the history of science diversified, and the different ways of conducting research in this field have proved uneasy to reconcile. Moreover, research is being published in specialized journals, in the

journals of the various disciplines, and in a number of edited volumes. The shape of the history of science becomes increasingly difficult to grasp, and this fuzziness affects its visibility. In addition, the viewpoints from which to assess research in our field have also multiplied, making work in our field quite difficult to evaluate. Varying concepts of rigor are in competition and yield completely different results when applied to the same piece of writing. In my view, this situation jeopardizes the cohesion of the history of science in a concrete way and requires further reflection.

The multiplication of types of institutional contexts in which historians of science do their work has undermined the cohesion of the field. This evolution has thus been perhaps even more noticeable in the history of ancient and medieval sciences, because of the greater number of disciplinary contexts in which research was and still is carried out in these fields. However, the history of ancient and medieval sciences has been hit by another general trend, to which so far, to my knowledge, not much reflection has been devoted.

As I have mentioned above, the more general field of history has become more diverse, since in most departments we now find historians dealing not only with Europe or North America, but with many other parts of the world. But this development has been accompanied by another general transformation, namely, a drift toward early modern and modern history. Institutional configurations in history and philosophy of science reflect this wider trend too, since the study of ancient and medieval sciences has been progressively marginalized in them, and in some cases has even simply disappeared. In this process, I think, a divide has appeared between these subfields and the history of early modern and modern sciences. Today, historians of ancient science rarely work or cooperate with specialists in modern history, as if they belonged to different disciplines.

One might think of fragmentation as an unavoidable consequence of increasing specialization. However, the case of our field is rather special. One ought at least to address the issue of whether this additional fragmentation would not be detrimental to the history of science. This seems to me a second major threat to the cohesion of this field, and a loss for the history of science. The history of ancient science and that of medieval science are subfields that need to be approached at a world level. At a moment when our profession attempts to reach for all-encompassing approaches, neglecting the contribution made by these two subfields is deleterious to the conversation.

But, there is more. Historians of ancient and medieval sciences deal with documentary evidence of a type quite different from that employed by modern historians. Historians working on antiquity and the middle ages have devised methods and developed reflections that differ from those of modern historians and can contribute to the field more generally.

Finally, the scattering of specialists in ancient and medieval science, their quasi-elimination from major centers in the history of science, and the general decrease of the number of specialists jeopardize this part of our field, at a moment when, worldwide, the history of ancient and medieval sciences is caught up in various forms identity politics. Therefore, anchoring these subfields in the general history of science and not leaving them in the hands of those for whom historical rigor is unimportant have become a matter of urgency.

At the international level, has the field become an assembly of sub-communities barely communicating with each other? Or do its practitioners still share a sense of participating in

a joint endeavor? What are the consequences for the types of research carried out in the history of science? Could we put forward new general goals that would elicit a greater integration of the field? How can teaching programs in the history of science and technology contribute to such integration? These are some of the questions that I think we should raise, and I look forward to discussing them with Jed and other colleagues.

Manfred D. Laubichler and Jürgen Renn

Daring to Ask the Big Questions

In a long conversation about his life and career, Murray Gell-Mann told fellow physicist Geoffrey West about his family's origin in Czernowitz. He was proud to come from this unique region at the intersection of Eastern and Western European cultures. At the turn of the 20th century, Czernowitz was a border town of the Austro-Hungarian Empire with a sizeable Jewish population. Many writers, intellectuals, and scientists came from that region. But when Murray also said that he therefore is Austrian, Geoffrey felt compelled to point out that at the time when Murray's parents left for America, Czernowitz was actually part of Romania. Murray would have none of it and understandably kept insisting that he is Austrian, because, as a physicist, this would put him in the kind of polymath company he felt most at home with, that of Mach, Boltzmann, and Schrödinger.

What does this anecdote about Gell-Mann have to do with Jed Buchwald? For one, they share an ancestral culture: Jed's family also hails from this part of the world—one much closer to Vienna, as Jed would insist, should he have a conversation with Murray on this topic. They share a polymath orientation anchored by a deep understanding of physics, but extending to history, philosophy, archaeology, and the arts. Murray and Jed also share a conviction that the kind of knowledge they treasure—a deeply connected form of knowledge that brings together the present and the past—needs a home in which

to develop and grow. This led Murray to establish the Santa Fe Institute, a unique place in the mountains of New Mexico devoted to "exploring the frontiers of complex systems science," which one of us (ML) calls his scientific home.

And Jed? Well, where to start? There are few in the history and philosophy of science professions who have not, in one form or another, benefitted from an infrastructure built by Jed, be it the Dibner Institute and the projects it supported, the numerous book series that Jed continues to edit, the *Archive*—the premier journal for technical work in the history of science, the group he built at Caltech, or simply his generous support and (mostly) welcome critical encouragement.

For us as evolutionary historians this begs the immediate question whether there exists an ancestral state from which both Jed and Murray descend that is not only a joint cultural region, but something more tangible. And indeed, it does not take long to discover the common ancestor, or rather the ancestral archetype linking them together. If we focus on the main elements of what taxonomists refer to as a character matrix—a deep understanding of physics and of history, an evolutionary conception of knowledge, a polymath form of curiosity, and a fondness for kvetching—we quickly discover a perfect match: Ernst Mach, whose origin is geographically closer to Jed, chronologically closer to Murray, but whose fundamental characteristics were remarkably similar. None of the three accepts the fragmentation of knowledge—which began in Mach's time and has steadily increased to this day—as an inevitable outcome of the evolution of knowledge.

Overcoming fragmentation requires the asking of big questions. This goes against trends in history and philosophy of science that might content themselves with ever more contextualized investigations, for instance, about Mach's sources for his evolutionary thinking or the influence he had on others, but other than that ignore Mach's claims about the evolutionary character of science, knowledge, and culture. This seems to be a great disservice to Mach's legacy. Probing the relevance of this legacy to the approaches and methodologies of today's humanities is surely risky, and may be even considered whigish, but its renunciation irrevocably declares Mach dead a century after his biological demise. Thankfully, there are those like Jed and Murray who encourage the asking of big questions.

Could it be that the humanities are in no need of resurrecting Mach's legacy? We do not believe so. For good reasons, the history of science is currently being extended to a history of knowledge. As it turns out, it is impossible to understand important developments in the history of science, such as the rise of modern physics during the Scientific Revolution, without taking into account the wealth of practical knowledge that served as the underpinning of the new physical theories, such as ballistics, machine technology, ship building, or military architecture. It is no accident that Galileo begins his famous *Discorsi* with a reference to the Venice Arsenale.

Furthermore, the highly contextual microhistories that have dominated the history of science in the last decades are insufficient to capture long-term aspects and structures of the history of scientific thinking. In general history, but also in history of science, or rather, in history of knowledge, the possibility of writing global and big history is now seriously being considered and discussed. Think of the long shadow of antiquity in the European tradition, or of the millenary exchange of knowledge between Europe, China, and India.

But what are the structures and explanatory modes that could frame such approaches? Some of the most prominent studies in historical epistemology essentially follow a taxonomic structure that classifies certain modes of knowledge production and emphasize the deep historicity of these practices, their embeddedness in other historical contexts, and their contingencies. Can one go beyond such descriptive narratives and aim for a more explanatory approach that does not ignore all that has been learned from the historiography of science in context? A return to a linear notion of progress or to simplistic concepts of evolution is hardly promising. Finding an appropriate concept of evolution for the history of knowledge is, indeed, a challenge, one that is rarely addressed. A second challenge to a synthetic approach is posed by the enormous number of existing detailed case studies. Only a naive and uninformed follower of 19th century thinkers would dare investigate the possibility of such a synthesis, or so it might seem to traditional humanists who are not aware that, even in the humanities, we are today in a position, at least in principle, to actually deal with big data.

Here we wish to explore the possibility of such a synthesis. We believe one ought to follow our colleagues in the natural sciences or at least the courage they have demonstrated. Erwin Schrödinger famously asked in 1944, in the preface to his trailblazing book, *What Is Life?*: "I can see no other escape from this dilemma (lest our true aim be lost forever) than that some of us should venture to embark on a synthesis of facts and theories, albeit with second hand and incomplete knowledge of some of them—and at the risk of making fools of ourselves."

We are quite willing to run this risk and ask the big question: How can one conceive the history of knowledge as an evolution of knowledge? This very question may provoke violent criticism. Mach, however, was an early reader of Darwin and, between 1864 and 1867, soon after the publication of the *Origin of Species*, held lectures in Graz on "the evolution of human knowledge as the result of a competition of scientific thoughts, as the survival of the most adapted." In April 1913, he wrote to the Secretary of the Austrian Academy of

Sciences: "The origin of my biological epistemological theory also owes much to this influence from the physiological side, a theory which has so alienated many physicists from me that I neither understand their speech nor they mine, which is why they have used means to proceed against me after the fashion of Pius X."

What Mach calls his biological epistemological theory has since been severely criticized by philosophers and historians for its reductionism. Indeed, some of his formulations suggest just that: "Slowly, gradually, one thought is transforming into another one, as it is likely that one animal species is gradually passing over into another one. Many ideas appear simultaneously. They carry out their struggle for existence in no other way than ichthyosaurus does, as well as the Brahman, and the horse. Few survive, spreading quickly over all areas of knowledge, to evolve further, to divide, and to take up the struggle again."

In his concise criticism of evolutionary conceptions of scientific change, Kurt Bayertz has ascribed to Mach a biological criterion of objectivity that anticipates later evolutionary epistemologies. According to his interpretation, the motivation for the development of science is, for Mach, a biological need, and the mechanism of its evolution is also quasi-biological. Bayertz admits that evolutionary conceptions have contributed to historicizing science, but accuses them of tending to eliminate the role of the subject and to reduce history to natural history. But this criticism actually does not do justice to the richness and potential of Mach's conceptions, let alone to some recent ones grounded in a more sophisticated understanding of evolution.

Mach was one of the first to propose an evolutionary theory of knowledge, but he was by no means alone. Prominent proponents of such theories include Karl Popper, Thomas Kuhn, and Stephen Toulmin. By the 20th century, the environment

for such proposals had drastically changed. The emancipation of new disciplines such as psychology, the growing specialization of the sciences, the linguistic turn in philosophy, and the later political disasters led to a veritable split of rationality which has made it difficult to combine ideas from biology, the history and philosophy of science, psychology, philosophy, or logic as freely, or perhaps as naively, as Mach had done. At about the same time, the study of "Entwicklung" in the life sciences underwent a similar split of rationalities after which development, inheritance, and evolution were now becoming ever more independent subjects of investigation.

Popper's *Logik der Forschung* can be read as a polemic against Mach. Popper himself had studied associative psychology in the mid-1920s, but eventually came to the conclusion that it was not helpful in explaining learning and cognition. Popper tried to split what Mach had attempted to connect, as Bayertz formulates succinctly: namely, the nexus between experience and theory. But in the early 1960s Popper returned to an evolutionary language. He wrote: "The epistemology that I want to propose is largely a Darwinist theory of the progress of knowledge. From the amoeba to Einstein the progress of knowledge is always the same: we try to solve our problems and, by selection, come to more or less useful solutions." But such statements hardly added anything of substance to his epistemology.

On the other hand, in the 1970s, Toulmin proposed a broader evolutionary approach to the history of rationality. He argued against the role of supra-historical principles of rationality in favor of an ecological perspective: "Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds—acknowledging the shortcoming of their former procedures and moving beyond them." In contrast to logical empiricism, Toulmin did not consider science a hierarchical system of propositions but as a collective of "conceptual populations" held together by the disciplinary aims of the participating scientists that are otherwise open to evolutionary changes. These changes take place as interplay of intellectual variations and selection among competing ideas. One key problem, however, is that the idea of "conceptual populations" remains rather vague and its analogy to the biological concept of species problematic. Another key problem is that, in the evolution of science, the generation of variation and the changing criteria for selection are coupled processes, in contrast to the standard model of biological evolution, so that, in the end, the analogy with Darwinian evolution remains rather hazy.

It would be easy to enlarge our list of examples, as for instance Kuhn's appeal to evolutionary ideas, or Paul Thagard's approach to conceptual revolutions, or recent work in cultural evolution. It would not be difficult to show that, while such approaches go beyond Mach's biological epistemology in important ways, they also tend to neglect some of its crucial stimuli that we believe are today more relevant than ever. In particular, recent advances in evolutionary theory hold the promise of overcoming the split of rationality mentioned above, and make use of some of the richer connotations still present in Mach's conception of Entwicklung. Let us just list a few of them:

We should conceive the history of science not in isolation but on the background of an evolution of knowledge in the spirit of Mach. A historical epistemology that searches for universal criteria of *scientific* development, as Michael Heidelberger once proclaimed it, is a futile hope. Knowledge can be conceived, with Mach, as a regulative of actions and as a dynamic

adaptive process. The evolution of knowledge proceeds through multiple regulatory niches and is a continuous and path-dependent process, just as Mach saw it: "Knowledge is being gained along manifold twisted pathways and the single steps are conditioned by the preceding ones, but also shaped by contingent physical and psychic circumstances."

There is a rich and underused potential in Mach's evolutionary thinking, unconstrained by the twin splits of rationality in evolutionary biology and the thinking about science. This brings us back to the big question: can one formulate an evolutionary theory of knowledge? What problems could it solve and how would it solve them? As we have pointed out in the beginning, two of the most challenging problems of today's history of science are to explain *longue durée* developments and account for the fact that scientific knowledge is just a special form of knowledge that cannot be considered separately from other forms of knowledge. Both of these challenges matter when it comes to questions such as: what is the long-term history of spatial thinking, what enabled Einstein's theories of relativity to overcome centuries-old preconceptions on space and time, and why had these conceptions been so stable in the first place? There is no doubt that a historical epistemology of space that also constitutes an evolutionary account of the human mastery of space should be a theory of human practice and of human thinking. It should take into account different kinds of regulative structures, in particular social as well as mental structures.

Developmental biology teaches us that knowledge is a function of the human organism, especially of the brain; as such, knowledge is a result of biological evolution. Studies of developmental psychology have also made clear that many fundamental structures of knowledge are not present at the outset of life, but are constructed over the course of child development.

Finally, the history of science teaches us that fundamental concepts, such as space and time, have changed over the course of history and can possess different meanings in different cultures. In summary, three strands of development can be distinguished: phylogenesis, ontogenesis, and historiogenesis.

The historiogenesis of knowledge is the object of historical epistemology. The continuity of development is given here by the external representations of knowledge, which serve the societal reproduction of knowledge structures within a culture or the transfer of knowledge between cultures. These external representations, such as language, characters, symbols, and tools encode experiential knowledge while simultaneously becoming prerequisites for further experiences and the construction of new knowledge structures. These can then be encoded through external representations of a higher order, which then, in turn, become the prerequisite for further development. The historiogenetic strand of evolution is linked with the other two in different ways. First of all, phylogenetic and historiogenic factors were intimately connected in the genesis of humans. Not only was biological evolution the prerequisite for the emergence of human culture, but as we know, this culture, for its part, decisively shaped the final steps of anthropogenesis, especially if we think of the biological repercussions of the use of tools and of social interaction.

Secondly, the development of the species is realized both phylogenetically and historiogenetically by the ontogenesis of the individual. The historiogenesis of cognitive structures depends on individuals who acquire the shared knowledge of a society at a certain historical moment in their ontogenesis and participate in the transmission and transformation of this knowledge through their cognitive activities.

The entanglement of the ontogenetic and historiogenetic developments of cognition explains why the means of externally

representing the understanding of the long-term development of knowledge is so important. These external representations mediate between socially shared knowledge, which is the object of historical development, and individual knowledge, which, despite all random processes that characterize the biographies of individuals, is the only true expression of this shared knowledge. The external means of representing knowledge define a space of possible transformations of shared knowledge. A historical epistemology of space must thus aspire to formulate the theoretically identifiable processes and stages of development that demarcate the horizon from those forms of spatial thought that are possible in a given situation.

On this background, what kind of stages can we distinguish in the evolution of spatial thinking and, more generally, in the human mastery of space by practice and thinking? Such questions are rarely addressed on the basis of detailed investigations of the historical sources combined with an overarching theoretical framework. Mach was convinced that one can actually trace the evolution of spatial thinking from animal behavior, via the practice of artisans, to the most sophisticated theories of non-Euclidean geometry. Matthias Schemmel from the Max Planck Institute for the History of Science and his research group suggest the following broad stages:

1) Naturally conditioned space, in the sense of schemata of action based on the similar biological constitution of all humans and the fundamental similarities in their physical environments. Here we are reminded of Mach's physiological space. These schemata of action are rooted in sensorimotor intelligence that allows for spatial inferences to be drawn in the context of action and perception but are otherwise inaccessible to the actors.

2) Culturally shared space, which is externally represented by the natural and cultural environment, by culturally

conditioned actions and by language, and builds upon the mental structures of naturally conditioned space, endowing them with cultural meaning. We can think here, in particular, of all forms of preliterate societies, past and present.

3) Administratively controlled space, which is externally represented by large building projects, by measuring tools, arithmetic and linguistic symbols, and tools for graphical representation such as the compass and the ruler. We can think here of the ancient civilizations of Babylonia, Egypt, China, and India.

4) Second-order concepts of space, as they are externally represented by written texts, possibly comprising diagrams, formalized language, and other symbol systems. Historically, we can think here of Babylonian, Egyptian, Chinese, Indian, and in particular Greek philosophy and science, and their long term consequences.

5) And finally, empirically and disciplinarily imposed spatial concepts and practices, as they emerged in the course of the expansion of spaces of experience by political expansion, trade, exploration, and engineering. Historically, this expansion was particularly strong in the early modern period and went along with new forms of organizing spatial knowledge both socially and intellectually.

Clearly the above is only a sketch of how an evolutionary theory of knowledge could provide richer and more connected explanations of the development of our understanding of nature. But it highlights what we mean by daring to ask big questions. And for encouraging us to do that we are immensely grateful to Jed!!

A final note: Most of this contribution was written in a Viennese Kaffeehaus, channeling the spirits of Jed, Murray, and Mach.

Alberto A. Martínez

Experiences and Experiments in Mentorship

I first met Jed Buchwald at a Seven Pines Symposium event in the 1990s. I was a graduate student at the University of Minnesota, working on a dissertation on the origins of Einstein's special relativity. I knew his solid books on *The Rise of the Wave Theory of Light* and *From Maxwell to Microphysics*, so I wanted to meet him. I remember that he looked very serious and tough, even a bit intimidating, but I needed to talk to him to get advice on my research. My advisor, Roger Stuewer, had suggested that I talk with Buchwald. So at one point I went up to him, and he replied something like: "later, after dinner." So, at the end of the last conference day I waited to talk to him, while he chatted with other professors. Stuewer and others were telling jokes. Still, for me it was serious, because I was waiting to meet with Buchwald, and back then I was standoffish and shy around professors, as I sort of felt that I didn't have much to say to experts who knew so much. Finally, he met with me one-on-one. He had stern eyes and asked me brief questions as if he were cross-examining me. Then, he launched into an encyclopedic summary of the history of theories of the ether and light. It was an impressive and authoritative account. That was the first time Jed helped me.

By January 2001, I completed my PhD and applied for a postdoctoral fellowship at the Dibner Institute. At the time, Jed was still its director, and therefore, I think, a key participant in the selection of fellows. In early 2001, I was in Washington

D.C. on a research fellowship, but for fall of that year I had not applied to anything at all other than the Dibner Institute. It felt like my one and only shot. For recent PhD graduates, these fellowships seem crucial, like sink or float, and when I received the letter offering me a two-year postdoctoral fellowship I was elated and relieved and felt that I owed my admission to Jed Buchwald. Since I hardly knew him, this also felt like a validation of my work, and I greatly appreciated it.

But when I arrived at the Dibner, Buchwald was gone. He had moved to Caltech. Still, I enjoyed laboring for two years at the Institute that he had shaped, and I subsequently worked for one year on the ongoing digital project *History of Recent Science and Technology* that he had initiated as well.

By the end of my time at the Dibner I had not published anything at all. I was working on several manuscripts but everything was 'in the pipeline.' I unsuccessfully applied for a postdoctoral fellowship at the Max Planck Institute. So again, at such junctures one's career seems to hang by a thin thread, and the personal experiment of working in academia seems awfully vulnerable to a lack of opportunities. Luckily, I had a forthcoming essay review in which I critically analyzed a new book, *Einstein's Clocks, Poincaré's Maps*. The Director of the Dibner, George Smith, forwarded my essay to Buchwald at Caltech and, fortunately for me, it turned out that Buchwald was sufficiently impressed with my manuscript that he kindly offered me a visiting position at Caltech. Out of the blue. So once again, I owed my academic survival to Jed Buchwald.

Thus I was appointed to be the Weisman Instructor in History of Science at Caltech. It was good for me, not just because it helped me to continue my research and writing on relativity and history of mathematics, but because I would co-teach a course with Buchwald. He decided that it would be on the replication of famous scientific experiments, something

he knew plenty about. He was thus in a position to be my mentor on this subject. Most importantly, he suggested that we should try to replicate Coulomb's famous electrostatic torsion balance experiment of 1785, and I agreed. This experiment seemed especially intriguing because, in 1992, Peter Heering had tried to reproduce it and famously concluded that Coulomb could not have obtained his alleged experimental results by using the device he had described. According to Heering, Coulomb had confected his data in order to match his theoretical expectation: the inverse square law.

However, Jed strongly thought that Coulomb had probably not fudged his data. Why? Because, Jed argued, Coulomb's measurements of electric charges had been strikingly confirmed in 1811 by S. D. Poisson's mathematical analyses of the distribution of charge on the surfaces of conductors. Poisson obtained deviations of less than 3.3% between his calculations and Coulomb's measurements, thus showing the accuracy of Coulomb's work in a domain in which he did not have the mathematical knowledge to predict such results independently of his experiments. Therefore, Jed thought that indeed Coulomb had used his electrostatic torsion balance to arrive at his measurements' results.

Jed had once unsuccessfully tried to reconstruct Coulomb's device at MIT in collaboration with students. So we tried to replicate Coulomb's experiment at Caltech in 2005. Late 18th century physics was not my field and neither were experiments, but with Jed's help I managed to get a solid footing. Jed gave me his initial mathematical analyses, relevant journal articles, along with the glass cylinders he had used at MIT, plus abundant advice. I worked to find the appropriate materials for reproducing all the components of the experiment, part by part: smoothly polished pith spheres, extremely delicate silver wire as thin as human hair, tiny metal clamps, etc.

I painstakingly and intensively worked on the experiment for four months, and subsequently wrote a long paper based on my findings. Long story short, it worked.

Peter Heering had explained that in his efforts to reproduce the experiment he had constantly observed erratic motions of the hanging needle which was supposed to measure the force of electrostatic repulsion, motions that had introduced errors. But at some point in my experiments the needle finally behaved in a very stable way: the pith ball at the end of the needle was suspended very stably, unmoving, a fixed distance away from a stationary ball. It was really stunning to see. I wrote Jed in an email that night: "Eureka!" Once the needle behaved in a stable way it was now a matter of gradually refining every part of the experiment to see whether three consecutive measurements could yield an exponent of 2 for Coulomb's inverse square law. Coulomb's data had produced an exponent of 1.91. Heering obtained exponents that were too low, such as 1.28; then he managed to get results as high as 1.7 but only by using a Faraday cage, a device that had not yet been invented in Coulomb's time. Therefore, Coulomb's results seemed implausible. However, without using any such device, by August 2005 I managed to get multiple consecutive results around 1.9 and 2.0.

Moreover, in one series of measurements, the torsion balance gave an initial separation of 36°, which matched the number published by Coulomb; I therefore took the opportunity to twist the torsion knob at the top of the wire to go from 0° to 126° of torsion, followed by 567°, that is, the very same torsions used by Coulomb in his published data. Surprisingly, the resulting separations between the two pith balls of 36, 19.5, 8.5 were very close to Coulomb's published results: 36, 18, 8.5. These measurements, and others, led me to conclude that Coulomb was right, and that his experimental prescriptions

and published account were accurate. This research project and its findings happened thanks to Jed's experience and mentorship.

Why does this matter? Because for decades the history of science has been one of the various fields in which certain apparently solid findings seemed to erode as some researchers re-conceived them to be "socially constructed." Traditionally, scientific knowledge, especially physics, enjoyed the prestige of possessing a kind of certainty based on reproducible experiments, and not just on opinions. However, some sociologists argued that even famous experiments depended on social conventions. Were the allegedly hard results of physics dependent on sociological forces?

Perhaps Coulomb's famous experiment, which had allegedly served to decisively prove the electrostatic force law, was also merely a play of rhetoric and fudging of data? Perhaps scientists merely negotiated agreements on what they considered laws of physics and only subsequently invented justifications? When I replicated the experiment with Jed's guidance it was as if I were carrying out not just an experiment in physics, but one in history as well: Can a device behave the way that someone said it would behave two centuries later? Certainly, yes, but when it actually does so, contrary to one's expectations, it can have a deep impact. I didn't know whether it would work, and I had read plenty of reasons for why it would not. So to me, it was as if I were digging and digging into the past until the shovel hit something as solid as bedrock. Yet the experimental apparatus was extremely sensitive and delicate. It was as if I could then sense the artisanal engineering and expert knowledge of materials that Coulomb wielded in order to investigate nature itself.

History of physics connects at least two fields: history, where most experiences are unique, local, not repeatable, but open to many interpretations; and physics, where ostensibly knowledge is not merely personal but objective, repeatable, and verifiable. Some theorists in sociology and anthropology of science have used history to undermine the credibility of physics, arguing that certain experiments were artificial, singular, local, and inconclusive. In contradistinction, my work with Jed Buchwald was a most striking experience in showing me the degree to which history can reveal solid knowledge about the past.

Jed's personality and work evince his enormous respect for the labors of scientists who struggled to ascertain reliable knowledge about nature by using experiments, concepts, and mathematics. History of science is fascinating not just because it reconstructs the ideas, agreements, and debates of past scientists, but because it traces the growth of "claims that, though limited in various ways, nevertheless transcended the place and time of their original production."[1] At the same time, Jed cultivated the notion that historians should proceed "as professional agnostics" in their efforts to ascertain how past scientists studied nature, without presupposing what allegedly must have been the case.[2]

My collaboration with Jed Buchwald was an example of the type of great mentorship that can happen even after one has finished graduate school. I warmly thank him for that, and for the other instances in which he kindly helped me, and of course, for the exemplars of extraordinary scholarship that live in his works.

1. *Buchwald and Feingold 2013a*, p. 10.

2. *Buchwald 1994a*, p. 1.

Marius Stan

De magistro

Jed taught me everything that I know to be true in the history of classical physics—the birth and rise of that mirific land *wie es eigentlich gewesen ist*. From him I learned how mechanics came to be and how, with Newton, it exited adolescence. Then he helped me see how the rest of physics made its way into science, with mathematical mechanics showing the way ahead for the many fathers of physics. And, from him I learned how the French, the British, and then Hertz, would reach across fields and domains for any concept, lemma, or heuristic that might help them solve a problem. That taught me to stop thinking of science as a game of stacking blocks higher and higher, as it were, discretely and one at a time. Rather, I should always ask what else those figures knew, what they had at hand, or what else they cared about; also, where they spent their time. Jed taught me to appreciate a certain kind of externalist history, when it is insightful. (By training, we analytic philosophers tend to dismiss material circumstances surrounding the life of ideas.) For instance, he opened my eyes to how important for the development of French optics Berthollet's house at Arcueil, its apparatus, list of guests, and Laplace's outsized presence was.

But that was just his overt lessons. Others things, he taught me without saying much—by doing them; or obliquely; or through his disciples. For me, his most striking exemplum was the *Zodiac of Paris*, which he co-authored with the greatly talented Diane Josefowicz. It keeps teaching me, beautifully, how exact science fares when it ventures *extra muros* and runs into humanities, the clerics, or down the hallways of power. I will never forget their moving portrait of Vivant Denon recounting how he watched a poor crocodile getting captured by fellahin, and what it taught him about Egypt; or al-Jabarti's weary contempt at the French arrogantly trampling over a civilization they expected to greet them as liberators. The *Zodiac* is *historia magistra vitae* at its best. Even better, it is beautiful prose, written for its own sake. I re-read it every three years or so, without failure and always with more delight.

Once, Jed taught me an oblique but lasting lesson. I had just discovered Truesdell's historiographic pieces—stumbled upon them, really, by some *mirabile casu*, deep in the library at Johns Hopkins, his last fiefdom. Truesdell's readings spoke to me forcefully, and I did not mind his harangues, alienating though they were to some. (Really, some of them were not unearned.) But, I did not know much about method and evidence in historiography, and many reviled Truesdell vociferously in those years; so I was understandably confused. Then in a brief but very insightful piece, Jed taught us to see past the old man's dyspeptic asides, and absorb the deep truths that he first, foremost mathematician as he was, could see in the past masters he loved.

Lastly, there is an area—Enlightenment dynamics, my constant infatuation—where Jed found a way to teach me retroactively, by some final causation, as it were. He mentored and guided Craig Fraser, his first graduate student, through the maze of constrained-systems mechanics in the 1700s: a steep, alien land, unfriendly to all but the most rugged explorer. Craig crossed it, and the things he saw are now my Ariadne's thread, unspooled from a yarn that began with Jed's early years at Toronto. I spend my days trying to deserve their gift.

It is not for me to discourse on the state of the field and its pathologies these days. I would rather help make things better or whole again, if I can. And yet, I write this homage in a crepuscular mood of sorts. Jed's generation, with him at the helm, had scaled the tallest peaks of scholarship and thus seen farther, or deeper, than everyone before them. As their eminence, he showed us best how internalist history (of the kind that made Truesdell beam with joy) can absorb the better tools and facts of externalist studies, all for the sake of better understanding our scientific past. His work had lessons for us philosophy folks too; it shook us from dogmatic torpor, and it taught us—better than Kuhn had—to leave our mental hamlets, and venture out to learn how physics really came to be. Jed's age was the high noon of history and philosophy of science; but, because of fashions and the choices of incurious men, we are now in its twilight, and as I look back upon Jed's oeuvre I must wonder wistfully what might have been.

Still, there is always the man. Jed and Diana believed in me when they had little to go by, and they nurtured my early years of apprenticeship with a mix of noblesse oblige and human kindness that I have not seen much elsewhere. I owe them the greatest opportunity I had, as a young scholar; and also my best glimpse at the true life of the mind.

Kristine Haugen

The Story of Cadmus

AT CALTECH, Jed Buchwald has pursued a new and brilliant research career, exploring the most difficult mysteries of the ancient Mediterranean, their recovery in later intellectual worlds, and the passionate debates that followed. In *The Zodiac of Paris* with Diane Greco Josefowicz, about the famous Egyptian astronomical clock brought to Paris in 1821 (*2010a*); in *Newton and the Origin of Civilization* with Moti Feingold, about the great scientist's preoccupation with ancient history and religion as revealed in thousands of manuscript pages (*2013a*); and in his forthcoming book about Champollion's decipherment of the Egyptian hieroglyphs, also with Diane Greco Josefowicz, Jed has graphically demonstrated why the history of science and the history of humanities are so powerful when rigorously deployed together.

My tribute in this volume is also a penetrating investigation of antiquity. Specifically, I will interpret the story of Cadmus, the legendary king and wise man whose supreme contribution to the history of culture was to introduce the letters into Greece. Jed has pointed out that Isaac Newton had arresting opinions about Cadmus, which I will mention in due course. But—and here is the special relevance to Jed's birthday celebration—Cadmus's preeminence reached even beyond his formidable technical expertise. He was profusely cosmopolitan and drank deeply of the wisdom of the Near East. He was a patron of communities, founding a city, defeating a dragon

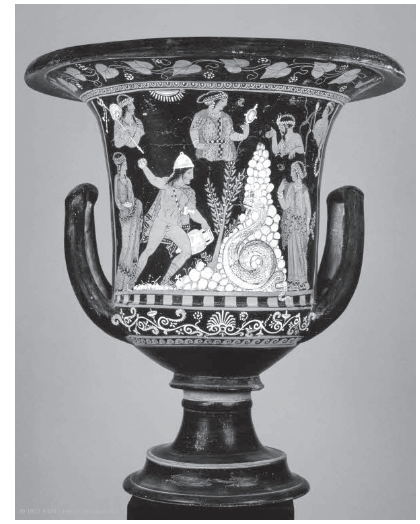


Figure 1. Cadmus brings water to Thebes by defeating a dragon, whose teeth will breed a new race of historians of science. Louvre, Collection Durand 1825.

(Figure 1), and inspiring the rise of a new race of men. Cadmus also became the grandfather of Dionysus, or Bacchus, fittingly for a commemorative conference. And in the young city of Thebes, where he spent the pinnacle of his career, he did all of this with his wife, the brilliant and wise Harmonia, at his side.

Cadmus's career will show us that the sciences and the humanities have been intimately connected from the beginning, as they remain today. Looking toward the future, we will be reminded that advances in the history of science often arise from a deep knowledge of the science of the past. Those advances come most easily to minds voraciously driven to master an encyclopedic range of disciplines and wide-ranging cultures and periods. Lastly, those advances depend on the visionary and patient building of institutions and publications, so that historians of science themselves can be multiplied like the army of men who sprang up when Cadmus planted rocks in the ground.

First of all, a word about methodology. When Isaac Newton set out to rewrite and quantify the entire interlocking history of all ancient civilizations—he made his task much easier by simply ignoring all predecessors in the field of early modern chronology—he relied not only on revolutionary forms of reckoning and calculation, as Jed has shown, but also on a bold approach to collecting data, or rather discovering data. For Newton, the myths conveyed by ancient poetry were grotesquely false, not to mention polluted by repugnant pagan superstitions. Only keen minds like his own could rip away the veil of poetic fiction to elucidate the historical truths hidden within. So for Newton, no god had ever really walked the earth. Much less had anyone ever visited the underworld and come back to tell the story. But by discovering inglorious mere mortals beneath these pagan enormities, Newton could create discrete individuals to stick wherever he liked on his vast ancient timeline. By the way, in Newton's gleeful hostility to vulgar religious error we might, indeed, recognize a friend.

Our investigation of Cadmus must proceed on diametrically opposite lines. Rather than diminishing our extraordinary subject, it is fitting to magnify him and praise his personality and achievements, like the great poets of antiquity. Rather than assailing received traditions, we should celebrate and expand them, like the erudite scholars of the Renaissance. Finally, rather than rejecting the culture and knowledge of the ancient Babylonians, Phoenicians, and Greeks, let us recognize them as our ancestors in science, scholarship, and ambition of every kind.

And it is to the Greeks that we must turn to learn the course of Cadmus's life, or at any rate for a panoply of stories to sift and decode. In antiquity as now, from a great tradition of scholarship can arise the most surprising conjectures; and so we are told that Cadmus was born in Egypt, Phoenicia, Thessaly, and even the mainland of Greece. I propose a bold solution suggested by our special knowledge of Jed: Cadmus was born in Babylon, the ancient world's most advanced civilization, above all in mathematics and astronomy. "Compared to them," Otto Neugebauer said, "the Greeks were children."

He lived in a propitious age, as I learn from the Greek chronologer Eusebius: late in the lifetime of Moses and also during the lifetime of Tat, son of the legendary Egyptian wise man Hermes Trismegistus. In other words, Cadmus lived in the 15th century B.C.E, more than 300 years before the Trojan War, when the Near East teemed with profound erudition and powerful political leadership.

Cadmus's upbringing in Babylon was charmed. His parents were people of elegance and substance who owned beautiful estates near the intersection of the four rivers of Paradise. They summered in Italy, at that time populated largely by wolves and, in the south, by rustic inhabitants who ran a vibrant trade in bootleg truffles. Cadmus attended an all-boys cuneiform school, where he shone: he memorized the characters in record time and even developed a method of conjoining wet tablets into 3-dimensional polyhedra, which made them hard to fit in a messenger's pocket. To Cadmus's great disgust, the cuneiform school also imposed mandatory chapel, and it was here that he conceived his lifelong distaste for the polytheistic gods. Decades later he was still unmasking this fraud to undergraduate students in Thebes, to the glee of most and the consternation of a benighted few. On the bright side, he developed inordinate skill at video games and regularly reached the highest levels in *Call of Duty: Garden of Eden* and *Assassin's Creed: Nebuchadnezzar*.

By a very certain conjecture, I propose that Cadmus also attended university in Babylon. This will have been in a slightly different neighborhood where the inhabitants ate lobster rolls, talked with an accent, and later fostered a cradle of democracy. Cadmus anticipated this spirit when he threw himself into the customary study of matter and motion—that is, mathematics and astronomy—while also taking two absolutely independent approaches. First, Cadmus eagerly studied a complex phenomenon that would later fascinate the Greeks but that the Babylonians utterly neglected: the magnet. Meanwhile, as to the required curriculum in astrological prediction, Cadmus scorned the customary questions about crop yields and the fate of kings, instead devoting himself exclusively to the future of science and technology. Could a method be found for reproducing writing, dozens of copies or thousands of copies at one time? Could humans fly into space? Could a short nap in the afternoon actually increase productivity by a significant amount?

By now Cadmus was equipped both with formidable intellectual expertise and with the inexorable drive and originality that would mark his entire career. But now that the historical record picks up again, there is again too much evidence rather than too little. At the height of Cadmus's career, all sources agree, he will found the city of Thebes, near Athens in Greece, the setting of so many myths concerning his descendants and indeed of many Greek tragedies. But what were Cadmus's exploits in the meantime? Eusebius says that first he became a king in Phoenicia. On the other hand, the handy guidebook of Greek mythology by Apollodorus says that Cadmus spent time in Thrace, which sounds much like a hardship post. This time I will exploit an ancient scholarly method and say that Cadmus did both (Figure 2). Accepting all the sources was once a viable, even favorite maneuver, which is why one often reads, for example, of the lifetimes of eight men named Homer.

Our stance of ancient credulity leads, first, to Cadmus's famous letters. Evidently he now traveled from Babylon to Syria or Phoenicia, a country renowned for its commerce, its wealth, and of course its writing. By a manifest conjecture, on Cadmus's journey he stopped in the ancient city of Palmyra and deciphered its famous inscriptions, which are (or, very regrettably, were) in a script unique to that city (Figure 3).

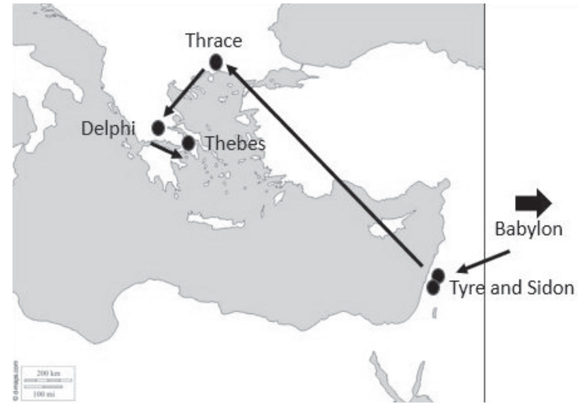


Figure 2. The travels of Cadmus, reconstruction from ancient sources based on reliable knowledge of Jed Buchwald's career.

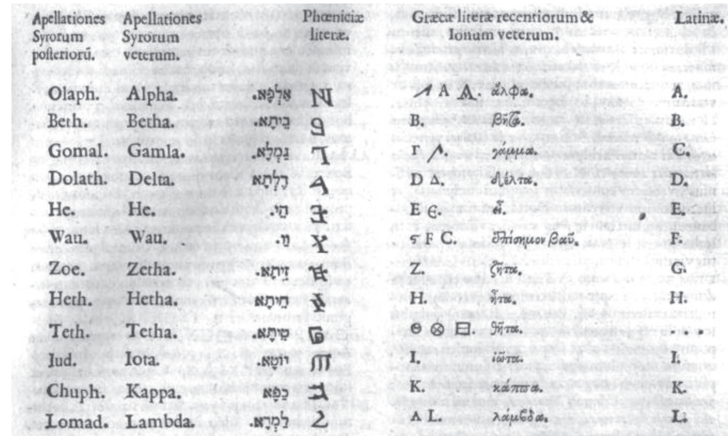
Figure 3. Inscriptions from the ancient city of Palmyra, from Robert Wood, *The Ruins of Palmyra* (1753). Cadmus, having prepared thoroughly for his triumphant takeover of Phoenicia, was expert in this local dialect of Aramaic.

Much later, these same inscriptions would fascinate Western scholars like the astronomer Edmund Halley, who published on them in the *Transactions* of the Royal Society of London. Many believe today that the Palmyrene script was invented only 1,300 years after Cadmus's lifetime, but here in particular the truth cries out to be restored.

Once in Phoenicia, according to Eusebius, Cadmus was immediately crowned king of the twin cities of Tyre and Sidon. These cities were distinguished for their notably cold climate, their civilized and tolerant culture, and of course their great university. As King Ptolemy would do in Alexandria more than a millennium later, Cadmus used his power to carry out scientific research and promote it on an institutional scale. (Today scholars would deny that the ancient Ptolemy who studied astronomy and geography was in fact an Egyptian king; but Ptolemy was indeed a king in the eyes of the Middle Ages and the Renaissance, so one should allow Cadmus this honorable peer.)

Cadmus now published his study of the magnet, and he proceeded to a truly universal subject of research: sunlight. Years later, Cadmus would return to this triumph when he and his wife Harmonia constructed a prism in their backyard out of a glass vessel and water. (The Thebans invented glass for the purpose.) And Cadmus used the influence and resources of his throne to bring together a court of brilliant historians of science and to educate a generation of admiring young Phoenicians.

It was also from this time that Cadmus's fantastic cultural gift to Greece and, indeed, to the entire West originated. Auguring his future as a master of scientific communication, he had grasped that the Phoenician alphabet would one day grow into a literally universal medium. As Joseph Scaliger definitively showed in 1600, the letters that Cadmus brought to Greece were Phoenician, descended from Hebrew, in turn giving rise directly to an old Ionic Greek alphabet that only used 19 letters, the customary ancient Greek alphabet of 24 letters, and from there to the Roman alphabet (Figure 4). This, then, was the golden thread with which Cadmus bound together the past, present, and future of the history of science,



Apellationes Syrorum posteriorũ.	Apellationes Syrorum veterum.		Phœniciæ literæ.	Græcæ literæ recentiorum & Ionum veterum.		Latinæ.
Olaph.	Alpha.	אלפא.		A A.	ἄλφα.	A.
Beth.	Betha.	ביתא.		B.	βῆτα.	B.
Gomal.	Gamla.	גמלא.		Γ Λ.	γάμμα.	C.
Dolath.	Delta.	דלתא		D Δ.	δέλτα.	D.
He.	He.	הי.		E Є.	εἶ.	E.
Wau.	Wau.	וי.		ϛ F C.	ἐπίσημον βαῦ.	F.
Zoe.	Zetha.	זיתא.		Z.	ζῆτα.	G.
Heth.	Hetha.	חיתא		H.	ἦτα.	H.
Teth.	Tetha.	טיתא.		Θ ⊗ ⊟.	θῆτα.	. . .
Iud.	Iota.	יוטא.		I.	ἰῶτα.	I.
Chuph.	Kappa.	כפא		K.	κάππα.	K.
Lomad.	Lambda.	למדא.		Λ L.	λάμβδα.	L.

Figure 4. Joseph Scaliger's demonstration that Cadmus's Phoenician alphabet was the missing link between the first language, Hebrew, and the Greek and Roman alphabets of Europe, from *Thesaurus temporum* (1600).

from the music of King David to the navigations of the Phoenicians to the atomism of the Greeks to the astronomy of Copernicus, Kepler, and Galileo.

Concatenating our authorities, we learn from Apollodorus that Cadmus, bearing the letters, first traveled to Thrace, now a region covering parts of Greece, Bulgaria, and Turkey. His choice might seem unaccountable if we did not know the special details that I have recovered from the history of science. In the annals of cultural history, Thrace has been wrongly obscure because the Greeks, then and now, held such an exclusive bias in favor of poetry and art. In fact, Thrace in the Bronze Age was extraordinarily distinguished for engineering and the study of the natural world; it bore some resemblance to the university of Babylon, but let us avoid confusion. Working in a collaborative institute, the Thracians developed robust new technologies for projectiles, chariots, sailing ships, palaces, and helmets with waving horsehair crests. Nor were they without things of beauty: the Thracians produced statues and red-figure vases depicting the creation of the world, the loves of the gods (which Cadmus interpreted allegorically), and the foundation of cities. In other words, the researchers of Thrace were driven by intellectual curiosity and craved rational government, and all that was needed to consolidate and extend their success was an alphabet.

That alphabet, of course, was what Cadmus brought from Phoenicia. In the hundreds of mountain villages of Thrace it began to spread: love letters, credit card statements, evangelical tracts, and political graffiti all burst into view where before neighbors had only whispered to each other (or shouted). But the most prescient people in Thrace were the scholars of what we might call the Croesus Institute for the History of Science, who saw how Cadmus could transform their entire field of study, not only by this amazing new information technology, but by the force of his personality. Instantly he accepted the directorship, and his first act was to have the Institute's library converted in its entirety out of the crude pictograms that had heretofore preserved historical knowledge. (They were not easy to decode, even for Cadmus.)

And Cadmus expanded his reach even more dramatically through a conceptual breakthrough he called publication. When Cadmus or one of his scholars made a discovery, he not only wrote it down to be kept in the Thracian archive. Scribes made dozens and hundreds of copies to be transported into the Greek peninsula, even into districts where Cadmus's letters were very new indeed. Many a teenager learned to read for the first time on a copy of *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves* (1,430 B.C.E.).

Finally, even where the new technology of publication was concerned, Cadmus made a special innovation that might otherwise have waited centuries. Intensively, tirelessly, he organized and edited his colleagues' work, making it clearer, more orderly, more interesting, and altogether more worthy of their heroic research. And he formalized that activity into an astonishing number of publication series, giving those colleagues access to the right scribes and the right reading public. Many of these same colleagues, of course, are joining in the celebration of Jed's career today. In recognition of all this and more, Cadmus received the rare and prestigious MacAristotle Fellowship, named for the encyclopedic polymath and voluminous author who set Europe's intellectual agenda for more than a millennium, to recognize extraordinary achievement and extraordinary promise in ancient intellectual life.

Here one must pause to refute Isaac Newton's habitual pessimism, which extended with a special vengeance to Cadmus. Not only did Newton suspect that Cadmus's "letters" were not what they seemed to be; more seriously, he insisted that all of this could never have been done by one man. As Jed has shown, Newton ultimately sought to pinpoint the origin of all human civilization, which he did by taking Cadmus's "letters" only as a figure of speech. In reality, Newton thought, the ancient chronological reports about Cadmus's "letters" really testified to the spread of the whole sphere of human learning across the entire Mediterranean, from navigation, astronomy, metallurgy, and an elegant lunisolar calendar to music and poetry. In turn, faced with the magnitude of this new task, Newton created an entire teeming crowd of Cadmuses who had fanned out from Phoenicia to revolutionize the world. In his favorite hermeneutic maneuver, Newton had cut off Cadmus himself at the knees.

In fairness, we as Jed's contemporaries have irrefutable information that Newton lacked. Cadmus really did carry the world before him, landing in place after place with a brilliant research program of his own and irresistible methods for promoting knowledge for an entire field. He acted, then, as director of the

Croesus Institute for nine years, where he reigned with such wisdom, energy, and care for his subjects as to place him among the most famous rulers of the ancient world. Like Lycurgus of Sparta, he inspired Thrace's historians of science to do their best work, and do it collaboratively, through a multimedia assault. Soft but insistent music, carefully chosen according to principles of mathematics, kept them on the battlefield day after day. The examples of great predecessors were constantly discussed, along with their striking sayings. And like Lycurgus, Cadmus had his historians of science spend lots of time together, constantly reminding them what they had in common and showing them their common goal.

But unlike Lycurgus, who was also the ancient world's most gloomy isolationist, Cadmus worked ingeniously to reinforce and expand the position of the Croesus Institute. He hired new colleagues as surely as Jason picked the Argonauts, for their brilliance, perseverance, and lust for the unknown. Like Agamemnon, he formed alliances so powerful and so numerous as to launch a navy of a thousand ships. And Cadmus ingeniously defeated bureaucratic intrigues, like Bellerophon, who was ordered by a king to fight the chimera (this was intended as a death sentence), but instead mounted his flying horse Pegasus and shot the monster with arrows from the air.

Yet Cadmus's mission to spread learning and transform humanity was not finished. He had heard of an alluring land to the south, called Greece, where his Phoenician letters had already spread and created an effulgence of research into the cosmos, the human body, and of course the history of humanity. Stubbornly, humanists like Homer refused to adopt the new technology, and when they sang their poetry aloud, they tended never to say the same thing twice. Be that as it may, Cadmus knew of Greece's white beaches, elegant mountain houses with majestic views, and olive trees of improbable size.

Most importantly of all, even in Thrace the fame of a wise woman named Harmonia had reached him, and he was captivated by a desire to see her and learn from her. Cadmus did not yet know how to find Harmonia, but he did know that somewhere, in a grove of extravagantly flowering trees, she had established a research institute on the life and writings of Heraclitus, the extraordinary philosopher who propounded a surprising theory of matter and motion and was also given to dropping mysterious sayings. So Cadmus did what any reasonable person in antiquity would do: he proceeded to the oracle of Apollo at Delphi.

In later times the Delphic oracle became bloodthirsty, even malevolent. It famously advised Agamemnon to sacrifice his daughter, instructed Orestes to go to Athens to stand trial after murdering his mother, and deceptively told the king of Lydia that his military campaign would destroy a great empire, which turned out to be his own. But in this kinder, gentler age, 250 years earlier, the Delphic oracle chose to welcome strangers, advance civilization, and promote love between equals.

The oracle instructed Cadmus to follow a cow that was conveniently hanging around nearby, to follow the cow wherever it went, and to found a new city wherever the cow lay down. Now the terrain around Delphi is very difficult, with mountains, rocks, and nice ladies walking the roads with wicker baskets of chickens, whereas the city of Thebes, which Cadmus was about to found, is 56 miles away. It is clear, then, that both the oracular cow and Cadmus had startling powers if they never lay down before reaching their destination. Newton-like, I conclude that Apollo gave Cadmus a spectacular and shiny new GPS to add to the collection of gadgets that he had already collected around the Mediterranean. The ingenious Thracians had already contributed several devices invented just for him, such as the garage door opener, the Apple TV, and the potato chip.

So Cadmus arrived in the place that his descendants would later name Thebes. It was a very young city, 400 years younger than Crete, 300 years younger than Rhodes, 100 years younger than Athens. The country was full of promise, and Cadmus found here a teeming assortment of agile minds. Yet, he sensed, a masterly hand might achieve even more.

Cadmus's first act was to visit the eminent Harmonia, whose penetrating knowledge of science and history was so much like his own, and who equaled him in courage, grace, and understanding. The affinity of their personalities was no less than he had hoped. But at Harmonia's institute for the study of Heraclitus, Cadmus also noticed two things highly relevant to the organization of the history of science. First, Harmonia had brought together scholars from all over the world and given them a common purpose, just as Cadmus himself had done in Thrace. And Harmonia had done this by amassing a vast collection of documents, exploiting the new technology of writing, and making collaborative publication the ultimate aim of the project. Love and emulation now fired Cadmus's heart.

The answer came to him, as it so often did in the ancient world, in a prophetic dream. One night Cadmus saw Athena filling a wooden chest with scrolls, then giving it wings so that it could fly to Thebes over islands, mountains, and plains. Cadmus racked his brain, but he was sure he had never seen anything like this in the video game called *Call of Duty: Olympus*, so he concluded it must be a sign. He saw instantly that rather than slowly increasing Thebes's library scroll by scroll, he could possess himself of entire existing libraries by conquest or engulfment. First, possibly with the aid of the gods, the library of the Croesus Institute in Thrace suddenly

appeared in Thebes, bringing great acclaim for it and for Cadmus; the Croesus Library also came with annual deposits of grain and olive oil to lure visiting scholars to mine its rich treasures. Second, because Cadmus was adept at making friends and never stopped gathering information, he had learned of a curious private library in the neighborhood founded for the sole purpose of proving that Achilles was the real author of the poems of Homer. Exceedingly curious books had been amassed to this end, and soon enough, these books also found themselves in Thebes. At this period in his career, even more tenaciously than before, good things stuck to Cadmus's fingers.

Cadmus worked feverishly on his publication series in Thebes and even founded one more. And he himself wrote on daring new subjects, predicting future interpretations and controversies over the science and scholarship of his own time: the religious debates inspired by the Zodiac of Paris, the blatant assault on ancient chronology launched by Isaac Newton, and Champollion's triumph in deciphering the hieroglyphs. It also looks as if Cadmus and Harmonia may have successfully reconstructed more early modern experiments in their backyard than Descartes's enigmatic optical prism. A 17th century decorative tapestry from England suggests that they either sacrificed to the pagan gods, apparently using a supercombustible material developed by ingenious Thebans, or penetrated the mysteries of early modern chemistry (Figure 5).

But as ever, the myth instructs us to concentrate on Cadmus as a founder of communities. Following a typically Californian dispute over water rights, which I'll pass over, as well as a celebrated fight with a dragon, which predictably he won (Figure 1), Cadmus was told by Athena to sow the vanquished dragon's teeth in the ground. From these teeth, of course, there grew a new race who came to populate Cadmus's

Figure 5. Wool and silk tapestry depicting Cadmus and Harmonia at a large pyre, circa 1670–1690, Chirk Castle, Wrexham, Wales. Laboratory assistants stand by, and a goddess who may be Frances Arnold watches from the upper left.

city. Some of these new citizens Cadmus had known for decades, like Noel Swerdlow, Nicolás Wey-Gómez, and Moti Feingold; others, like me, were attracted by the excitement of the new letters and the opportunity to learn. Together with the friends Jed has cultivated in all fields at Caltech, not only in the humanities but across the Institute, all of these loyal subjects are a tribute to the extraordinary breadth of his intellect and curiosity, his understanding of people and institutions, and his constant spirit of collaboration.

The authors of myth treat the love of Cadmus and Harmonia last, which is certainly wrong as chronology but right as the culmination to a story. Theirs was one of the great weddings of ancient legend, like that of the parents of Achilles: all the gods descended from the sky for a marriage feast and sang hymns in a place where a temple to Cadmus was later built. We're also told that Cadmus gave Harmonia a robe and a necklace forged by Hephaestus. This may well be true, but I believe Cadmus also took Harmonia several times

to Italy, Germany, and France, as well as Egypt. This extraordinary couple also inspired the bestselling novel by the Italian author Roberto Calasso titled *The Marriage of Cadmus and Harmony*. As beautiful, and as carefully researched, as Ovid's *Metamorphoses*, this is a rhapsody of the loves of the gods and heroes that delights both old and new readers of these sublimely intertwining myths.

The history of Cadmus's and Harmonia's family is long and illustrious, but I will point out only that they were the grandparents of Dionysus, the son of Zeus, renowned for discovering the grape vine. It is an astonishing fact that Cadmus apparently achieved everything in his entire career while drinking only beer. So the gifts of Cadmus and Harmonia to future generations were a deep desire for knowledge, a fearless independence, an irresistible urge to build communities, and an enduring love for family, friends, and the health of society. There could be no more fitting legacy for a 70th birthday.

Mark J. Schiefsky

De cameris non liquet

Of all the colleagues I know, Jed has cost me the most money. It's all about the cameras of course. It seems like every time I visit Pasadena there is always a new camera, and—even better—it's always the "best camera ever" (until the next visit). I used to be pretty skeptical of this approach. I had one camera (actually two) that did everything I needed, so why switch? But the siren call of Sony was too strong, and I made my first eBay camera purchase in the spring of 2015. It's been all downhill from there. I'm proud to announce that there are a couple (still current) models that I got my hands on before Jed did. I won't say which ones. But I still have a lot to learn from him: namely, that selling is just as important as buying when it comes to eBay. At least I haven't bought anything from Leica (yet). As for the question "how many cameras do we own, dear" which my wife Mary sometimes asks me, I can only reply with the answer the ancient skeptics gave when they were asked whether the number of the stars is odd or even: *non liquet*.

One time Mary asked Jed for suggestions about what to get for my birthday. Maybe she could pick up a lens or two? As she tells the story, he explained that the cost of modern optics was a bit higher than she probably had in mind, so I got a camera bag instead (a very nice one in fact). This just goes to prove the truth of the old saying "What happens at Samy's Camera stays at Samy's Camera."

I have many fond memories of long photography walks with Jed, also a regular occurrence during my visits to Pasadena. Eaton Canyon, Chaney Trail, Devil's Gate: each one different and beautiful in its own way. We would even sometimes talk about subjects other than cameras, like the history of science. But somehow or other it always came back to image stabilization, f/ratios, and the hyperfocal distance. Forget Newton and hieroglyphics: what the world clearly needs is a set of YouTube videos in which we put the latest equipment to the torture test.

Mary and I are so grateful to Jed and Diana for their hospitality, for welcoming us to Pasadena, and for making it an intellectual home for us.

Jeremy Schneider

The One with the Beard

I remember first meeting Jed in the summer of 2016. I was waiting outside of Chandler, the cafeteria at Caltech. "I'm the one with the beard" was the last thing he had written to me via email.

Then he arrived. Jed—with his intense eyes and distinctive New York twang—can be intimidating. But that evaporated quickly. His warm encouragement and constant moral support guided my first steps as an aspiring scholar. He has been there ever since. Jed was a vital presence as I applied to graduate school. He watched me as I gave my first-ever talk. And he was the first mentor to hand me a book with a written dedication, as I left Caltech for the pastures of New Jersey. It is one of those small rites that we have in the academic world that seem to mean little, but in fact mean a lot. "Work hard in Princeton!" he wrote on the flyleaf. It was the 1,000-page *Oxford Handbook of the History of Physics*.

Above all, Jed taught me to develop my own voice. To not let pre-existing debates dictate all the terms. It is, in other words, the call to find something new and interesting within the historical sources.

Jed's startling publication record excels in these qualities. Two small examples must suffice. In "Descartes' Experimental Journey" (*Buchwald 2007b*), Jed unearths a completely new side of the French philosopher. In reconstructing Cartesian optical experiments, Jed shows us that Descartes was not just

a scholar in a study, but an avid experimentalist—one who philosophized about invisible entities only when experiments took him no further. In fact, in the course of researching the paper, Jed himself seems to turn into a sort of Californian Descartes: hanging water-filled spheres from the balcony of his Altadena home and fiddling around with prisms in the living room. Then, there is Jed's beautiful paper "Discrepant Measurements" (*Buchwald 2007a*). Aside from the remarkable discovery that Newton averaged out the values within large sets of data, Jed unexpectedly shows us that the telescope of a Robert Hooke was no more accurate than the eye of a Johannes Hevelius. Jed takes his history seriously: will the machine-eye replace the human? As academia moves to digital methods, Jed makes his case not for the abstraction of the algorithm, but "the scholar's seeing eye."

Jed Zachary Buchwald is an unforgettable presence in the history of science. With an eye for technical details and a vision for conceptual shifts, he has illuminated past science in both theory and practice. I am deeply indebted to him: for his insights and his inspiration.

Jesse H. Ausubel

Microphysics and Macrohistory

The microphysics of butterflies causes history. So does the leadership of great women. Others attribute history-making to the deadly sins catalogued by Thomas Aquinas in the 13th century—wrath, greed, pride, envy, lust, gluttony, and sloth. Christopher Marlowe wrote unforgettably in 1594 that Helen's face launched a thousand ships. Others say that the cardinal virtues, mainly justice and courage, are the prime movers.

No matter what we choose as our prime human historical mover, we have to understand that strong existential limitations greatly reduce the freedom of strategists, whether farmers, scientists, or generals, whether a family, corporation, or nation. In a century so far glorifying the power of human decisions, let us not forget fate.

Let me begin with a doctrine from American history known as Manifest Destiny. The term, first used in 1845 by a journalist, referred to the inevitably continuing westward territorial expansion of the United States through conquest and purchase—or, I would say, diffusion. Maps which all American students saw on classroom walls showed the major spatial changes, encompassing the transition from colonial settlements with foot paths into the forests to a nation integrated by transcontinental railways, interstate highways, gas pipelines, and electricity grids (figure 1).

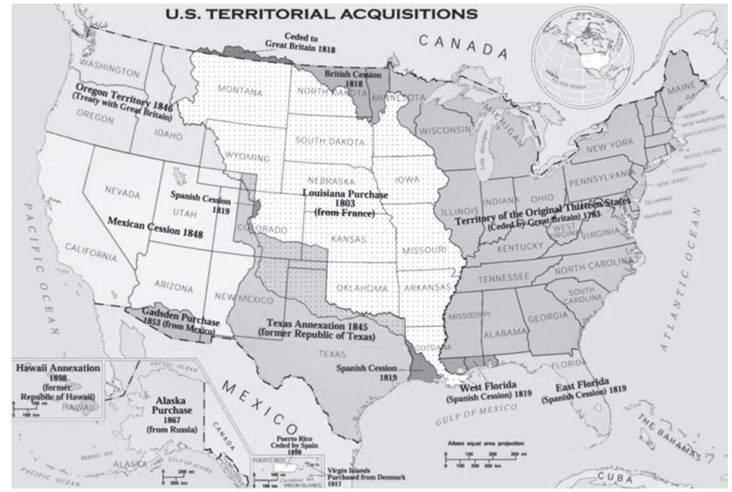


Figure 1. National Atlas map (circa 2005) depicting U.S. territorial acquisitions. Source: National Atlas of the United States, Department of the Interior.

In a study of the quantitative history of twenty human empires, Cesare Marchetti and I plotted the areal growth of the USA as analogous to the growth in height of a sunflower (figure 2).[1] The fit is beautiful, over 250 years—through wars, depressions, epidemics, and other disturbances.

So no matter what the results of their agency and individual actions may have been, Thomas Jefferson and Lewis & Clark and Sacajawea and so on were also actors in a play. Most people, whether generals or bandits, like to believe they are decision makers, not the blind executors of a blind but all-powerful fate. Greek mythology helps us to understand the problem with this kind of thinking. Because although all gods reported to Zeus, *tuchē*, or fate—abstract, invisible, and

1. C. Marchetti, J. H. Ausubel. Quantitative Dynamics of Human Empires. Adapted from Marchetti and Ausubel, *International Journal of Anthropology* 27(1-2):1–62, 2012. 2013.

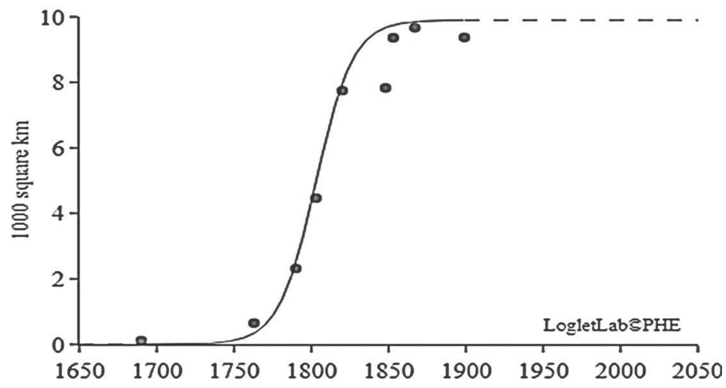


Figure 2. Spatial trajectory of the Roman Empire. Source: Marchetti and Ausubel, 2013, p. 13.

all-pervasive—ran the system, Zeus included. Americans—and scholars everywhere—still have much to learn from the ancient Greeks.

Let me introduce a general concept about how systems grow and evolve. Systems grow by substitution, by mutation and selection. Evolution is a series of replacements. An innovation, a mutation, enters the picture and if it is fitter for the task, it gains a growing and often obliviating share of its ecological niche or market. Often the substitution process follows an s-shaped curve, both in taking over a niche and in subsequently losing it.[2] A familiar example is recording media, where tapes overtook long-playing records, and in turn CDs replaced tapes, and MP3s and systems of downloading and streaming have now overtaken CDs (figure 3). In addition, the superior competitor often spurs system usage to grow.

2. P. S. Meyer, J. W. Yung, J. H. Ausubel. "A primer on logistic growth and substitution: The mathematics of the Loglet Lab software." *Technological Forecasting and Social Change* 61(3): 247–271, 1999.

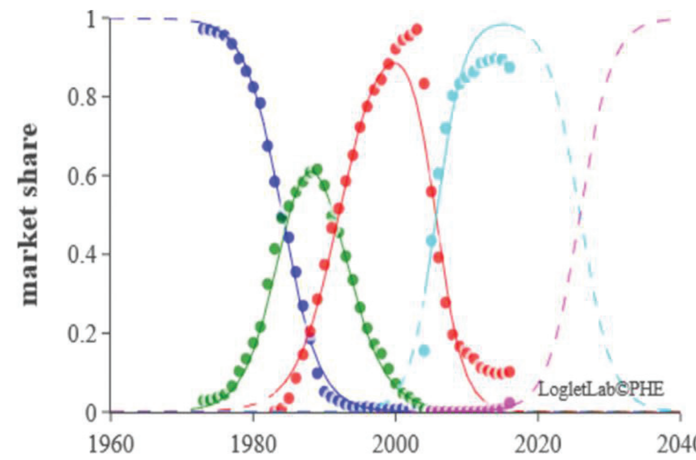


Figure 3. Substitution of recording media in the US market. Media are records or vinyl (dark blue), cassettes or tape (green), CDs (red), downloads (light blue), and paid subscriptions or streaming (purple). Plotted by Perrin Meyer and David Burg. Data available at https://logletlab.com/?page=index&preload=library.get.1

Consider a substitution process from the said-to-be free-for-all world of high technology and venture capital. Eight generations of sales of Dynamic Random Access Memory (DRAM) chips increase in Prussianesque order from 1973 to 2000 (figure 4).[3]

Another—in this case grim but elegant example of substitution—comes from the causes listed on death certificates. Think of causes of death such as heart attacks, cancer, and infections as competitors for corpses, a market that we all seek to shrink. Charts we plotted twenty years ago found an orderly evolution in America during the seemingly disorderly 20th century and thus

3. N. M. Victor, J. H. Ausubel. "DRAMs as a model organism for study for technological evolution." *Technological Forecasting and Social Change* 69(3): 243–262, 2002.

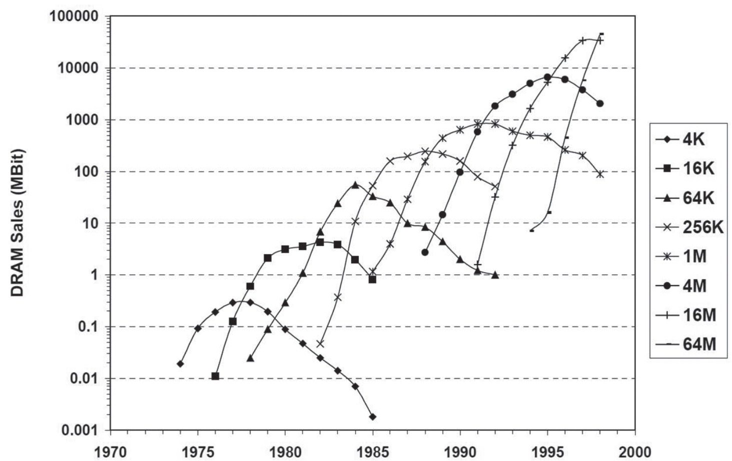


Figure 4. Logistic substitution of sales of Dynamic Random Access Memory chips. *Victor and Ausubel 2002.*

allowed us to predict that cancer would become the number one cause of death by about 2020 (figure 5).[4] America is fulfilling this destiny, too. Only the fittest causes of death survive.

These four examples span the Louisiana Purchase, presidents and generals, turnpikes, railroads, and telegraphs; corporations such as RCA and KLH, phonographs, magnetic tape, and optical disks; personalities from Thomas Edison to Steve Jobs; inventions and patents at the IBM Watson Lab and then ferocious competition by Intel and other players in the silicon game, in Taiwan and Japan too; and a great flu pandemic, sewage treatment, vaccines, and hundreds of drugs and millions of surgeries. They embrace countless lawsuits, regulations, mischief, crimes and conspiracies, janitors and billionaires.

4. J. H. Ausubel, P. S. Meyer, I. K. Wernick. "Death and the human environment: The United States in the 20th century." *Technology in Society* 23(2): 131–146, 2001

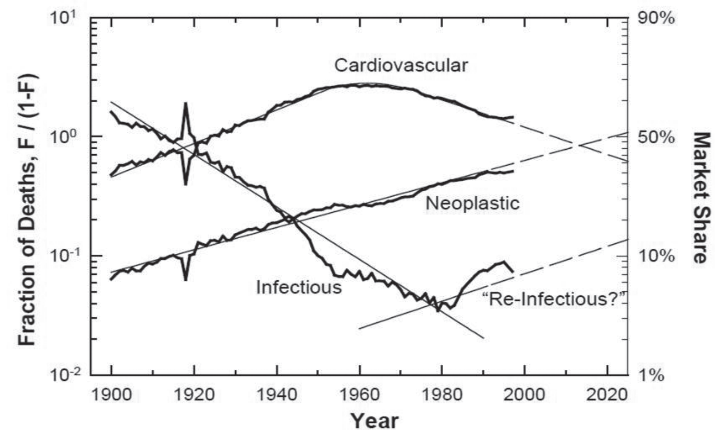


Figure 5. Competition for corpses among major causes of USA deaths during the 20th century, plotted on semi-log scale (normalized to one hundred percent of the market). Dashed lines show a fit with the logistic substitution model, including a forecast. *Ausubel, Meyer, and Wernick 2001.*

Let me add one more—environmentally crucial—example from primary energy, where human behavior has managed to defy the script for three decades or so after long, faithful repetition. For about 150 years, until about 1990, the substitution of hydrogen for carbon in the energy system, and from wood and hay, to coal, to oil, to gas, and the resulting decarbonization, beautifully described the ongoing energy transition (figure 6).[5]

The explanation for this long-term pattern is simple. The evolution of the system is driven largely by the increasing spatial density of energy consumption at the level of the end user, that is, the energy consumed per square meter, for example, in a city. As high-rise urbanization lifts spatial density of energy consumption, fuels must conform to what the end user will accept, and constraints become more stringent. Rich, tall,

5. J. H. Ausubel. "Where is energy going?" *The Industrial Physicist* 6(1): 16–19, 2000.

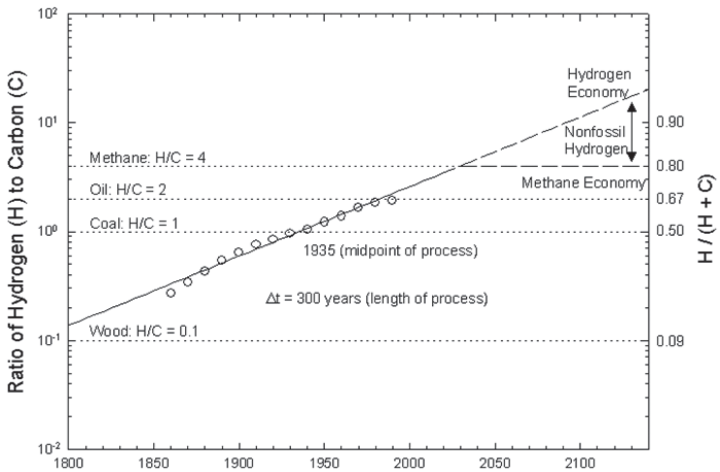


Figure 6. Decarbonization of the global energy system measured as the ratio of hydrogen atoms to hydrogen + carbon atoms in primary energy sources. "Policy' appears to have deferred decarbonization by about a generation. *Ausubel 2017.*

dense cities accept happily only electricity and natural gas, and, incipiently, hydrogen.

About a generation ago, humans managed to stall decarbonization through a series of incredibly contrived energy policies favoring the evolutionarily unfit. Had the energy system not become so self-conscious, it would probably be far closer to its low-carbon destiny today. In the energy system, reflexivity has mobilized interest groups whose interactions have favored the *status quo*. But finally, after many rationalizations, clean supply systems that benefit from economies of scale will produce the lion's share of the electricity and gases we will need. If you might dismiss scale, think of Facebook, Amazon, and Google, or Samsung and Alibaba. In a society of flash trading and flash mobs, perfect power—that is, ultra-reliable electricity—also wins in the Darwinian game.

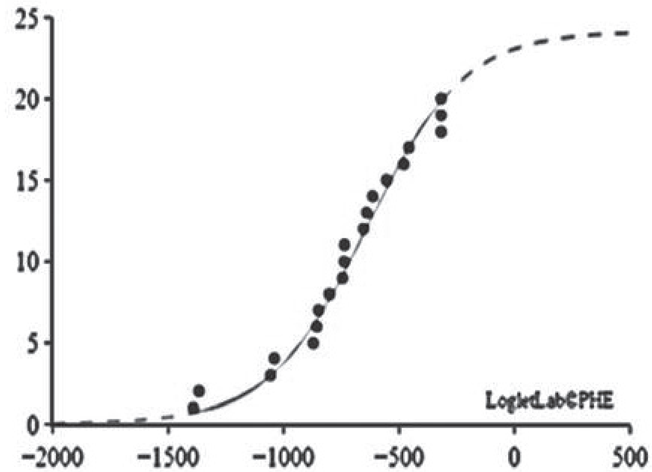


Figure 7. The writing of the Hebrew Bible charted as logistic growth process by the estimated birth dates of the authors. The process took 906 years to go from 10% to 90% completion, should have involved about 24 authors if fully realized, and reached its midpoint about 667 before the Christian era. *Wernick 2016.*

In the case of the USA, the script for energy supply is simply to favor natural gas (with some carbon capture and sequestration), nuclear, and hydrogen.[6] Although few have noticed, USA hydrogen production is climbing nicely. And fuel cells, engines on hydrogen, will greatly increase their market, as wise automakers understand. On the demand side, we naturally seek to raise the rates of efficiency gain, to shrink usage, to decouple energy from GDP and carbon from BTU. A key is to focus on systems and practices with big upsides, such as the share economy which can lift capital utilization, and magnetically levitated trains and other vehicles which carry

6. J. H. Ausubel. "Density: Key to Fake and True News About Energy and Environment." Presented at a meeting of the American Association of Petroleum Geologists, *Next 100 Years of Global Energy Use: Resources, Impacts and Economics*, Houston Convention Center, 4 April 2017. Published in AAPG's *Search and Discovery*, as contribution #70272, 28 June 2017.

neither engine nor fuel and thus weigh far less per kilo of passenger than traditional cars, trains, and planes. We can lessen the jack rabbit excursions around these ultimately inevitable trends often proposed and organized by politicians and stakeholders.

Historians traditionally view their subject as unfolding in an essentially random way, contingent upon the violent, retributive whims of a citizenry and the political machinations of a handful of influential individuals. But history is more accurately seen through a more deterministic lens in which it obeys its own internal logic, unbeknownst to those staffing the think-tanks or Sandinistas.

We feel a freedom of decision inside ourselves whose legitimacy economists and politicians assume as sacred dogma, in the face of the obvious determinism of many global or national outcomes such as Manifest Destiny. The situation fits the famous analogy between the somewhat free and unobservable behavior of single molecules and the beautifully clean relationship of pressure and volume in a gas on a macroscopic scale. The determinism and feeling of liberty may not be contradictory. For example, the system requires the kamikaze behavior of entrepreneurs to evolve. But in the end we all feel the breath of fate. The writing of the Bible is a beautiful S-curve, accomplished by 24 authors over about 900 years (figure 7).[7]

Most of history, including the history of science and technology, is preprogrammed. Don't forget the system. It won't forget you.

Acknowledgements: Thanks to David Burg, Cesare Marchetti, Perrin Meyer, Nadejda Victor, and Iddo Wernick.

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